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THE STOPS AND SPIRANTS OF EARLY GERMANIC

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1. The testimony of the handbooks. The student who turns to our many excellent handbooks for information on the stops and spirants of Proto-Germanic finds that they agree completely in some respects, but disagree just as completely in others. All of them assume for Proto-Germanic a set of voiceless spirants and a set of voiceless stops: the spirants /f þ h/ as in English *fish*, *thin*, *horn*, derived from PIE /p t k/ as in Latin *piscis*, *tenuis*, *cornu*; and the stops /p t k/ as in English *lip*, *two*, *kin*, derived from PIE /b d g/ as in Latin *labium*, *duo*, *genus*. All of them also assume that there were some further sounds derived from PIE /bh dh gh/ and, by Verner's Law, from /p t k/, and that these sounds were voiced; but there is no agreement whatever as to whether these sounds were stops, or spirants, or both.

A minority view is that PIE /bh dh gh/ became the stops [b d g] in Germanic; that these stops then became the spirants [b̥ ð̥ g] in intervocalic position; and that these spirants merged with the [b̥ ð̥ g] that had resulted from the operation of Verner's Law.¹ The majority view is that both /bh dh gh/ and /p t k/ (by Verner's Law) gave the spirants [b̥ ð̥ g] at some stage in Germanic; and that these spirants later became stops in certain environments. But there is no agreement either on the environments or on whether the development to stops should be assigned to Proto-Germanic or to the individual dialects. In a fair sampling of the handbooks we find assumptions as different as the following: (1) spirants in all environments in Proto-Germanic; (2) stops 'im Anlaut, nach Nasalen, usw.'; (3) stops after nasals and in gemination; (4) stops initially (except for [g]) and after nasals; (5) the same, but a dental stop also after /z/; (6) stops initially (except for [g]), after nasals, and in gemination; (7) labial and dental stops initially and after nasals, a dental stop also after /l/ and /z/, but no velar stop in any environment.² In the face of this disagreement, it is not surprising that the authors of a number of handbooks, after pointing out the possibility of stops in certain environments, should solve the problem by simply refusing to commit themselves.³

¹ Thus A. Meillet, *Caractères généraux des langues germaniques* 27-55 ('1949), *Les dialectes indo-européens* 89-90 (1908); Karl Luick, *Historische Grammatik der englischen Sprache* §618, esp. Anm. 3 (1914 ff.).

² (1) R. C. Boer, *Oergermaansch handboek* §143 ('1924). (2) T. E. Karsten, *Die Germanen* 124 (1928). (3) W. Streitberg, *Urgermanische Grammatik* §122 (1896). (4) Karl Brugmann, *Kurze vergleichende Grammatik der indogermanischen Sprachen* §271.7 (1902). In *Grundriss der vgl. Grammatik der idg. Sprachen* §§792, 799 ('1897), he also assumes stops in gemination. (5) Richard Loewe, *Germanische Sprachwissenschaft* 1.70 ('1933). (6) E. Prokosch, *A comparative Germanic grammar* 58, 75 (1939). (7) Friedrich Kluge, *Urgermanisch* 48, 61 ('1913).

³ R. Bethge, *Laut- und Formenlehre der altgermanischen Dialekte* 1.§110 (Ferdinand Dieter, ed.; 1898); W. Wilmanns, *Deutsche Grammatik* 1.§26 ('1911); Hermann Hirt, *Handbuch des Urgermanischen* 1.§55 (1931); Carl Karstien, *Historische deutsche Grammatik* 1.110 (1939).

TABLE 1. THE GOTHIC EVIDENCE

	1. C-	2. CC	3. NCV	4. NC+	5. ICV	6. IC+	7. rCV	8. rC+	9. VCV	10. VC+	11. VCs	12. VCt
/p/	pugg	?	-mampidedun	-tramp	hilpan	-halp	wairpan	-warp	greipan	-graip	hrops	
/t/	fadar	?	hamfamma	fimf	wulfos	wulf	?	parf	-hofun	-hof gaf	? hlaifs	aftra gaft
/b/									giban	↑	↑	
/t/	baurgs	?	lamba	lamb	silba	?	-swairbands	-swarb				
/p/	tuggo	skattans	wintrus	kant	salta	salt	hairto	swart	haitan	haihait	mats	(see 2)
/d/	þank	aipþau	munþa	munþ	gulþa	-gulþ	wairþan	warþ	qiþan	qap -baup	bleiþs staþs	
									-biudip	↑	↑	
/k/	dags	twaddje	landa	land	skulda	skuld	waurda	waurd				
	kaurn	smakkans	drigkan	dragk	skalkos	skalk	waurkeip	?	-aukan	auk	reiks	
/g/										dag	dags	magt
	gasts	triggws	lagga	lagg	balgeis	-balg	baurge	baurg	dagos	↑	↑	↑
/h/	haurn				filhan	-falh	gatarhiþs	pairh	slaha	slah	ahs	maht

The handbooks also disagree on whether the various stops and spirants of Proto-Germanic should be treated as three contrasting series or as four. Where the emphasis is on historical development, [b ð g] and [b d g] are usually grouped together and contrasted with [f þ h] and with [p t k]. But where the purpose is rather to make an inventory of the PGmc. consonants, all four sets are given equal rank. Since all the handbooks are written in terms of phonetics rather than of structure, both treatments can be presented in the same book without producing any apparent contradictions.⁴

Where there are disagreements and inconsistencies such as these, a new attempt to analyze the PGmc. stops and spirants seems justified. In the following paragraphs I shall try to find a solution through the use of structural methods of analysis, and also by a more strict application of the principles of the comparative method. Before this can be done, of course, it will be necessary to analyze the stops and spirants of each of the oldest dialects, and to determine the structural changes which brought about each of these systems.^{4a} Much of what I shall have to say is old and well established. The rest is new and tentative, and perhaps wrong in many places. I offer it in the hope that it will lead to further and better analyses.

2. The Gothic evidence. We begin our investigation by considering the evidence of Gothic, since this is the earliest dialect for which we have adequate records. This evidence is presented in Table 1,⁵ divided into the twelve environments which seem to be significant for our purposes: 1. initially; 2. in gemination; 3. after nasal before vowel; 4. after nasal finally; 5. after /l/ before vowel; 6. after /l/ finally; 7. after /r/ before vowel; 8. after /r/ finally; 9. after vowel before vowel; 10. after vowel finally; 11. after vowel before /s/; and 12. after vowel before /t/. Where there is no form to fill a given environment, a question mark indicates that the lack of such a form is thought to be fortuitous.⁶ At the left are the phonetic values (in square brackets) and the phonemic assignments (between slant lines) which I hope to justify.⁷

⁴ Typical of many is Ernst Kieckers, *Handbuch der vergleichenden gotischen Grammatik* (1928), where four sets are listed on p. 12, but three on pp. 46-7.

^{4a} A brief structural analysis of PGmc. /b d g/ has been written by Bohumil Trnka, *From Germanic to English: A chapter from the historical English phonology*, *Recueil linguistique de Bratislava* 1.139-49 (1948). Since this journal is not easily available in this country, I am grateful to Paul L. Garvin for having called Trnka's article to my attention and for having lent me his copy of it after I had read a preliminary version of this paper at a meeting of the Linguistic Society in December 1951.

⁵ All Gothic forms cited may be found in Ernst Schulze, *Gothisches Glossar* (1848). Where necessary, Schulze's readings have been corrected to agree with those in Wilhelm Streitberg, *Die gotische Bibel*, 1. Teil (1919).

⁶ Loanwords can be cited for the clusters /pt/ and /kt/: *saraipta* 'Sarepta' and *laiktjo* 'lectio'. Comparative evidence suggests that Gothic had the geminates *pp*, *ff*, *bb*, and perhaps *hh*. Only Greek loans can be cited for *pp* and *bb*: *filippus* 'Philip', *sabbato* 'sabbath'. For *ff* and *hh* the forms *affalht* 'didst hide' and *uzuhhof* 'lifted' might be cited; but these were probably /af+fálht/ and /uzuh+hó:f/. (I use the symbol + to indicate the phoneme of open juncture which seems to have existed in Gothic, as in other Germanic languages. Those who do not resort to juncture may read it as 'beginning of a stem after a prefix' or 'beginning (or end) of a word or of part of a compound word'.)

⁷ In a historical study such as this, dealing with written documents and reconstructions, it is obvious that so-called 'phonetic' transcriptions in square brackets do not mean the

The evidence for the labial order⁸ suggests that *p* was a voiceless stop and *f* a voiceless spirant; the phonetic meaning of these symbols seems clear from the Gothic spelling of such Greek loans as *prauſetus* 'prophet' and *ſlippus* 'Philip'. The letter *b* transcribes Greek beta, which represented a phoneme with both stop and spirant allophones in the Greek of Wulfila's time.⁹ Internal evidence suggests that Wulfila's *b* stood for a phoneme with similar allophones. Postvocalic *b* before vowels (*giban* 'give', dat. *hlaiba* 'bread', column 9) alternates automatically with *f* before /+ s t/, where plus represents the open juncture assumed to occur at word boundaries (*gaf* 'gave', acc. *hlaif*, nom. *hlaifs* 'bread', *gaft* 'gavest', columns 10–12).¹⁰ This is usually taken as evidence that postvocalic *b* stood for a voiced spirant which had become unvoiced (as indicated by the arrows in Table 1) before /+ s t/.¹¹ (The cluster /ft/ was inherited from Proto-Germanic.) Postconsonantal *b*, on the other hand, does NOT alternate with *f*: beside *lamba* 'lambs', *silba* 'self', *afswairbands* 'blotting out' (columns 3, 5, 7) we also find *b* in *lamb* 'lamb', *dumbs* 'dumb', *biswarb* 'wiped', *gabaurbs* 'temperate' (columns 4, 8).¹² From this we may deduce that postconsonantal *b* represented a stop, not a spirant; and since it contrasts with *p* (columns 3, 4, 5, 7, 8) it was presumably voiced, not voiceless.

For initial *b* there is no internal Gothic evidence, and we must turn to the spellings of Gothic names in Latin documents. We may note first that contemporary Latin had in the labial order a contrast between initial voiced stop ([b] from earlier [b]) and initial voiced spirant ([β] or [v] from earlier [w]).¹³

same thing as phonetic transcriptions based on speech that has been heard. Except in rare cases, the only phonetic details that we can obtain from written documents have to do with distinctive features. This is even more true of reconstructions: here we cannot possibly uncover any phonetic details, though I believe it is possible for us to reconstruct both the phonemes and the distinctive features which compose them. When, therefore, I cite Gothic (or PGmc.) [b] as a voiced labial stop and [β] as a voiced labial spirant, I shall imply only the three distinctive features of voice, labial articulation, and occlusion. These three features can be read from the written documents and from the comparative evidence. Nothing can be read, and nothing will be implied in my transcription, as to whether [b] and [β] were bilabial or labiodental, fortis or lenis, etc.

⁸ Following André Martinet, *Word* 8.13 (1952), I shall say that a number of phonemes sharing a PLACE of articulation as one of their distinctive features form an ORDER (e.g. Gothic /p f b/); and that a number of phonemes sharing a MANNER of articulation as one of their distinctive features form a SERIES (e.g. Gothic /p t k/).

⁹ Edgar H. Sturtevant, *The pronunciation of Greek and Latin* §95 (1940). Greek beta remained a stop only in gemination and after nasal.

¹⁰ On occasional spellings with postvocalic final *b*, *bs*, and also *d*, *ds*, *z*, see Wilhelm Streitberg, *Indogermanische Forschungen* 18.383–400 (1906). I interpret such spellings as morphophonemic, like modern German *gab*, *gibt*, *gibst*, beside *geben*, *gaben*, etc.

¹¹ E.g. in Max Hermann Jellinek, *Geschichte der gotischen Sprache* 34–5 (1926).

¹² There are no recorded examples of final *lb* (or *lbs*), column 6; the form *halb* which is sometimes cited (e.g. Prokosch, *Comp. Gmc. gram.* 75) does not exist. There is of course a postconsonantal alternation of *b* and *f* in Gothic, due to the effects of Verner's Law, as in pl. *paurbun*, sg. *þarf* 'need'. However, this alternation is not automatic, as is proved by a minimal contrast such as *þarb* 'needy' vs. *þarf* 'needs'; and only automatic morphophonemic alternations (like *giban*, *gaf*) can be taken as evidence for the phonetics of Gothic.

¹³ Sturtevant, *Pron. of Gk. and Lat.* §§154, 199.

In transcribing Gothic names, Latin writers always used *b* for Gothic initial *b*: *Ansbald*, *Hildibald*, *Amalaberga* (presumably with open juncture: /áns+báld/, etc.).¹⁴ From this we may deduce that Gothic initial *b* was definitely a stop; if it had been a spirant, Latin writers would surely have used the letter *v*.¹⁵

This gives us evidence on the phonetics of *b* everywhere except in gemination (column 2). Here we can only cite such loan words as *sabbato* 'sabbath'. Presumably this *bb* was a long voiced stop, as in Greek; but there is no evidence one way or the other.

Let us now attempt a phonological analysis of these orthographic and phonetic data. Three phonemes are necessary to account for the three-way contrasts in columns 1, 3, 4, 5, 8, 9; and three phonemes are sufficient, since there is no four-way contrast anywhere. We may assign all instances of *p* to a phoneme /p/, and all instances of *f* to a phoneme /f/. In column 9 the voiced spirant of *giban* cannot be assigned to /f/, since this environment is already filled by the *f* of *andhofun* 'answered'. Hence we must assign it to /b/, and this is then a phoneme with two allophones: a voiced spirant after vowels, a voiced stop elsewhere.

Two distinctive features characterize these three phonemes: a primary one of voice, which distinguishes voiceless /p f/ from voiced /b/; and a secondary one of occlusion, which distinguishes the stop /p/ from the spirant /f/. Since occlusion is not a distinctive feature of /b/, it can (and does) show both stop and spirant allophones. We may symbolize this relation as (p : f) : b, where the colon outside the parentheses represents the primary opposition of voice, while the colon inside the parentheses represents the secondary opposition of occlusion.

The automatic alternation between /b/ (*giban*, *hlaiba*) and /f/ (*gaf*, *hlaiþ*, *gaft*, *hlaiþs*) shows that the opposition of voice in postvocalic spirants was suspended before /+ s t/. This is confirmed by another set of spirants in Gothic: /s/ and /z/. Before vowels we find such contrasts as *wasuh* vs. *hazuh* 'and was, each', *lauseiþ* vs. *maiza* 'teaches, more'; but before /+/ and /t/ we find only *was*, *has*, *lais*, *mais* 'was, who, know, more', *wast*, *laistjan*, *maist* 'wert, follow, most'.

So much for a structural analysis of Gothic /p f b/. It can hardly be called original, since Bishop Wulfila (as his choice of symbols shows) made the same analysis some 1600 years ago. But at least the agreement is comforting.

If we now turn to the dentals, we find the same situation as with the labials.

¹⁴ Franz Dietrich, *Über die Aussprache des Gothischen während der Zeit seines Bestehens* 71 (1862). The citation of such forms as evidence about biblical Gothic implies that the Gothic represented in these names had the same phonological structure as biblical Gothic. I shall make this assumption in all cases where it does not conflict with the internal evidence of biblical Gothic. Without this assumption the evidence of the names cannot be used at all.

¹⁵ One is tempted to add here that Gothic postvocalic *b* is usually transcribed in Latin with *v*, and Gothic postconsonantal *b* with *b*. Similarly, Latin *November* is transcribed in the Gothic calendar as *naubaimbair*, with postvocalic *b* for Latin *v* and postconsonantal *b* for Latin *b*. If these two types of evidence were reliable, they would be excellent corroboration of the internal Gothic evidence. However, the possibility of sound substitution in both cases is so great that such spellings prove little or nothing. Cf. the cautious remarks in Jelinek, *Gesch. d. got. Sprache* 36-7.

The letters *t* and *p* are in contrast in all environments, and may be interpreted as representing the phonemes /t/ and /p/. Postvocalic *d* before vowels (*anabiudip* 'orders', dat. *stada* 'place', column 9) alternates automatically with *p* before /+/ and /s/ (*anabaup* 'ordered', acc. *stap*, nom. *staps* 'place', columns 10 and 11); again we interpret this as indicating that postvocalic *d* represented a voiced spirant which had become unvoiced before /+/ and /s/. Postconsonantal *d*, on the other hand, does NOT alternate automatically with *p*: beside dat. *landa* 'land', *skulda* 'ought', *waurda* 'words' (columns 3, 5, 7) we also find *d* in acc. *land* 'land', *frijonds* 'friend', neut. *skuld* and masc. *skulds* 'owing', *waurd* 'word', *ubilwaurds* 'evil speaking' (columns 4, 6, 8); and so also after *g* and *z*, the only other consonants which precede *d*: acc. *gahugd* 'thought', *huzd* 'treasure', *gazds* 'sting'. We may again interpret this as evidence of stop pronunciation. Initially (column 1) the Latin evidence does not help, since spellings with *d* for Gothic *d* might represent sound substitution. I assume stop pronunciation in analogy with Gothic /b/ and because all other Germanic languages show stop pronunciation. For *dd* (column 2) there is also no evidence available; I assume stop pronunciation because all other Germanic dialects show stops for geminate /bb dd gg/.

We find in these dentals the same structure as in the labials. A primary feature of voice distinguishes voiceless /t p/ from voiced /d/; a secondary feature of occlusion distinguishes the stop /t/ from the spirant /p/. Since occlusion is not a distinctive feature of /d/, it can (and does) show stop and spirant allophones. The automatic alternation between postvocalic /d/ and /p/ before /+/ and /s/ shows that the opposition of voice in postvocalic spirants was suspended in this latter environment.

The evidence for Gothic *k h g* is quite different from that for the labials and dentals.^{15a} There can be little doubt that they represent three separate phonemes, since we find a three-way contrast in at least seven different environments.¹⁶ Because they are derived from PIE /g k gh/, we would like to group them together as a velar order, corresponding to the labials and dentals. But if we do this, it is clear that their phonological structure was quite different. The retention of postvocalic /g/ in columns 10, 11, and 12 (acc. *dag*, nom. *dags* 'day', *magt* 'canst') puts it in direct contrast with /h/ (acc. *slah* 'stroke', *ahs* 'ear', acc. *maht* 'might'). From this we may deduce that the phonological relationship between postvocalic /g/ and /h/ was not the same as that between /b d z/ and /f p s/: postvocalic /g/ cannot have been the voiced counterpart of /h/.

The most immediately obvious way to explain this lack of correlation would be to assume that postvocalic /g/ represented a stop (necessarily voiced, since it contrasted with /k/). This would create a structural anomaly: /g/ would be the only postvocalic voiced stop in Gothic. But that is not a compelling objection, since (as we shall see later) Old English (medially) and Old Saxon (medially

^{15a} I interpret the symbols *q* and *h* as representing respectively the clusters /kw/ and /hw/. See *Lg.* 24.83 fn. 28 (1948).

¹⁶ The absence of /nh/ in columns 3 and 4 reflects the change of PGmc /inx anx unx/ [i:x a:x u:x] to Gothic *eih ah uh* /i:h a:h u:h/. In column 7 the lack of evidence for /rk+/ is surely fortuitous. On /hh/, see note 6 above.

and finally) also show only one postvocalic voiced stop, namely /d/. It would also create a comparative anomaly: of the three PGmc. phonemes /b d g/, it is precisely /g/ which (as we shall see later) shows spirantal allophones in the greatest number of environments. The most convincing argument against assuming stop pronunciation for postvocalic /g/, however, is the evidence of Latin spellings of Gothic names. Here we find *Eila* beside *Egila* (and *Agila*), *Sisi(fridus)* beside *Sigis(vultus)*, etc.;¹⁷ and the most likely explanation of such spellings is that they reflect spirantal pronunciation of Gothic intervocalic /g/.

All the evidence we have, then, points to the fact that postvocalic /g/ was a spirant and not a stop. We must therefore look for another explanation of the fact that /g/ was not the voiced counterpart of postvocalic /h/. The most likely hypothesis is that /h/ did not belong to the velar order at all, but was a glottal spirant.¹⁸ The best argument in favor of this assumption is a negative one. If Gothic *h* had represented a (palato)velar spirant, we would expect to find it used to transcribe the Greek palatovelar spirant *χ* in such words as *Xovfā*, *Tvχukós*, *Zaxapias*, *Avróχeia*. But Greek *χ* is regularly transcribed with Gothic *k*, in a few instances by the special Gothic letter *x*, NEVER with Gothic *h*.¹⁹ From this we may infer that Gothic /h/ was not the phonetic equivalent of Greek *χ*, and hence that it was not a (palato)velar spirant.

The positive evidence in favor of assuming that Gothic /h/ was a glottal spirant is rather slim. It transcribes the Greek rough breathing; but this was little more than a scholastic tradition by the time of Wulfila.²⁰ It apparently alternated with zero, since the scribes sometimes left it out (writing *liuteiþ* for *liuhteiþ* 'shines', *harjo* for *harjoh* 'each', etc.) and sometimes added it incorrectly (writing *waurhtai* for dat. *waurtai* 'root', *þauh* for *þau* 'than'—though here two pairs of words may be confused).²¹ Further possible evidence that it alternated with zero are the spellings *freijhals* 'freedom' (presumably /fri:ʝals/) beside more common *freihals* (presumably /fri: + hals/), and the hapax legomenon

¹⁷ Dietrich, *Aussprache d. Got.* 74. Ostrogothic examples in Ferdinand Wrede, *Über die Sprache der Ostgoten in Italien* 173-4 (1891).

¹⁸ It is of course true that the prototypes of *g* and *h* shared the feature 'velar articulation' in Pre-Gmc. times when the automatic alternation known as 'grammatical change' arose through Verner's Law. Cf. Gothic *filhan* 'hide' but *fulgin* 'hidden', *taihun* 'ten' but *tiguns* 'tens', etc. They were also still paired in Pre-Gothic times when the automatic alternation known as 'spirantal dissimilation' arose, whereby a spirant following an unstressed vowel was voiceless if the vowel was preceded by a voiced phoneme, but voiced if the vowel was preceded by a voiceless phoneme. Hence a given suffix has the shape -is- in *agisa* 'fear', *rimisa* 'quietness', but -iz- in *hatiza* 'hate', *riqiza* 'darkness'; and, for the velars, -ah- in *ainaha* 'only' but -ag- in *wulpaga* 'glorious'. (By the time of recorded Gothic this formerly automatic alternation had been mostly lost in the case of the velars: *h* appears only after voiced phonemes, but *g* appears after both voiceless and voiced. Cf. Jellinek, *Gesch. d. got. Spr.* 67.)

¹⁹ See Jellinek, *Gesch. d. got. Spr.* 32. The Gothic letter *x* is used twice (by error?) to transcribe Greek *κ*; otherwise it is used only to transcribe Greek *χ* (always in the abbreviations for *Χριστός*, and nine times in other Greek words).

²⁰ Cf. Sturtevant, *Pron. of Gk. and Lat.* §83; Jellinek, *Gesch. d. got. Spr.* 37-8.

²¹ Jellinek, *Gesch. d. got. Spr.* 72. But see also the skeptical remarks in W. van Helten, *Indogermanische Forschungen* 14.74-5 (1903).

gudhusa (dat.) 'house of God' (representing /gúdu:sa/ beside /gúp+hù:sa/?).²² It was also far more subject to assimilation than any other Gothic phoneme.²³

All of this is slim evidence indeed. The best we can say is that a sound whose symbol was used to transcribe the Greek rough breathing, which alternated with zero, and which was subject to extensive assimilation, may well have been a glottal spirant. Since it was clearly not a velar spirant, there is not much else that it can have been. I shall therefore assume that Gothic /h/ was a glottal spirant in all positions.²⁴

If /h/ was a glottal spirant, and did not share any distinctive feature with /g/, the way is open for the velar order to be composed of the same four phones as the labials and dentals. We assume /k/ to have been a voiceless stop in all environments; /g/ may then have had as its allophones, in complementary distribution, a voiced stop, a voiced spirant, and a voiceless spirant. We have already assumed that it was a voiced spirant intervocalically (*dagos* 'days', column 9); we may now assume that, parallel to the treatment of the other voiced spirants, this was unvoiced to [x] before /+ s t/ (*dag*, *days*, *magt*, columns 10–12).²⁵ On the analogy of /b/ and /d/ we may also assume a voiced stop in gemination and after consonants (columns 2–8); this is supported by the rather distant evidence of Ostrogothic.²⁶

²² Wilhelm Streitberg, *Indogermanische Forschungen* 24.181 (1909).

²³ In the more conservative manuscripts (Silver Codex, excluding Luke, and Ambrosian Codex B) it is assimilated only to a following /p/, and even that not commonly. In other manuscripts, especially Ambrosian A, it is assimilated to a following /k s b d g m n l r/. Cf. Josef Janko, *Prager deutsche Studien* 8.64–5 (1908).

²⁴ Scholars who consider /h/ to have been a glottal spirant before vowels often insist that it must have been at least a weak velar spirant before consonants and finally, since otherwise it would not have been audible. Cf. Hermann Paul, *Beiträge zur Gesch. d. dt. Spr. und Lit.* 1.156 (1874); W. van Helten, *Indogermanische Forschungen* 14.75 (1903). However, we do not even need to go outside of Germanic to find examples of a perfectly audible pre-consonantal [h]: cf. the preaspirated stops of modern Icelandic described by Einar Haugen, *Acta linguistica* 2.98–107 (1941).

²⁵ Jellinek, *Gesch. d. got. Spr.* 35, considers this analysis, but rejects it. His reasoning is: Gothic *x* was copied from Greek *X*; Greek *X* stood for a voiceless palatovelar spirant; therefore Wulfila would surely have used it for any voiceless palatovelar spirant in Gothic; therefore Gothic cannot have had any voiceless palatovelar spirant, and postvocalic *g* before /+ s t/ must represent a stop. For two reasons this argument strikes me as most unconvincing. On the one hand, Wulfila seems to have had no qualms at all about borrowing a letter representing one type of sound in Greek and using it for quite a different type of sound in Gothic. Witness his borrowing of Greek *ψ* for his *þ* symbol, and of Greek *θ* for his *h* symbol. Secondly, Jellinek's argument assumes that a native speaker (Wulfila) is sufficiently aware of the allophones of his own language to use special symbols for them. This is of course contrary to nearly all our observations of the way in which native speakers write their own languages.

To test the hypothesis which (following many other investigators) I am proposing—that Wulfila's *g* stood for [x] before /+ s t/—we would need to know how Wulfila transcribed Greek *χ* in final position, the only position where by our hypothesis both languages had [x]. Unfortunately no such forms occur. The name 'Ερώχ occurs once (Luke 3:37), but only with an ending in Gothic: gen. *ainokis*, translating τοῦ 'Ερώχ.

²⁶ Wrede, *Sprache der Ostgoten* 173–4.

It remains for us to determine what allophone /g/ showed in initial position. Here the evidence is conflicting. The analogy of /b/ suggests that initial /g/ was a stop; this is supported by the fact that Latin documents spell it consistently as *g*.²⁷ Beside this evidence, however, we must place that of the two loan words *kreks* 'Greek' and *marikreitung* 'pearls'; cf. Latin *Graecus* and *maragrīta* (or *margarīta*).²⁸ Here voiceless [k] seems to have been substituted for voiced [g]; and presumably this substitution was made because the borrowing language had no initial [g], but only initial [k] and [g]. These two bits of evidence suggest that Gothic initial /g/ was a spirant at the time of the borrowings, but had become a stop by the time the Gothic names appear in Latin documents. Since these Latin documents date back to the beginning of the 4th century, I shall assume (without too much conviction) that initial /g/ was a stop in the Gothic of our biblical manuscripts.

These assumptions give the following structure for the Gothic stops and spirants; the probable allophones of /b d g/ are as indicated in Table 1: (p : f) : b, (t : þ) : d, k : g, s : z, h.

3. Pre-Old-Icelandic changes. Before we turn to the evidence of the older Norse runic inscriptions and of Old Icelandic, it will be helpful to examine two changes which occurred during the Pre-Old-Icelandic period: the unvoicing of all voiced stops and spirants in final position; and the subsequent voicing of all medial and final voiceless spirants (except /s/) which did not stand in clusters with voiceless phonemes.²⁹

The unvoicing of final stops is made clear by the following sets of correspondences. Beside OE *bindan* *band* 'tie', *giēdan* *geald* 'pay', *springan* *sprang* 'jump' we find OIc. *binda* *batt*, *gialda* *galt*, *sprunga* *sprakk*. Further, beside OE *camp* 'battle', *brant* 'steep', *drincan* 'drink' we find OIc. *kapp*, *brattr*, *drekka*. From a comparison of these two sets of forms we may draw three conclusions: (1) Pre-OIc. /d g/ were stops in the clusters /nd ld ng/³⁰; (2) they next became

²⁷ Dietrich, *Aussprache d. Got.* 73-4; Wrede, *Sprache der Ostgoten* 173 and note 3. With Wrede, we may interpret such spellings as *Vitiscslus* beside *Witigiscslus* as representing /wítigi:sl/ beside /wítí+gi:sl/, respectively. (Wrede of course does not present an analysis in terms of close and open juncture, but he clearly gives the 1891 equivalent.)

²⁸ There are exact parallels to Gothic *kreks* in West Germanic: OE *Crēcas* (pl.), OFris. *Crēc(land)*, OHG *Kriahi* (pl.). On this loan, see Gustaf Kossinna, *Zur Geschichte des Volksnamens Griechen*, *Festschrift zur 50jährigen Doktorjubiläum Karl Weinholds* 27-42 (1896).

The form *marikreitung* is dative plural. It occurs in only one passage (1. Timothy 2:9), but is attested in two manuscripts. I interpret the *k* as 'initial' because the word was presumably /mári+kri:tum/, with the stress and juncture of a compound. Cf. the treatment as a compound in OE *meregrēot*, OS (Heliand C) *merigriotun*, OHG *merigrioz*, literally (by popular etymology) 'sea grit'.

²⁹ For the unvoicing of final stops, see Adolf Noreen, *Altisländische und altnorwegische Grammatik* (1923) §220, final spirants §223.2; voicing of voiceless spirants §240.1 (f), §221.1 (þ), §230 (h).

³⁰ Pre-OIc. /b/ was also a stop in the cluster /mb/; cf. OIc. *lamb* 'lamb' etc. However, there are no recorded examples of final /mb/ > /mp/; cf. Noreen, *Aisl. Gram.* §§219 Anm. 1, 220 Anm. 2. To complete the evidence on /b d g/, we should add that Pre-OIc. /d/ seems to have been a stop also after /z/. Cf. Gothic *huzd* 'treasure', *gazds* 'sting', OIc. *hodd*, *gaddr*.

unvoiced in final position, giving /nt lt nk/; (3) all occurrences of /mp nt nk/ were then assimilated to /pp tt kk/.²¹ The sequence of changes is: */bánd, gáld, spráng/ > */bánt, gált, spránk/ > OIc. *batt, galt, sprakk*.

As regards relative chronology, the unvoicing of /nd ld ng/ to /nt lt nk/ must have been completed before the loss of final nasalized /a/ after long syllable, since Pre-OIc. */lámba, bánda, gélda, gánga/ give OIc. *lamb, band, gíald, gang* 'lamb, bond, payment, course,' with final /b d g/ preserved. This loss of final /a/ can be dated as follows: it is preserved in runic *-wulafa* (Istaby, ca. 650) and *-wolafa* (Gummarp, ca. 700) 'wolf' (in names), but it is lost in runic *sot* < */só:hta/ 'sought' and *la[n]t* < */lánda/ 'land' (both Eggjum, ca. 700).²² If this loss of final /a/ therefore dates from the beginning of the 8th century, the unvoicing of final voiced stops must have been completed by the end of the 7th century.

This unvoicing of the stop allophones of final /b d g/ is quite clear; much less clear is the unvoicing of their spirantal allophones. There are two bits of evidence for it. First, we find runic *alawid* 'Allviþ' (man's name) with final *d* from the 5th century (Skodborg), but *gaf* 'gave' and *bariutiþ* 'breaks' with final *f, þ* < PGmc. /b d/ from the second half of the 7th century (Stentoften). From this we may conclude that final postvocalic /b d/ were voiced spirants in the 5th century, but that they were unvoiced to /f þ/ by the second half of the 7th century. This dating agrees with that of the unvoicing of final voiced stops: it was completed by the end of the 7th century.

The second bit of evidence concerns the unvoicing of the spirantal allophones of final /g/. Here there are no runic examples, and we must turn to the evidence of recorded Old Icelandic. Corresponding to Gothic *mag* 'can' and *þwah* 'wash' (imperative) we find OIc. *mā* and *þuā*. From this we may conclude (1) that the final /g/ of Pre-OIc. */mág/ was a voiced spirant, like /b/ and /d/; (2) that it was unvoiced to */máh/; and (3) that the final /h/ of */máh, þwáh/ was then lost, giving OIc. *mā* and *þuā*. This unvoicing must also have been completed before the loss of final /a/ after long syllables, since Pre-OIc. acc. sg. */dájga/ gives OIc. *deig* 'dough' with final /g/ = [g] preserved.

If we combine the evidence for the unvoicing of the final stop allophones of Pre-OIc. /b d g/ in the clusters /mb nd ld ng/ with the evidence for the unvoicing of their final spirant allophones in all other environments, we arrive at a considerable structural change in the phonology of Pre-Old-Icelandic: voice is given up as a distinctive feature of obstruents in final position—all final obstruents are voiceless.²⁴ Our evidence has shown that this change took place

²¹ Cf. Noreen, *Aisl. Gram.* §266.1-3.

²² All runic forms and datings are from Noreen, *Aisl. Gram.* 374-93. They can easily be located with the aid of Noreen's special index, 462-6.

²³ *bariutiþ* (with epenthetic *u*) represents PGmc. */br̥wtid/. That Old Norse generalized the PGmc. 2nd and 3rd sg. endings /-iz, -id/ < PIE /-esi, -eti/ rather than /-is, -ip/ < PIE /-ési, -éti/ is evident from the 2nd sg. ending -r < -x, which must come from /-is/ rather than /-is/. Cf. runic *barutx* 'breaks' (Björketorp, ca. 700), where this 2nd person ending has already spread to the 3rd person.—This interpolation is necessary, since otherwise the ending of *bariutiþ* could be construed as representing PGmc. /-ip/.

²⁴ Since runic *x* (< PGmc. /x/) did not become /s/ through this change, it is clear that by

after the 5th century, and that it was completed by the end of the 7th century, before the loss of final /a/. We may label it (more for convenience than for chronological exactitude) the '7th-century unvoicing.'

In the case of final stops, voice was very soon re-established as a distinctive feature through the loss of final /a/. The evidence can be most easily presented in tabular form. Reconstructions are phonemic.

	5th-cent.	7th-cent. unvoicing	8th-cent. loss of /a/	Old Icelandic spelling
/mb+/	?	?	?	
/mba/	lámba	lámba	lám̥b	lamb 'lamb'
/nd+/	bánd	bánt	bánt	batt 'bound'
/nda/	bánda	bánda	bánd	band 'bond'
/ld+/	gáld	gált	gált	galt 'paid'
/lda/	gélða	gélða	géald	giald 'payment'
/ng+/	gáng	gánk	gánk	gakk 'gol'
/nga/	gánga	gánga	gáng	gang 'course'

As the table shows, after the loss of final /a/ the clusters /nt lt nk/ were in contrast with /nd ld ng/; there are no examples for a similar contrast of /mp/ with /mb/.

It is possible that the 8th-century loss of final vowels also re-established voice as a feature distinguishing final spirants: after *[rájð] 'he rode' had been unvoiced to *[rájþ], the *ō*-stem noun *[rájðu] 'riding', upon losing its final vowel, would presumably have become *[rájð]. If, however, we wish to regard as best that hypothesis which assumes the smallest number of structural changes, we shall assume that this development never took place, but that *[rájþ] 'rode' became *[rájð] again before (or along with) the change of *[rájðu] 'riding' to *[rájð]. For also during the 8th century, apparently, a major structural change took place in Pre-Old-Icelandic: all voiceless spirants except /s/ became voiced in voiced surroundings, including final position. Voiceless /f þ/ thereby became [b ð] and hence merged with the spirantal allophones of /b d/; and, apparently as part of this same voicing, /h/ was lost.²⁵ A few examples of the dentals will

this time it was no longer the voiced counterpart of /s/. I do not know what distinctive feature(s), if any, it shared with other phonemes. It fell together with /r/ in preliterate times. The earliest indication of this is the spelling *afatz* 'after' on the Istaby stone (ca. 650), with *a* for inherited /r/. (Noreen's reading *afair*, *Aisl. Gram.* 380, is incorrect. Cf. the photograph in Wolfgang Krause, *Runeninschriften im älteren Futhark* [1937], plate 79).

²⁵ Although isolated spellings with *f* and *þ* for inherited /b d/ occur as early as the middle of the 6th century, inscriptions from around the year 700 (Björketorp, Eggjum, Stentof-ten) still distinguish /f þ/ from /b d/ almost without exception. (Cf. Noreen, *Aisl. Gram.* §221 Anm. 2.) I shall therefore refer to this voicing as an '8th-century' change.

The earliest loss of /h/ in voiced surroundings (excluding the assimilation of /ht/ to /tt/ and the dissimilation of /hs/ to /ks/) is *sigadux* < */sīgu+hādus/ 'Sigöpr' (Svarteborg, shortly after 400)—a rather special case, since it involves loss of open juncture. Otherwise /h/ is not lost until ca. 700: Björketorp *-sð* < */spáhu/ (though the same inscription keeps /lh/, with epenthetic vowel, in *lalah*). Cf. Noreen, *Aisl. Gram.* §230.1.

Bohumil Trnka, Phonological remarks concerning the Scandinavian runic writing,

make this development clear. They also illustrate the development of the labials; examples of /h/ and /g/ will be given later. Reconstructed forms are phonetic.

	5th-cent.	7th-cent. unvoicing	8th-cent. voicing and loss of final vowels	Old Icelandic spelling and phonetics
/VpV-/	lí:þan	lí:þan	lí:ðan	liþa [lí:ða] 'go'
/VdV-/	rí:ðan	rí:ðan	rí:ðan	riþa [rí:ða] 'ride'
/VpV+/-	lájþu	lájþu	lájð	leiþ [léjð] 'going'
/VdV+/-	rájðu	rájðu	rájð	reiþ [réjð] 'riding'
/Vp+/-	lájþ	lájþ	lájð	leiþ [léjð] 'he went'
/Vd+/-	rájð	rájþ	rájð	reiþ [réjð] 'he rode'

Pre-Old-Icelandic /h/ and /g/ remain to be discussed. We have seen above that the 7th-century unvoicing apparently changed */mág/ = [mág] 'can' to */máh/, since by the 8th-century voicing it became OIc. *mā* just as */þwáh/ 'wash!' became OIc. *þuā*. Further parallel developments are: */stájg, fláwg, dró:g/ > OIc. *stē, flō, drō* 'climbed, flew, drew', just as */tájh, fláwh, sló:h/ > OIc. *tē, flō, slō* 'showed, fled, slew'. This would seem to prove that, in the environments where /g/ was a spirant, Pre-OIc. /g/ and /h/ belonged structurally to the same (velar) order and were distinguished by the same feature of voice as were /b—f/ and /d—þ/.

Two other bits of evidence, however, make it clear that /g/ and /h/ were NOT voiced-voiceless counterparts. First, the 8th-century voicing did not change /h/ to [g] (as it changed /f þ/ to [b ð]), but rather caused its loss: */sláhan, sló:h/ > OIc. *slā, slō* 'slay, slew'; */félhan, fálh/ > OIc. *fela, fal* 'hide, hid'; */fórhu/ > OIc. *for* 'furrow'; etc.³⁶ Secondly, the 7th-century unvoicing quite clearly did not change postconsonantal [g] to /h/, since Pre-OIc. */bárg, swálg/ = [bárg, swálg] did not become OIc. **bar, *suar* but rather *barg, sualg* = [bárg, swálg] 'helped, swallowed'. The only plausible explanation for this is that **[bárg, swálg]* were not unvoiced to **[bárh, swálh]*, but rather to **[bárx, swálx]*; and that the 8th-century voicing changed these back to [bárg, swálg].³⁷

Travaux du Cercle linguistique de Prague 8.292-6 (1939), argues convincingly that it was this voicing of voiceless spirants which caused the graphic contrasts *p-f-b, t-p-d, k-h-g* to be reduced in the younger futhark to *b-f, t-p, k-h*. The first step in this 'dephonologization of the voice of spirants' was the change of /z/ to /x/; the remaining steps were the voicing of /t/ and /p/, and the assimilation, dissimilation, and loss of non-initial /h/.

³⁶ This argument is not entirely decisive, since conceivably /h/ could have been lost before or independently of the 8th-century voicing. However, since /h/ became lost in precisely the same environments in which /t/ and /p/ became voiced, I prefer to consider the loss of /h/ as part of the 8th century voicing.

³⁷ OIc. *barg, sualg* could theoretically have analogical *g*, but this is highly unlikely. Post-vocally we find in class I strong verbs both regular *hnē, mē, sē, stē* and analogical *hneig, meig, seig, steig*; in class II both regular *flō, lō, mō, smō* and analogical *flaug, laug, maug, smaug*; in class III only regular *brā*; in class V both regular *vā, frā, lā, þā* and analogical *lag*; and in class VI only regular *drō*. (Noreen, *Aisl. Gram.* §§482, 486, 495, 497-8, 501.) This means that analogical formations certainly do occur—but only beside regular ones. It is

If this line of reasoning is correct, two deductions may be made from it: (1) Pre-OIc. /h/ did not share with /g/ (and /k/) the distinctive feature of velar articulation, but was presumably a glottal spirant; (2) final /Vg/, like /lg/ and /rg/, was presumably first unvoiced to [Vx], but then underwent a phonemic change to /Vh/. The sequence of changes would be as follows; reconstructed forms are phonetic:

	5th-cent.	7th-cent. unvoicing	Vx > Vh	8th-cent. voicing and loss of final vowel	Old Icelandic spelling and phonetics
/VhV-/	sláhan	sláhan	sláhan	slá:an	slā 'slay'
/VhV+/-	tájhu	tá:hu	tá:hu	tó:	tō 'toe'
/Vh+/-	sló:h	sló:h	sló:h	sló:	slō 'slew'
/lhV-/	félhan	félhan	félhan	félan	fela 'hide'
/lhV+/-	fálhǫ	fálhǫ	fálhǫ	fál	fal 'covering (acc.)'
/lh+/-	fálh	fálh	fálh	fál	fal 'hid'
/VgV-/	drágan	drágan	drágan	drágan	draga [drága] 'draw'
/VgV+/-	dájga	dájga	dájga	dájg	deig [dég] 'dough (acc.)'
/Vg+/-	dró:g	dró:x	dró:h	dró:	drō [dró:] 'drew'
/lgV-/	swélgan	swélgan	swélgan	swélgan	suelga [swélga] 'swallow'
/lgV+/-	swálgǫ	swálgǫ	swálgǫ	swélg	suelg [swélg] 'throat (acc.)'
/lg+/-	swálg	swálx	swálx	swálg	sualg [swálg] 'swallowed'

Having established the Pre-OIc. phonemic status of /h/, the 7th-century unvoicing, and the 8th-century voicing, we may now turn to the recorded evidence of the older runic period.

4. The evidence of the older Norse runic inscriptions. Most of the Gothic material which we examined in §2 comes from a single historical period, is written in a remarkably consistent orthography, and is extensive enough to present evidence on most of the environments that we wish to test. In all three of these respects the older Norse runic materials are precisely the opposite. They extend in time from the 3rd to the 8th century; they are written in an orthography which presents great problems of interpretation; and the materials are so scanty that they contain reasonably sure examples for barely half of the environments in which we are interested. With all their deficiencies, however, they offer valuable evidence.

The runic forms which we shall examine are written in the older, 24-rune futhark that is found, for example, on the 6th-century bracteate of Vadstena, Sweden. It can be transcribed as *f u þ a r k g w : h n i j é p r s : t b e m l ŋ o d*.³⁸

therefore almost inconceivable that in *barg* and *sualg* ONLY analogical forms should have survived.

³⁸ See Krause, *Runeninschriften*, plates 5 and 6. Following Krause (and many others), I use *b d g* in transcribing runic forms, rather than the *ḃ ḏ ȝ* that appear in many handbooks. These latter symbols are typographically inconvenient; they are also misleading, since they make it look as though the runic inscriptions offered evidence for spirantal

TABLE 2. THE EARLY NORSE RUNIC EVIDENCE

	1. C-	2. CC	3. NCV	4. NC+	5. ICV	6. IC+	7. rCV	8. rC+	9. VCV	10. VC+	11. VCs	12. VCt
/p/ [p]	?	?	?	?	?	?	?	(warb ⁷⁰⁰)	(kaiba ⁷⁰⁰)	?		
/f/ [f]	faihid ⁴⁰⁰	?	?	?	-wulafa ⁶⁰⁰	?	?	?	?	?	-wulfs ⁶⁰⁰	after ⁶⁰⁰
/b/ [b]					?	?	arbija ⁶⁰⁰	?	gibu ^{6th}	?		
/b/ [b]	birg ^{6th}	?	?	?								
/t/ [t]	tawido ⁴⁰⁰	sate ⁷⁰⁰	glanta ^{6th}	?	holtijar ⁴⁰⁰	?	wurte ⁶⁰⁰	?	haitinar ⁶⁰⁰	pat ^{7th}	?	(see 2)
/p/ [p]	par ⁴⁰⁰	?	skinpale ⁶⁰⁰	afunp ⁷⁰⁰	owulpu ³⁰⁰	?	?	?	lapu ^{6th}	?	bariuti ^{6th}	
/d/ [ð]							?	?	tawido ⁴⁰⁰	slawid ^{6th}		
/d/ [d]	dagar ⁴⁰⁰	?	ungandir ⁴⁰⁰	(lant) ⁷⁰⁰	-staldir ⁶⁰⁰	uilald ^{6th}						
/k/ [k]	-kurne ^{6th}	?	?	?	?	?	wurki ⁶⁰⁰	?	laukar ⁶⁰⁰	ek ⁴⁰⁰	sakse ⁷⁰⁰	
/g/ [g]					?	?	arageu ⁷⁰⁰	birg ^{6th}	dagar ⁴⁰⁰	?		
/g/ [g]	-gastir ⁴⁰⁰	lagi ⁷⁰⁰	iupingar ⁶⁰⁰	?								
/h/ [h]	horna ⁴⁰⁰		?	?	wilha ⁶⁰⁰	falah ⁷⁰⁰	worahto ⁶⁰⁰	?	faihid ⁴⁰⁰	ail ^{6th}	?	dohtrir ⁶⁰⁰

The symbols of particular interest to us are *p f b*, *t þ d*, *k h g*, *s z*. During the period ca. 650–750, the graphic contrasts represented by *p–b*, *t–d*, *k–g* were given up, and in the younger futhark of 16 symbols only *b*, *t*, *k* were used.

The runic evidence is presented in Table 2. Superscript numbers indicate the approximate date or century of the inscription; interpolated letters are in italics; parentheses enclose forms showing a transition to the younger, 16-rune futhark. Small cap *A* stands for the earlier *j* rune, which came to be used for *a* when its name changed by regular phonetic development from *jāra* to *āra*. The arrows in columns 4, 6, 8, and 10 indicate the 7th-century unvoicing of final stops and spirants and the change of final /Vg/ = [Vx] to /Vh/.

In the labial order no examples of runic *p* can be given, since it is never found in the spelling of a word, but only in futhark listings. The forms *warb* 'threw' and *kaiba* 'sleigh runners' (Olc. *varp*, *keipa*) show the later runic use of *b* for *p*; the same inscription (Eggjum) also shows the later use of *k* for *g* in *fokl* 'bird' (Olc. *fogl*) and of *t* for *d* in *la[n]t* 'land' (Olc. *land*). In column 11, the form *-wulfs* represents /ICs/ rather than /VCs/.

In the dental order the form *sate* 'he set' is given in the /tt/ column since it comes from an earlier */sátide/ (cf. 1st sg. *satido* on the Rø stone, shortly after 400) and corresponds to Olc. *sette*.³⁹ The form *wurte* 'he wrought' is doubtful, since it may perhaps represent /wúrhte/ with preservation of /h/, or /wúrtte/ already showing assimilation of /ht/ to /tt/. The form *la[n]t* 'land' shows late *t* for *d* (see above) and also loss of final /a/.

In the velar order the form *sakse* 'stone' shows the dissimilation of /hs/ to /ks/. In *arageu* 'magic' the second *A* is epenthetic. The form *lagi* 'they lay' (pres. subj.) is given in the /gg/ column since it corresponds to Olc. *leggi*.

In the /h/ row the form *wilha-* is given because it probably represents *walha-* 'foreign'. The second *A* of *falah* and the *a* of *worahito* are epenthetic; the latter form represents /rhC/ rather than /rhV/. The environment /NhV/ in column 3 could perhaps be filled by the form *haha* (acc. sg.) 'block of wood' (Strøm, 1st half of 7th century), but the phonemics are probably different from this. Phonetically the form seems to have been [hɑ:hɑ] < PGmc. */hánxan/ < PIE */kánkom/.⁴⁰ We may be sure that the stressed vowel was long and nasalized, since the First Grammarian later on gives this word (in his spelling *hár*, acc. *hđ*) as his example of long nasalized /ɑ:/ contrasting with long oral /a:/ in

articulation where Gothic does not. Prokosch, for example, seems to have been misled by them when he writes (*Comp. Gmc. gram.* 75): 'Initial *b* and *d* appear as stops everywhere in Germanic, with the exception only of a few isolated forms in early Runic inscriptions, (*darutR* = *brjtr* "breaks", Björketorp; *ðohtriR* = *dōtr* "daughters", Tune ...), for which, however, other explanations might be given.' (See also a similar remark on p. 58.) These runic spellings are not isolated, but entirely regular; and they offer no information whatever on stop vs. spirant pronunciation. In this respect the futhark alphabet was quite phonemic, and hence provided no symbols to distinguish [b ð] from their allophones [b d].

³⁹ Geminat consonants are never written double in the older runic inscriptions. Cf. Alexander Jóhannesson, *Grammatik der urnordischen Runeninschriften* §60 (1923).

⁴⁰ See Walde-Pokorny, *Vergleichendes Wörterbuch der idg. Sprachen* 1.335 (1930).

hár 'hair'.⁴¹ Therefore, since there was no contrast between runic [a:h] and [anh], the stressed vowel might be analyzed as /an/. On the other hand, if the unstressed vowel was also nasalized (as is usually assumed), it contrasted with unstressed final [an]: Pre-OIc. acc. *[dága] > OIc. *dag* 'day', but Pre-OIc. infinitive *[drágan] > OIc. *draga* 'draw'. Hence nasalization was probably phonemic rather than allophonic, and *haha* was */há:ha/ and not */hánhan/.

By borrowing heavily from the later Old Icelandic evidence it is possible to make a phonemic analysis of the runic material. In the dental order, where the runic evidence is least scanty, we find a three-way contrast in columns 1, 3, 5, 9, and 10, and so we may be sure that there were three separate phonemes, /t þ d/. Transitional spellings with *t* indicate that /d/ was a stop after /n/ and /l/: *la[n]t* 'land' (Eggjum, ca. 700), *asmu[n]t* 'Asmund' (Sölvesborg, end of 8th century), *rhoaltz* 'Hroaldr' (Vatn, beginning of 8th century). The form *bariutib* < */briwtid/ (discussed in §3) is evidence that postvocalic /d/ was a spirant, unvoiced in final position to /p/ by the 7th-century unvoicing. This covers the allophones of /d/ in columns 3, 4, 5, 6, 9, 10; the remaining assignments of allophones are based on the later OIc. evidence.

The evidence for the labial order is so scanty as to be almost worthless. However, even without the parallel evidence of the dental order and of Old Icelandic, we could assume three phonemes /p f b/ because the futhark alphabet provides symbols for a three-way contrast—even though the letter *p* is never used in the spelling of an actual word. As for allophones of /b/, the alternation of *b* in *gibu* 'I give' with *f* in *gaf* 'he gave' (columns 9 and 10) is evidence that postvocalic /b/ was a spirant, unvoiced finally to /f/ by the end of the 7th century. The remaining assignments of allophones are based on the later OIc. evidence.

The velar order shows enough two-way contrasts (columns 1, 7, 9) to justify our setting up the phonemes /k/ and /g/. The assignment of allophones of /g/ is based on the later OIc. evidence. From the discussion in §3 it is clear that /g/ at first had only two allophones: [g] initially,⁴² in gemination, and after /n/, but [g] elsewhere. The 7th-century unvoicing then gave it another allophone by changing final [lg rg Vg] to [lx rx Vx]. This was later given up by the change of [Vx] to /Vh/ and by the voicing of [lx rx] back to [lg rg].

The setting up of /h/ as distinct from the velar order has already been justified in §3. This /h/ seems to have developed special allophones before /s/ and /t/, leading to the changes /hs/ > /ks/ (so already *sakse* 'stone' in column 11) and /ht/ > /tt/ (cf. runic *dohtriæ* 'daughters', column 12, but OIc. *dóttir* 'daughter'). By the 8th-century voicing of spirants it was then lost everywhere except initially.

The above assumptions give the following structure for the older runic Norse stops and spirants: (p : f) : b, (t : þ) : d, k : g, s, h. Except for the change of

⁴¹ See Einar Haugen, *First grammatical treatise* 16, 34 (LANGUAGE MONOGRAPH No. 25, 1950). The First Grammarian uses the word in the meaning 'shark'; it also meant 'thole' in Old Icelandic.

⁴² For convincing evidence that initial /g/ was a stop in Old Norse—and hence, as far as we can tell, in older runic—see Stefán Einarsson, *Journal of English and Germanic philology* 40.42-7 (1941).

/z/ to /r/ (see note 34 above), this is of course the same phonological system that we found in Gothic. The two systems differ only in the distribution of phonemes and allophones. Both Gothic and runic show stop allophones for /b d g/ initially and in gemination. Gothic /b d g/ were also stops after all consonants; runic /b d g/ were stops only after nasals and, in the case of /d/, after /l r/.⁴³ The spirantal allophones of Gothic /b d g/ were unvoiced in final position: [b̥ ð̥] > /f þ/, and [g] > [x]; runic /b d g/ did not undergo this development until the 7th-century unvoicing, when both the spirant AND the stop allophones were unvoiced in final position: [b d g] > /p t k/, [b̥ ð̥] > /f þ/, and [g] > [x]. The spirantal allophones of Gothic /b d g/ were also unvoiced before /s/; we shall find a similar development in Old Icelandic, but the syncopes needed to produce such clusters had not yet occurred in older runic.

5. The Old Icelandic evidence. The Old Icelandic evidence is presented in Table 3. We have already seen how a good many of the structural contrasts in the runic material were at least temporarily lost through various Pre-OIc. changes. The positions earlier filled by final /mb nd ld ng/ became empty when the 7th-century unvoicing changed these to /mp nt lt nk/. Then the positions filled by medial and final /mp nt nk/ became empty when these clusters were assimilated to /pp tt kk/. Further, the positions filled by medial and final /nþ lþ/ became empty when these were changed by another assimilatory process to /nn ll/; and /mf/ was lost when /Vmf/ became /V:f/ (with long nasalized vowel).⁴⁴ At the same time, of course, other changes were producing forms that gave new structural contrasts and re-established old ones. The dissimilation of /hs/ to /ks/ (runic *sakse*) gave a new cluster /ks/; and the loss of final /a/ (/lánda/ > /lánd/ etc.) refilled the positions for final /mb nd ng/. One change in particular—the 8th-century voicing of spirants—served to remove a large number of structural contrasts; this is indicated by the arrows in Table 3. Another change in particular—the syncope of medial vowels—served to establish a large number of new structural contrasts; this is indicated by the italicized forms in Table 3.⁴⁵

The labial order shows most clearly the structural results of the 8th-century voicing of voiceless spirants. For the older runic period we have assumed three-way contrasts extending from column 1 through column 10. The [f-b] contrast in final position (columns 6, 8, 10) was lost when final [b̥] > [f] by the 7th-century unvoicing; and the [f-b] contrast after nasal was lost when /Vmf/ > /V:f/. Nevertheless, [f] and [b] still contrasted in columns 1 and 2, and [f] and [b̥] still contrasted in columns 5, 7, 9. Then, through the 8th-century voicing of voiceless spirants, all the [f]'s in columns 5 through 10 became [b]. Since these [b]'s were then in contrast neither with [f] nor with [b], their structural status

⁴³ This may be illustrated by the following sets of correspondences: Gothic *lamb*, (*silba*), *-swarb*, *land*, *skuld*, *ward*, *gahugd*, *huzd*, *lagg*, *-balg*, *baurg*; OIc. *lamb*, *siglf*, *suarf*, *land*, *skuld*, *orþ*, *-ugþ* (-*up*), *hodd*, *lang*, *belg* = [belg], *borg* = [borg].

⁴⁴ E.g. Gothic *-gulþ*, *fanþ* 'gold, found', but OIc. *gull*, *fann*; and OIc. *fiþ* 'giant' < */fīmfill/. See Noreen, *Aisl. Gram.* §§275, 298.2.

⁴⁵ For examples and dating of vowel syncope, see Noreen, *Aisl. Gram.* §§153-60.

TABLE 3. THE OLD ICELANDIC EVIDENCE

	1. C-	2. CC	3. NCV	4. NC+	5. ICV	6. IC+	7. rCV	8. rC+	9. VCV	10. VC+	11. VCs	12. VCt
/p/	pungr	uppe	hampe	hamp	hialpa	hialp	verpa	varp	grípa	greip	diúps	diúpt
/f/	fæbr	eff			↓	↓	↓	↓	↓	↓	hófs liúfs	aftr liúft
/b/					ulfar siqlfom	ulf siqlf	?	þarf suarf	hófar gefa	hóf gaf		
/t/	borg	krabbe	lambe	lamb	salta	salt	hiarta	suort	heita	hēt	gōz	(see 2)
/ð/	tunga	skattr	nenta	blint			↓	↓	↓	↓	mōþs	
	þokk											
/d/					talþa	tólþ	verþa orþom	varþ orþ	kueþa biþa	kuap baup		
/k/	dagr	kodde	lande	land	gialda	giald	hirda					
	korn	lokkr	seinka	hōnk	folkom	folk	verke	verk	auka	ok	sax	spakt
/g/						↓		↓			heilags	heilagt
					suelga	sualg	biarga	barg	draga	dag		
/g/	gestr	vagga	langa	lang								
/h/	horn				↓	↓	↓	↓	↓	↓		

was ambiguous: they could be assigned with [f] to a phoneme /f/, or equally well with [b] to a phoneme /b/.

The dental order must have gone through a similar development. Again, for the older runic period we have assumed three-way contrasts extending from column 1 through column 10. By the 7th-century unvoicing the contrast of final [p-d] (columns 4 and 6) and of final [p-ð] (columns 8 and 10) was lost, because final [d ð] thereby became [t þ]. The loss of final /a/ etc. then restored the [p-d] contrast in columns 4 and 6; but it was again lost in columns 3-6 when /nþ lþ/ > /nn ll/, and also in column 2 when /pþ/ > /tt/ or /þ/.⁴⁶ Nevertheless, the [p-d] contrast was still maintained in column 1, and the [p-ð] contrast was still maintained in columns 7 and 9. Then, by the 8th-century voicing of voiceless spirants, all the [þ]'s in columns 7 through 10 became [ð]. Since these [ð]'s were then in contrast neither with [p] nor with [d], they could be assigned either with [p] to a phoneme /p/, or with [d] to a phoneme /d/.

After both the labials and the dentals had attained this ambiguous structure, the syncope of medial vowels began to take place—first, apparently, after long syllables. At the time when *[kénniðq:, fýlliðq:] 'taught, filled' lost their medial vowels, [ð] still functioned as an allophone of the /d/ phoneme and became [d], giving *[kénndq:, fýlldq:]. The complementary distribution of [nd ld] with [rð Vð] was therefore still maintained. Presumably at this same time syncope also occurred in such forms as *[fð:ðiðq:, hírðiðq:] > *[fð:ddq:, híreddq:] 'fed, herded', with [ðð] giving [dd] in accordance with the already existing allophonic distribution.

At a somewhat later date, however, two changes took place which produced a new contrast in the language: [ð-d]. On the one hand, geminates in clusters with other consonants were simplified. The change *[kénndq:, fýlldq:] > *[kéndq:, fýldq:], OIc. *kenda*, *fýlda*, produced nothing new structurally; but the change *[híreddq:] > *[hírdq:], OIc. *hirda*, introduced a new cluster [rd] in contrast with the already existing [rð]. On the other hand, syncope now took place also after short syllables, and *[wániðq:, táliðq:] > *[wánðq: tálðq:], OIc. *vanþa*, *talþa* 'accustomed, told', producing new clusters [nð lð] in contrast with the already existing [nd ld].⁴⁷

The effect of these changes was to give an entirely new structure to the Pre-OIc. obstruents. In runic Norse (as in Gothic), [ð] contrasted with [p] but not with [d], and therefore belonged to the /d/ phoneme. Later, after [p] had become voiced in most positions, [ð] contrasted neither with [p] nor with [d], and was structurally ambiguous. Then, after the change of [rdd] to [rd] and the syncope of medial vowels after short syllables, [ð] contrasted with [d] but not with [p], and therefore belonged to the /p/ phoneme. There were still only two distinctive

⁴⁶ E.g. OE *moppe* 'moth', Gothic *aipþau* 'or', but OIc. *motte*, *epa* (with /tt/ after stressed vowel, /p/ after unstressed vowel). See Noreen, *Aisl. Gram.* §241. The cluster /pþ/ was never re-established; the First Grammarian (Haugen 27) tells us explicitly that it did not exist in his dialect.

⁴⁷ For details of these developments, and for subsequent changes, see Noreen, *Aisl. Gram.* §238.1a, b. Forms such as *vonþ*, *tolþ* (for *vaneþ*, *taleþ*) are later analogical developments; see Andreas Heusler, *Altisländisches Elementarbuch* §317 (1932).

features characterizing the phonemes /t þ d/, voice and occlusion, but the roles of primary and secondary feature had been reversed. Instead of voice, occlusion was now the primary feature: it distinguished the stops /t d/ from the spirant /þ/; and instead of occlusion, voice was now the secondary feature: it distinguished voiceless /t/ from voiced /d/. Since voice was not a distinctive feature of /þ/, this phoneme could (and did) show both voiced and voiceless allophones. In terms of our formulas, the Gothic and runic structure (t : þ) : d had changed to (t : d) : þ.

This analysis of the dentals now permits us to turn back to the labials and ascertain the structural position of the phone [b]. It belonged to the /f/ phoneme not because it contrasted with [b] (there was no such contrast), but because its dental counterpart [ð] belonged to the phoneme /þ/. By analogy with the dentals, then, the Gothic and runic structure (p : f) : b had changed to (p : b) : f. This analysis of course agrees with that of the OIc. scribes as shown by their spellings.

In the velar order no such reassignment of allophones and shift of distinctive features took place, since Pre-Old-Icelandic had (as we have seen) only two velar phonemes, /k/ and /g/. For older runic we have assumed only two allophones for /g/: the stop [g] initially, in gemination, and after nasal (columns 1-4), and the spirant [g] elsewhere (columns 5-10). A new allophone [x] was introduced by the 7th-century unvoicing, when *[swálg, bárg, dró:g] > *[swálx, bárx, dró:x] (see §3). This was lost again by the change of *[dró:x] to */dró:h/ and by the 8th-century voicing of *[swálx, bárx] back to *[swálg, bárg] (indicated by the arrows in Table 3).

The development of a new allophone [x] in Old Icelandic seems to have been a matter of analogy. When the unstressed vowels of gen. sg. masc. and neut. *[hájlāgas] and nom. acc. sg. neut. *[hájlāgatu] 'holy' were syncopated, the regular developments of [gs] and [gt] seem to have been [ks] and [kt]; cf. the OIc. spellings *heilax*, *heilakt*. By analogy with other inflectional forms, however, /g/ was reintroduced here also. The spirant was then unvoiced before /s t/, giving [héjlaxs, héjlaxt], spelled *heilags*, *heilagt*.⁴⁸

The story of the /h/ phoneme is easily told: it survived only in initial position (column 1). If there ever was a cluster /hh/ in Pre-Old Icelandic (column 2), no forms embodying it survived; the First Grammarian tells us specifically that no /hh/ existed in his dialect.⁴⁹ We have already seen in §3 that the cluster /Vnh/ became /V:h/, with long nasalized vowel (columns 3 and 4). We have also seen that the clusters /hs ht/ became /ks tt/ (columns 11 and 12). Elsewhere (columns 5-10) /h/ was lost, apparently through the same 8th-century voicing that changed [f þ] to [b ð].

As a result of all these changes, the OIc. obstruents came to have the following

⁴⁸ See Noreen, *Aisl. Gram.* §239.1b. The development is most clearly described in Heusler, *Aisl. Elementarbuch* §181.3: 'Die Formen mit *kt*, *x* stellten oft den Reibelaut (ɣ) der verwandten Formen her, und der wurde vor *t*, *s* automatisch stimmlos (χ): *heilagt*, *heilags*; *sagt*, *dags* usw. Dies waren die einzigen χ-Laute des Aisl.' Noreen's assumption of palatal and velar allophones of /g/ is not borne out by the modern evidence; see Einarrson, *JEGP* 40.40.

⁴⁹ Haugen 24-5.

structure: (p : b) : f, (t : d) : þ, k : g, s, h. The velars and /s/ and /h/ show the same structure as in runic Norse; the only changes are in the distribution of allophones ([x] now occurs before /s/ and /t/, and does not occur finally) and in the extreme limitation on the occurrence of /h/. The labials and dentals, however, show a markedly different structure. Though they have the same allophones as before ([p f b b; t þ ð d]), the voiced spirants [b ð] now belong structurally with the voiceless spirants [f þ] rather than with the voiced stops [b d]. As regards phonological structure, this means that the primary distinctive feature in each order is now occlusion and the secondary feature is voice, rather than the reverse.

6. The Old English evidence. The Old English evidence, as presented in Table 4, shows two changes that are reminiscent of Pre-Old-Icelandic: a voicing of medial voiceless spirants (indicated by the downward arrows in Table 4) and an unvoicing of final voiced spirants (indicated by upward arrows).⁵⁰ Although the dentals illustrate only the former of these changes, we may begin with this order because the resulting phonemic structure is entirely clear.

The dentals present a special structure because of a familiar phenomenon that is common to all the West Germanic languages: the spirantal allophones of PGmc. /d/ became stops, so that /d/ showed stop allophones in all positions. We may therefore assume a stage in Pre-Old-English during which the contrast [t-þ-d] existed through columns 1-10 except after nasal (where /Vnþ/ > /V:þ/, cf. Gothic *kunþs* but OE *cūþ* 'known') and after /l/ (where /lþ/ > /ld/, cf. Gothic *-gulþ* but OE *gold* 'gold'). This structure was phonologically ambiguous, since either voice (as in Gothic and runic Norse) or occlusion (as in Old Icelandic) could be considered the primary distinctive feature. That is to say, because of the absence of [ð], the system could be analyzed either as (t : þ) : d, or as (t : d) : þ.

The elimination of the spirantal allophones of /d/ of course opened the way for /þ/ to develop voiced allophones.⁵¹ When this change took place (see columns 7 and 9), the ambiguity of the phonological structure was removed. The primary feature was now one of occlusion, which distinguished the stops /t d/ from the spirant /þ/; the secondary feature was one of voice, which distinguished voiceless /t/ from voiced /d/. Since voice was not a distinctive feature of /þ/, it could (and did) show voiced allophones. The structure was now clearly (t : d) : þ.

The voicing of medial voiceless spirants affected those following a stressed vowel, but not those following an unstressed vowel.⁵² Thus the [þ]'s of dative

⁵⁰ See Karl Luick, *Historische Grammatik der englischen Sprache* (1914 ff.) §639 (voicing of medial [f þ s]), §656 (loss of medial [h]), §651 (unvoicing of final [b g]).

⁵¹ It might also be assumed that the spirantal allophones of /d/ became stops because /þ/ began to develop voiced allophones. For two reasons, however, this alternative analysis is unlikely. First, the change of [ð] to [d] presumably occurred before the Anglo-Saxon migration from the continent, since it is common to all the WGmc. dialects; whereas the voicing of medial voiceless spirants was apparently not completed at the time of our earliest OE documents. (See below under the discussion of the labials.) Secondly, final [ð] became a stop just like medial [ð], even though final [þ] did not become voiced and thereby 'push' [ð] into becoming a stop.

⁵² For the following, see Luick, *Hist. Gram.* §639.2. Other examples of voiceless [þ] following an unstressed vowel are the ordinal numerals *sēofopa*, *eahtopa*, *nigopa*, etc.

TABLE 4. THE OLD ENGLISH EVIDENCE

	1. C-	2. CC	3. NCV	4. NC+	5. ICV	6. IC+	7. rCV	8. rC+	9. VCV	10. VC+	11. VCs	12. VCt
/p/	pæp	uppan	gelimpan	gelamp	helpan	healp	weorpan	wearp	gripan	grāp	gripest	cēpte
/f/	fisc	woffian			↓	wulf self	↓	pearf swearf	↓	hōf geaf	swifst drifst	æfter
/b/					wulfas selfa	↑	? sweorfan	↑	hōfas giefan	↑		
/b/	burg	crabba	lambes	lamb						crib(b)		
/t/	tunge	sceattes	winter	brant	sealtan	sealt	heorte	sweart	hātan	þæt	bletsian	(see 2)
/p/	þanc	moppe	mōnpe	wiŋp	hælpe	fielp	↓	wearp	↓	cwæp	cwiþst	
/ð/							weorpan		cweþan			
/d/	dæg	coddes	landes	land	scyld	scyld	wordes	word	bēodan	bēad		
/c/	cēn	strecc(e)an	drenc(e)an	benc	gefylce	swelc	wyrc(e)an	wyrc	stice	pic		
/g/		lieg(e)an	seng(e)an	feng						wecg		
/k/	cēne	hnecca	drincan	dranc	folces	folc	weorces	weorc	ēacen	ēac	oxa	iecte
/x/		cohhetan				sealh swealg		mearh bearg		scōh drōg	niehsta stihst	meaht
/g/					swelgan	↑	beorgan	↑	dragan	↑		
/g/	gēs	dogga	springan	sprang								
/h/	horn				↓		↓		↓			

*[mó:naþæ, há:liþæ] 'month, health' remained voiceless, and when the medial vowels were syncopated, the new medial clusters [nþ lþ] arose: *mōnþe*, *hǣlþe* (columns 3 and 5). Final [nþ lþ] also arose through syncope in such forms as *winþ*, *fielþ* 'wins, falls' and nominative *hǣlþ* 'health' < */winnīþ, fǣlliþ, há:liþu/ (columns 4 and 6). Syncope also brought about the new clusters /ts þs/ in *bletsian* (beside *blessian*) 'bless', *blīþs* (beside *bliss*) 'bliss', and *cwiþst* 'sayest' < */bló:diso:jan, blī:þisi, kwīþis(t)/ (column 11).

In the labial order we find evidence for the same voicing of medial voiceless spirants, whereby [wúlfas, hó:fas] > [wúlbas, hó:bas] 'wolves, hooves' (columns 5, 7 [no extant examples], 9), and also for an unvoicing of final voiceless spirants, whereby [sélþ, swéarþ, jǣþ] > [sélþ, swéarf, jǣf] 'self, swerved, gave' (columns 6, 8, 10). Whatever the exact phonetics of these changes may have been, we can date them phonemically as being in the process of completion during the first half of the 8th century, since a few of the earliest documents still distinguished PGmc. medial and final /f/ and /b/ as *f* and *b*.⁵³

Before these changes, [b] was in contrast with [f] (columns 5–10) but not with [b]; hence it belonged with [b] to a phoneme /b/. After these changes, the phonological structure per se was ambiguous: [b] was no longer in contrast with [f], but it was also still not in contrast with [b]. Given the relationship of [b] and [f] in columns 5–10, any modern analyst would probably assign them both to the same phoneme in order to keep the morphophonemics as simple as possible, i.e. analyze the singular and plural forms as /wúlþ, wúlfas/ rather than as /wúlþ, wúlbas/, etc. More important, however, is the fact that in the dental order the voiced spirant [ð], corresponding to labial [b], necessarily belonged with [b] rather than with [d]. As was the case with the OIc. labials, it is therefore the analogy of the dentals which leads us to assign [b] and [f] to the same phoneme, and to assume that the older structure (p : f) : b had changed to (p : b) : f. This is of course the analysis made by the OE scribes, as is shown by their spellings *wulf*—*wulfas*, *self*—*selfa*, etc.

A number of minor points need special mention. The inherited cluster /fs/ perhaps became /ps/ in the Pre-OE period: cf. *ræpsan* 'reprove', though early documents also show *ræfsan*.⁵⁴ The clusters /ps pt fs/ (columns 11 and 12) also arose through vowel syncope: *grīpst*, *cēpte*, *swīfst*, *drīfst* 'graspest, kept, sweeppest, drivest' */grī:pis(t), kó:pidæ, swī:fis(t), drī:bis(t)/.

⁵³ E.g. *Epinal Glossary* 223 *giroefa* 'officer', 183 gen. *uulfes* 'wolf', 142 *uuf* 'owl', 192 *obaer* 'over', 51 *halbae* 'halves', 577 *staeb* 'staff'; Henry Sweet, ed., *The oldest English texts* (1885 = *Early English Text Society*, Vol. 83). Cf. Eduard Sievers, *Altangelsächsisch F und B*, *Beiträge z. Gesch. d. dt. Spr. u. Lit.* 11.542–5 (1886), and *Anglia* 13.15–6 (1891). Luick, *Hist. Gram.* §639 Anm. 4, §658, dates the voicing of medial [f s þ] as 'spätestens im 6. Jahrhundert', and interprets the *f*-*b* spelling contrasts of the early documents as representing labiodental vs. bilabial voiced spirants. However this may be, it is clear that the phonemic merger of /f/ and /b/ had not been completed at the time of these documents, but that it was completed shortly thereafter. This point is nicely made in Herbert Penzl, A phonemic change in early Old English, *Lg.* 20.84–7 (1944).

⁵⁴ Cf. Luick, *Hist. Gram.* §634.2; Eduard Sievers (ed. Karl Brunner), *Altenglische Grammatik* §193.3 (1942). For a different view (*ræfsan* < */ráfisjan/, *ræpsan* < */rábisjan/), see Albert Morey Sturtevant, *Lg.* 7.191 (1931).

One further form needs interpretation: column 10 *crib(b)* 'crib' < */kríbbu/ < PGmc. */kríbjō:/. When the earlier [bb] in forms of this type came into final position through the apocope of the final vowel, the long stop seems to have been shortened but to have kept its stop articulation, thus introducing [b] into a new environment. In all words of this type the OE scribes wrote indiscriminately in final position both single and double letters (though always *cg*; see below). The double spellings were almost certainly taken from forms where the consonant really was phonemically long (*cribbe* etc.); the single spellings therefore represent the actual phonemics: *crib* = /kríb/ = [kríb].⁵⁵

PGmc. /k g h/ underwent even more extensive restructuring in Pre-OE times than did the labials and dentals. The most striking development was the palatalization of /k/ and /g/ in the neighborhood of front vowels.⁵⁶ This change must have begun phonetically before the migrations from the continent, since it is shared by continental dialects (Frisian). For Pre-Old-English we may assume a phoneme /k/ with palatal allophones in such forms as */strákkjan, drákkjan, bánki, ké:na/ 'stretch, submerge, bench, torch', but velar allophones in such forms as */hnékkō:, drínkan, dránc, kó:ni/ 'neck, drink, drank, bold'. Still in Pre-OE times, after umlaut had taken place, postconsonantal /j/ and final /i/ were lost, so that the palatal and velar allophones came to contrast with one another and hence split into two separate phonemes: in the environment /V()V/ as /stréccan/ vs. /hnécca/; in the environment /C()V/ as /drénčan/ vs. /drínkan/; and in the environment /C()+/ as /bénč/ vs. /dránc/. Cf. OE *strecc(e)an, hnecca, drenc(e)an, drincan, benc, dranc*. In early OE times, after /o/ and /o:/ had been umlauted to /ö/ and /ö:/ and then unrounded in most dialects to /e/ and /e:/, /č/ and /k/ came to contrast also in the environment /+()V/ as /čé:n/ vs. /ké:ne/, OE *cēn* 'torch' and *cēne* 'bold'.⁵⁷

The development of Pre-OE /g/ was more complicated than that of /k/ because it had both stop and spirant allophones. The evidence indicates that it was a stop in only three positions: initially before consonants and back vowels,⁵⁸

⁵⁵ This is the usual interpretation; cf. Sievers-Brunner, *Ae. Gram.* §231.1. Luick, *Hist. Gram.* §§625.2, 631.1, 644.1, states that geminates which came into final position thereby lost their geminate quality, but still remained long.

⁵⁶ For details, see Luick, *Hist. Gram.* §637.

⁵⁷ To indicate the OE palatal stops, I use the traditional symbols *č* and *ǵ*; the exact phonetic quality is uncertain. For an excellent analysis of palatalization in Old English, see Herbert Penzl, The phonemic split of Germanic *k* in Old English, *Lg.* 23.34-42 (1947). Penzl says that the 'crucial innovation' in the phonemic split of /k/ into /č/ and /k/ was 'the appearance of velar [k] before the new palatal vowels developed through i-umlaut' (42). This would mean that the phonemic split did not take place until early OE, when /ö/ and /ö:/ were unrounded in most dialects and thus merged with inherited /e/ and /e:/. However, if my own analysis is correct, the 'crucial innovation' was the loss of /j/ and final /i/, which put the [č] of [stréccan, bénč] in contrast with the [k] of [hnécca, dránc]; and this happened before the time of the earliest documents.

⁵⁸ E.g. Pre-OE */gró:nja, gó:si/ > *grēne, gēs* 'green, geese'. The evidence that OE initial *g*- plus back vowel or consonant represented a stop is inconclusive; it could conceivably have been a velar spirant. I assume stop articulation because that is what we find as far back as we can trace the pronunciation from modern times. Luick's assumption (*Hist. Gram.* §§633, 696) that both velar AND palatal *g*, including *g* < /j/ (!), were stops initially, leaves me quite unconvinced. For the more generally accepted view (which I follow), see Sievers-Brunner, *Ae. Gram.* §206.8.

in gemination, and after /n/. In the latter two positions we can assume for Pre-Old-English a palatal stop allophone in such forms as */lǣggjan, sǣngjan, fǣngi/ 'lie, singe, grasp', but a velar stop allophone in such forms as */wǣggo:, sprǣngan, sprǣng/ '(ear)wig, jump, jumped'. As in the case of the voiceless stop, it was the loss of postconsonantal /j/ and final /i/ which later brought the palatal and velar allophones into contrast with one another and hence split them into two separate phonemes: in the environment /V()V/ as /lǣggan/ vs. /wǣgga/; in the environment /C()V/ as /sǣngan/ vs. /sprǣngan/; and in the environment /C()+/ as /fǣng/ vs. /sprǣng/. Cf. OE *lǣg(e)an*, (*ǣar*)*wicga*, *seng(e)an*, *springan*, *feng*, *sprang*.

In all other environments Pre-OE /g/ was a spirant. We can assume a palatal allophone in such forms as */wǣ:gi/ 'wave' (OE *wǣg*) and a velar allophone in such forms as */dǣ:ga/ 'dough' (OE *dǣg*). What apparently caused them to split was not that they came to contrast with one another (though this would eventually have happened), but rather that the palatal allophone merged with inherited /j/: OE *wǣg* /wǣ:j/ < */wǣ:gi/ 'wave' like *wǣg* /wǣ:j/ < */wǣ:ju/ 'wall', or initially *gǣafon* /jǣ:fon/ < */gǣ:bun/ 'they gave' like *gǣar* /jǣ:r/ < */jǣ:ra/ 'year'. The velar allophone continued as [g] in Old English, but was of course unvoiced in final position: pl. *dǣgas* [dǣ:gas], sg. *dǣg* [dǣ:x] 'dough'. That [g] and [j] were in contrast is clear from such forms as *beorgas* 'hills' with [g] but *hergas* (*herias*, *herigas*, *herigeas*) 'armies' with [j].

We have seen how the stop allophones of PGmc. /g/ split into OE palatal /g/ and velar /g/, and how the spirantal allophones split when the palatal one merged with inherited /j/. We must now ask: was the original complementary distribution of stop and spirant allophones also destroyed? That is, can we demonstrate a contrast between [g] and [g], and between [g] and [j]?

Let us examine first [g] and [j]. We find [g] in gemination, between nasal and vowel, and finally after a nasal (columns 2, 3, 4). Since [j] did not occur in any of these positions, it would be possible to analyze *lǣg(e)an*, *seng(e)an*, *feng* as /lǣjjan, sǣnjan, fǣnj/. On the other hand, both [g] and [j] seem to have occurred finally after a vowel (column 10) and to have contrasted in this position: *wecg* /wég/ 'wedge' vs. *weg* /wéj/ 'way'. The only question about this contrast is the interpretation of the spelling *cg*: should it perhaps be analyzed as long [gg], so that *weg* could be /wéj/ and *wecg* could be /wéjj/? The answer to this question has already been given, I believe, in the discussion of such spellings as *crib(b)*: Old English had no long consonants in final position. The spellings *cribb*, *mann*, *eall* 'crib, man, all' could alternate freely with *crib*, *man*, *eal*, since neither spelling created any ambiguity. On the other hand, a spelling such as *wecg* could not alternate with *weg*, since this latter form would be ambiguous. Hence *cg* was retained in final position. It did not indicate a long consonant, but merely the phone [g] as opposed to [j]. The loss of final vowels and the subsequent shortening of long final consonants were therefore the changes which brought [g] and [j] into contrast with one another.

There remains the relationship between [g] and [g]. As Table 4 shows, they were nowhere in contrast with one another and could therefore be analyzed as allophones of a single phoneme. But for a proper understanding of their structural relationship we must first examine the history of [x] and [h] in Old English.

Whatever the status of [x] and [h] may have been in Proto-Germanic, Pre-Old-English seems to have had velar [x] finally and before voiceless consonants (/s/, /t/, and later /p/), but to have had glottal [h] initially and between voiced phonemes medially.⁵⁹ The proof of glottal (or at least non-velar) articulation medially is the same as in Pre-Old-Icelandic: the voicing of medial voiceless spirants changed [f þ] to their voiced counterparts [b ð]; it did not change [h] to velar [g], but caused its loss; therefore [h] was not velar but something else—presumably glottal. The same early documents which show medial *f* in contrast with *b* also show medial *h*: Epinal Glossary 3 *thohae* 'clay', 1066 dat. pl. *uulohum* 'ornaments', 884 dat. pl. *furhum* 'furrows', etc.; therefore /h/ still occurred at this time, either as voiceless or—if voicing had already taken place—as voiced [h]. By the time of the later documents it had been lost: *þō*, *wlō(u)m*, *fūrum*. In initial position we find the same development: Pre-OE */fri: + hals/ > */fri: hals/ > OE *frīols* 'freedom'. Here also the spirant cannot have been [x], but was presumably the same [h] that survived through Middle English into Modern English.

The proof of velar rather than glottal articulation in other positions is easily given. In gemination and before /t/, OE *h* survives as [x] in Middle English: OE *cohhetan*, *hlæhhan*, *dohtor*, *meaht*, ME *coughen*, *laughen*, *doughter*, *might* 'cough, laugh, daughter, might'. In final position it not only survived as ME [x], but merged in OE times with final unvoiced [g]: OE *rūh* and *rūg*, *þurh* and *þurg*, ME *rough*, *thurgh* 'rough, through'. Whether [x] or [h] occurred before /s/ we cannot know, since it was dissimilated to /k/: Gothic *auhsa*, OE *oxa/óksa* 'ox'.⁶⁰

Having established the approximate sound values of these phones, we may now turn to their phonemic status. Since [h] was not distinguished by the feature of velar articulation, it was a separate phoneme /h/. It occurred only in the environments of columns 1, 5, 7, 9. In position 1 it survived through Old English: *horn* 'horn'; in positions 5, 7, 9 it was lost during early OE times, apparently as part of the general voicing of medial voiceless spirants: OE genitives *sēales*, *mēares*, *scōs* 'willow, horse, shoe' < */séalhes, méarhes, scó:hes/, cf. the nominatives *sealh*, *mearh*, *scōh*.

Before the unvoicing of final [g], we may assume a stage during which [x] and [g] were in contrast: */séalx, méarx, scó:x/ 'seal, horse, shoe' vs. */swéalg, béarg, dró:g/ 'swallowed, protected, drew' (columns 6, 8, 10). Since [g] was in contrast with [x] but not with [g], it clearly belonged with [g] to a phoneme /g/, and [x] was a separate phoneme /x/. The Pre-OE phonological structure of the velars was therefore (k : x) : g. After the unvoicing of final [g], however, the phonological structure per se was (as with the labials) ambiguous: [g] was no longer in contrast with [x], but it was also still not in contrast with [g]. Again,

⁵⁹ Cf. Luick, *Hist. Gram.* §636; Sievers-Brunner, *Ae. Gram.* §§217–23. It is possible that a medial [x] still existed at the time of 'breaking', but changed to glottal [h] afterwards. This would give the following sequence of changes: */séxan > séoxan > séohan > se:on/, OE *sēon* 'see'. However, this is slim evidence indeed, since there is no reason why 'breaking' could not have taken place before both [x] AND [h].

⁶⁰ The cluster [xs] was later restored in such forms as *nēhsta* 'next' and *stīhst* 'climbest' (column 11); cf. Sievers-Brunner, *Ae. Gram.* §214.1 Anm. 2, §222. The cluster /kt/ in column 12 also arose as a result of vowel syncope: */é:akidæ/ > *īecte* 'increased'.

given the relationship of [x] and [g] in columns 5-10, any modern analyst would probably assign them to the same phoneme in order to keep the morphophonemics as simple as possible. He could not avoid the morphophonemic alternations of /sé:ales—séalx, mé:ares—méarx, scó:s—scó:x/; but he could certainly choose the analysis /swélxan—swéalx, béorxan—béarx, dráxan—dró:x/ in preference to /swélgan—swéalx, béorgan—béarx, drágan—dró:x/. Even more important than this, however, would be the fact that in the dental order the voiceless and voiced spirants [p̥ ɸ] necessarily belonged together as allophones of a phoneme /p/. It is again the analogy of the dentals which leads us to assign [x] and [g] to the same phoneme, and to assume that the older structure (k : x) : g had changed to (k : g) : x. All three orders then show the same structure: a primary feature of occlusion, distinguishing the stops /p b, t d, k g/ from the spirants /f ɸ x/; and a secondary feature of voice, distinguishing voiceless /p t k/ from voiced /b d g/. The three spirantal phonemes, not having voice as a distinctive feature, can and do show both voiceless and voiced allophones.

Our structural analysis of the labials and dentals agrees with the analysis of the OE scribes as shown by their choice of symbols. With the velars this is no longer true. For one thing, the scribes used an alphabet that was hopelessly inadequate: it had only the three symbols *c, g, h* (disregarding the rare uses of *k*) to use for the seven phones [č ǵ k x g h] which we have analyzed into the six phonemes /č ǵ k x g h/. Then too, the lack of contrast between [g], [ǵ], [x], and [h] in columns 3-12 of our table made it possible to use either *g* or *h* quite unambiguously for any of these four phones in any of these ten positions, and the scribes took full advantage of this opportunity.⁴¹ The stop [g] was written quite consistently with the letter *g*, but the spirants [ǵ] and [x] were written with both *g* and *h*. For a while *g* predominated for [ǵ], and the choice of *h* or *g* for [x] was governed largely by morphophonemic convenience: *ealh*, *mearh*, *scōh* usually with *h*, but *swealg*, *bearg*, *drōg* usually with *g* because of the related *swelgan*, *beorgan*, *dragan*, etc. Later on the scribes began to write [x] more or less consistently with *h*, regardless of morphophonemics, and continued the use of *g* for [ǵ] and [g].

With the phonemic status of [g] established, we can rapidly complete our analysis of the OE stops and spirants. The PGmc. contrast /s-z/ was of course lost when /z/ merged with inherited /r/: Gothic *maiza* but OE *māra* 'more'. The way was then open for /s/ to develop voiced allophones, and it did this at the time of the general voicing of medial spirants. This is indicated by the modern reflexes of /s/, and also by the *-de* of such forms as *līesde*, preterit of *līesan* 'set free', as against the *-te* of *cyste*, preterit of *cyssan* 'kiss'. Thus all of the OE spirant phonemes (except /h/, which had been lost in voiced surroundings) showed both voiceless and voiced allophones, in complementary distribution.

Our analysis has led us to set up /č/ and /ǵ/ as some sort of palatal phonemes (palatal or palatalized stops or affricates), with no spirantal counterpart; and

⁴¹ The following description gives only the broad outlines; no attempt is made to trace the detailed usage of *g, h, gh, hg, ch* for the spirants, and *g, c, cg, gc* for the (palatal and velar) stops. Cf. Sievers-Brunner, *Ae. Gram.* §211-23. I use the letter *g* to represent the special OE symbol.

TABLE 5. THE OLD SAXON EVIDENCE

	1. C-	2. CC	3. NCV	4. NC+	5. ICV	6. IC+	7. rCV	8. rC+	9. VCV	10. VC+	11. VCs	12. VCt
/p/	péda	uppan	krampo	?	helpan	halp	uuerpan	uuarp	gripan	diop	hripsod	dópte
/f/	fisk	aheffian			↓	uulf* self	↓	tharf huarf	↓	hof gaf	lofsamost	after
					uuuos selbo	↑	? huerban	↑	hobos geban	↑		
/b/	burg	libbian	umbi	lamb								
/t/	tunga	scattes	uuintar	unt	salte	salt	herta	suart	hétan	hēt	blitzea	(see 2)
/p/	thanc	ettho					↓	uuard	↓	quad		
			giménthon		sálda		uuerðan		quethan			
/d/	dag	biddian	landes	land	geldan	geld	uuoerde	uuord	-biodan	bód		
/k/	corn	likkodun	drincan	dranc	folkes	fole	uuerke	uuerc	ócan	óc	écan	
/x/		hlahhian*				balg		burh		noh mah	sehs	mahtig
	gast				belgan	↑	burgo	↑	mugan			
/g/		liggian	sprungun	sprang								
/h/	horn				-fel(a)han	-fal(ah)	fer(a)he	fera(h) ferh	se(h)an	hó(h)		

to set up /s/ as a spirant with no stop counterparts. If we could group these together as (è : g) : s, we would have a perfect parallel to the labial, dental, and velar orders. To do this, however, it would be necessary to establish some distinctive feature (say, palatal articulation) common to all three of them. Since the subsequent development of these phonemes indicates that there was no such feature, we cannot assign them to a single order. The OE stops and spirants therefore seem to have had the following structure: (p : b) : f, (t : d) : þ, (k : g) : x, è : g, s, h.

7. The Old Saxon evidence. The Old Saxon evidence in Table 5 shows an obvious resemblance to the OE material that we have just examined, except for the lack of a palatal order.⁶² Again we find evidence for a voicing of medial voiceless spirants (the downward arrows in columns 5, 7, 9) and for an unvoicing of final voiced spirants (the upward arrows in columns 6, 8, 10).⁶³ Since the irregular spelling of the OS documents somewhat obscures these changes, we may cite some brief evidence to make them clear.

The voicing of medial spirants in the /h/ row raises special problems (which we shall take up later); in the dental order it is obscured by the indiscriminate use of *ð*, *th*, *d* both medially and finally. But the labial order shows it clearly in spellings with *ð*, *b*, *u* medially, as against the prevailing use of *f* finally: *uulbos* M,⁶⁴ *uuluos* C < */wulfans/ acc. pl. 'wolves'; *hobos* MC, *hobos* C < */hófans/ acc. pl. 'courts'.

The unvoicing of final voiced spirants did not affect the dental order since, as in all WGmc. dialects, the spirantal allophones of PGmc. /d/ had become stops. However, unvoicing is clear in both the labial and velar orders from such spellings as: *self* MCV (the usual spelling), *selb* M < */sélba/ 'self'; *huarf* MC (the usual spelling), *huarð* C < */hwárð/ 'went'; *fargaf* MC (the usual spelling), *-gab* M, *-gab* C < */-gáb/ 'forgave'; *burg* MC (the usual spelling), *burh* C < */[búrg/ 'city'; *mag* MCG (the usual spelling), *mah* C, *mahg* G < */mág/ 'can'; etc.

In attempting an analysis of the OS evidence we may begin with the dental order, since again (as in OIc. and OE) it shows a structure that is entirely clear. After the spirantal allophones of PGmc. /d/ had become stops, we may assume for Pre-Old-Saxon the same stage as for Pre-Old-English: a [t-þ-d] contrast throughout columns 1-10 except after nasal (where /Vnþ/ > /V:þ/, cf. Gothic

⁶² Except as noted below, all forms are from Edward H. Sehr, *Vollständiges Wörterbuch zum Heliand und zur altsächsischen Genesis* (1925). The form *uulf** 'wolf' (column 6) is assumed on the basis of Heliand *uulbo*, *uuluo* (gen. pl.) and *uulbos*, *uuluos* (acc. pl.). The infinitive *hlahhian** 'laugh' (column 2) is assumed on the basis of Heliand pret. *hlōgun* and ppl. *bihlagan*, and MLG *lachen*. Cf. Agathe Lasch, *Mittelniederdeutsche Grammatik* §351 Anm. 2 (1914). The forms *krampo* 'hook' [hri]psod 'reproves', and *unt* 'up to' are from Elis Wadstein, *Kleinere altsächsische Sprachdenkmäler* (1899).

For an analysis of the graphemes and phonemes of the Heliand and Genesis manuscripts, see Carl Richard Page, *The phonological system of the Old Saxon language*, Cornell University M.A. thesis (typescript; Ithaca, 1952).

⁶³ See F. Holthausen, *Altsächsisches Elementarbuch* (1921) §197 (voicing of [f]), §206 (voicing of [þ]), §223 (unvoicing of [b]), §234 (unvoicing of [g]).

⁶⁴ The abbreviations M, C, V, P, G refer to the Munich, Cotton, Vatican, and Prague manuscripts of the Heliand, and to the Genesis.

kunþs but OS *cūð* 'known') and after /l/ (where /lþ/ > /ld/, cf. Gothic *-gulf* but OS *gold* 'gold'). Since either voice or occlusion could be considered the primary distinctive feature, this structure was phonologically ambiguous and could be analyzed as either (t : þ) : d or (t : d) : þ. The way was open for /þ/ to develop voiced allophones, and when this change took place (columns 7 and 9), the ambiguity was removed: the system was now clearly (t : d) : þ.

Vowel syncope re-introduced the clusters /n/ and /l/ plus dental spirant when */ga+májniþo:, sá:liþo:/ became *gimēnitha, sālða* 'community, blessedness'. Presumably this /þ/ was (or became) voiced [ð], since it later on became /d/ (as, for that matter, /þ/ did eventually in all environments). Vowel syncope also introduced the new cluster /ts/ when */blí:þisjo:/ > /blí:tsja/, spelled *blidsea, blitsea, blizza*.

The labials also underwent the same development as in Pre-Old-English. Before medial voicing and final unvoicing had taken place, [f] and [b] were in contrast in the environments of columns 5-10. Since [b] contrasted with [f] but not with [b], it clearly belonged with the latter to a phoneme /b/, and the structure of the labial order was (p : f) : b. After medial voicing and final unvoicing had taken place, [f] and [b] were no longer in contrast, but [b] was also still not in contrast with [b]. The structure could therefore be analyzed as either (p : f) : b or (p : b) : f. Again, any modern analyst would probably choose the latter interpretation, both in order to keep the morphophonemics as simple as possible and especially because of the analogy of the dentals. As for the scribal spellings, since [f b b] were not in contrast in any of the environments of columns 3 through 12, any one of the letters *f, ð, b, u* would do equally well—and the scribes used all of them in nearly all positions. (The symbol *ð* is pretty much limited to the *Heliand* and *Genesis* manuscripts.) The fact that *b* could be, and was, written unambiguously for the labial spirant in columns 5-10 may perhaps explain why in the dental order *d* was also written for /þ/ (thus very commonly in M), even though here it contrasted with the stop which was also written *d*.

The cluster /pt/ in column 12 was the result of vowel syncope: */dáwpido:/ > *dopte* M, *dopta* CP 'baptized', beside unsyncopeated *dopida* C. The form *lofsamost* 'most praiseworthy' given in column 11 for /fs/ is uncertain, since it may perhaps be /lóf+sámost/.

In the velar order PGmc. /k/ appears as OS /k/. If it was palatalized before a following front vowel (as some spellings seem to indicate⁶⁵), this was apparently allophonic and not phonemic. The cluster /ks/ (column 11) arose when the [g] of */ájgisan/ was unvoiced and dissimilated to *écsan* (acc.) 'owner'. This much seems quite clear; the remaining velars and /h/ are more difficult to analyze.

We may first look for evidence of inherited [x] in Old Saxon. The form *hlahhian** < PGmc. */hláxjanan/ 'laugh' (column 2) is not recorded in any manuscripts, but we may set it up on the basis of MLG and modern *lachen* and assume that the spirant was phonetically [xx]. Simple [x] seems to have occurred before /t/ (column 12) and finally after short stressed vowel (column 10), since it survives

⁶⁵ See Holthausen, *As. Elementarbuch* §242.

in Middle Low German as a palatovelar spirant in these positions:⁶⁶ OS *mahtig* (the usual spelling), *magtig* C 'mighty'; *noh* (the usual spelling), *nog* M 'still'; *gisah* (the usual spelling), *gisach* G 'saw'; MLG *mechtich*, *noch*, *sach*. (The spellings with *g* for [x] of course reflect the unvoicing of [g] to [x].) Probably the spelling *hs* (column 11) also indicates [xs], but this is obscured by the late OS change of this cluster to /ss/: OS *uuahsan* 'grow', MLG *wassen*.

In these positions, then, PGmc. /x/ seems to have given velar [x] in Old Saxon. Elsewhere the evidence shows that it gave something other than [x], presumably glottal [h]. We again have the fact that when voicing changed medial [f b] to their voiced counterparts [b ð], it did not change the reflexes of PGmc. /x/ to [g]; hence these reflexes cannot have been velar at the time of the voicing. Medially where voicing took place (columns 5, 7, 9) and finally after consonants, long vowels, and unstressed short vowels (columns 6, 8, 10), we find OS spellings with and without *h*. Perhaps the /h/ was medially voiced and finally voiceless. In any case, it was quite clearly in the process of being lost in these positions, and it was completely gone by MLG times.⁶⁷ There remains an interpretation of initial *h*. It was presumably glottal [h], since that is what it is now in Low German before vowels (before consonants it was lost by MLG times), and there is no spelling evidence to indicate that it has ever been anything else.

PGmc. /g/ seems to have given a stop in Old Saxon in gemination and after nasal (columns 2-4); this can be deduced from spellings with *c*, *ck*, *gk*, *kk* in the smaller documents, as well as from the MLG spellings.⁶⁸ In other environments it gave a spirant, and before front vowels and /j/ this spirant became palatal and then (as in Old English) merged with inherited /j/.⁶⁹ This seems clear from the spelling system which the scribes adopted. They wrote /j/ before /i e/ as *g*, rarely *i*; they wrote /g/ = [g] before /a o u/ as *g*; and they wrote /j/ before /a o u/ as either *i*, *gi*, or *ge*. Examples of /ji je/: *gi(h)id* < */jʰid/ 'says', *gibid* < */gʱibid/ 'gives', *ge(h)an* < */jéhan/ 'say', *geban* < */géban/ 'give', and also *tôgid* < */(a)táwgid/ 'shows', *tholoie* < */pólo:je:/ 'suffer' (3rd sg. pres. subj.). Examples of /ga go gu/: *gast* 'guest', *gold* 'gold', *gumo* 'man', and also *daga*, *dago*, *dagun* 'day' (dat. sg., gen. pl., dat. pl.).⁷⁰ Examples of /ja jo ju/: *(g)iac* 'also', *(g)iordan* 'Jordan', *(g)iungaro* 'disciple', and also *thrêgian* < */þrájjan/ 'threaten', *uuêgian*, *uuêgean* < */wájgian/ 'torment', *fêgion*, *fêgiun* (dat. pl.) < */fájgjum/ 'fated', *sāiu* < */sá:ju/ 'I sow', *tô(g)iu* < */(a)táwgju/ 'I show'.

The proof that PGmc. /g/ gave a velar spirant initially before consonants and non-front vowels (column 1), medially before non-front vowels (columns 5, 7, 9),

⁶⁶ See Lasch, *Mnd. Gram.* §350.

⁶⁷ See Lasch, *Mnd. Gram.* §351. Holthausen, *As. Elementarbuch* §213, assumes that *h* represented [x] finally in all environments and not just after stressed short vowel. This is unlikely, as he shows in §214 by listing the many forms in which it was not written finally after consonant, long vowel, or unstressed short vowel.

⁶⁸ See Holthausen, *As. Elementarbuch* §§252, 252a; Lasch, *Mnd. Gram.* §§343-4.

⁶⁹ This is certainly what the OS spellings seem to indicate, though it does not agree entirely with the evidence of MLG. See Lasch, *Mnd. Gram.* §§341-2, 349.

⁷⁰ /g/ apparently also in the variant dative spelling *dage* and in genitive *dages*.

and finally (columns 6, 8, 10) is based partly on the OS spellings, partly on later evidence. For initial position there is no OS spelling evidence at all; the assumption of a spirant is based on the evidence of Middle Low German and of the modern dialects.⁷¹ For medial position there is also only the later evidence, but in final position the OS spellings are quite clear. Usually the unambiguous spelling *g* was used both medially and finally: *belgan-balg* 'be angry', *burgo-burg* 'city', *mugan-mag* 'can'. Variant spellings, however, point to the unvoicing of [g] to [x]: *burh* C, *mah* C, *mahg* G, as well as *genoh* C 'enough', etc.⁷² That the letter *g* was used finally for voiceless [x] is also clear from the spellings *nog* < */nóx/ etc. mentioned above.

We have justified the phonetic assignments of the velars and of [h] in Table 5; there remains the matter of their phonemic assignment. Initial *h* and the medial and final *h*'s that vary with zero must make up a separate (presumably glottal) phoneme, since we have seen that they did not share the distinctive feature of velar articulation. For the remaining four phones [k x g] we must set up the three phonemes /k x g/ to take care of the three-way contrast in column 2. All instances of [k] clearly belong to the phoneme /k/, all instances of [x] to the phoneme /x/, and all instances of [g] to the phoneme /g/. The status of [g] is ambiguous per se, since this contrasts neither with [x] nor with [k]. Again, however, morphophonemic convenience and the analogy of the dentals will probably persuade us to assign them to /x/. The scribes were able to write *g* for [x g] quite unambiguously in columns 3-12 since the three phones did not contrast in these positions. The only ambiguous spellings they used were such forms as *burh* and *ferh*, beside *burg* and *fera(h)*, which obscured the contrast /búrx/ vs. /fér(a)h/.

We may complete our analysis of the OS stops and spirants with brief mention of the phoneme /s/. As in Old English, after PGmc. /z/ had merged with inherited /r/ (Gothic neut. *maizo*, OS *mêra* 'more'), the way was open for /s/ to develop voiced allophones. It did this at the time of the general voicing of medial voiceless spirants. Proof of the voicing comes from the modern reflexes of /s/, and also from the *-da* of such an OS form as *lôsdā*, preterit of *lôsian* 'set free', as against the *-ta* of *custa*, preterit of *cussian* 'kiss'. This means that all of the OS spirant phonemes (probably including the /h/, soon to be lost, of *felhan*, *sehan*, etc.) showed both voiceless and voiced allophones in complementary distribution.

Our analysis has given the following structure for the OS stops and spirants: (p : b) : f, (t : d) : þ, (k : g) : x, s, h. We find in this the same reversal of distinctive features as in Old Icelandic and Old English: voice and occlusion continue to function as distinctive features, but occlusion is now the primary feature and voice the secondary one.⁷³

⁷¹ See Lasch, *Mnd. Gram.* §342, who also assumes an initial voiced spirant for Old Saxon. Holthausen, *As. Elementarbuch* §251, assumes an initial stop for all of West Germanic; this is hard to reconcile with the change of /g/ to /j/ before front vowels in Old English, Old Frisian, and Old Saxon, and with the widespread modern spirants before vowels and consonants (e.g. in Westphalian and Dutch).

⁷² For similar evidence in the smaller documents, see Holthausen, *As. Elementarbuch* §234.

⁷³ To complete the picture of the Old Saxon stops and spirants we must add one further

8. The Old High German evidence. To give a full picture of the OHG stops and spirants, and of their development from Proto-Germanic, would go far beyond the scope of this paper. The effects of the HG sound shift—even disregarding dialect differences—were so sweeping that they would have to be treated in a separate study. Furthermore, because they were so sweeping, the OHG evidence is of little help in determining the structure of the stops and spirants of Proto-Germanic. We may therefore limit ourselves here to a few tentative remarks. They will apply only to Alemannic and Bavarian, the dialects in which the HG sound shift was most far-reaching.⁷⁴

The first question to be settled is the status of the reflexes of PGmc. /x/.⁷⁵ In gemination and before /+ s t/ it seems to have given a velar spirant: OHG (h)lahhen 'laugh', sah 'saw', sehs 'six', maht 'might', MHG lachen, sach, sechs, macht. Elsewhere it gave glottal [h], which is still preserved initially before vowels, but was lost in other positions at various times in nearly all dialects. We may therefore assume for OHG a phone [x] belonging to the velar order, and a glottal spirant /h/.

We have already seen how the WGmc. change of all [ð] allophones to [d] gave an ambiguous structure [t—b—d], in which either voice or occlusion could be analyzed as the primary distinctive feature. In Pre-OHG all instances of [b g] also became stops, so that all three orders then showed the same ambiguous structure: [p—f—b, t—b—d, k—x—g]. This ambiguity seems to have been removed in Pre-OHG not by a general voicing of medial voiceless spirants (as in early Old English and in Pre-Old-Saxon), but by a voicing of /p/ first medially and then also initially and finally.⁷⁶ We may therefore assume for this stage of Pre-OHG a structure (p : b) : f, (t : d) : þ, (k : g) : x, in which occlusion was the primary distinctive feature and voice the secondary one.

The next step which we may assume—and it was the first phonetic step in the HG sound shift—was the development of aspirated allophones for /p t k/ in all the environments where they contrasted with /b d g/: [páð, ópan] > [p'áð, óp'an] 'path, open'; [téhan, étan] > [t'éhan, ét'an] 'ten, eat'; [kórñ, máko:n] > [k'órñ, mák'o:n] 'grain, make'; etc. This hypothetical (though generally accepted) step affected allophones only, but it opened the way for a change in the system of distinctive features. Since /p—b, t—d, k—g/ were now opposed not

detail which affects the distribution of phonemes and distinctive features, though not the basic phonological structure. Table 5 indicates a contrast in final position between voiceless and voiced stops: *salt-geld, dranc-sprang*. Variant spellings such as *dump* for *dumb*, *dot* for *dod*, and also *hold* for *holt*, *uwilld* for *uwillt*, indicate that this contrast was being given up (or perhaps had already largely been given up) during OS times. See Holthausen, *As. Elementarbuch* §§246, 248, 252, and 238 Anm. 1. The contrast was of course entirely gone by MLG times; see Lasch, *Mnd. Gram.* §227.

⁷⁴ In making the following analysis of Old High German I have benefited greatly from some spirited correspondence with W. Freeman Twaddell.

⁷⁵ For the following, see Otto Behaghel, *Geschichte der deutschen Sprache* 404–7 (1928).

⁷⁶ See Wilhelm Braune (ed. Karl Helm), *Althochdeutsche Grammatik* §166 (1936). Primus Lessiak, *Beiträge zur Geschichte des deutschen Konsonantismus* 130 (1933), considers the voicing of /p/ to be parallel to that of /f/ (59–70) and /s/ (79, 95). However, the voicing of these latter two spirants seems clearly to have occurred during the OHG period, whereas the voicing of /p/ just as clearly began before the OHG period.

only as voiceless vs. voiced, but also as aspirated vs. unaspirated, the way was open for /b d g/ to develop voiceless (lenis) allophones. The development of these voiceless allophones (in all positions in Alemannic and Bavarian) then brought about the first structural step in the OHG sound shift: voice was replaced by release as the secondary feature distinguishing /p t k/ from /b d g/.⁷⁷ Presumably the aspiration of /p t k/ and the unvoicing of /b d g/ occurred simultaneously; the above sequential description is only for ease of presentation.⁷⁸

At this stage the structure was still (p : b) : f, etc., but the feature opposing /p t k/ to /b d g/ was release rather than voice; and /b/ was probably voiced in at least some environments. All of these phonemes also occurred in gemination, partly as geminates inherited from Proto-Germanic and partly as a result of the WGmc. consonant doubling. Then, still in the Pre-OHG period, two changes occurred which brought about a complete restructuring of the stops and spirants.

One change was that of the unaspirated geminates /bb dd gg/ to fortis stops. After short vowels the OHG scribes wrote the reflexes of these clusters with double letters: *crippa*, *hutta*, *bruca* < */kríbbja, húddja, brúggja/ 'crib, hut, bridge'. After long vowels and diphthongs the scribes wrote double letters only in the earliest documents, single letters later on: *scuoppa*, *leittan hācco* < */skó:bbja, lájddjan, há:ggo/ 'scale, lead, hook', later *scuopa*, *leiten*, *hāco*. After consonants only single letters were written, even in the earliest documents: *uulpa*, *gerta*, *rinka* < */wúlbbja, gárddja, hrínggja/ 'she-wolf, switch, brooch'.⁷⁹

In the labial and velar orders the reflexes of these geminates contrasted with inherited /b g/ after short vowels as long vs. short: *crippa* 'crib' vs. *geba* 'gift', *bruca* 'bridge' vs. *bogo* 'bow'. Here the contrast could still be analyzed as /bb gg/ vs. /b g/. After long vowels and diphthongs the same analysis could also be made as long as the old geminates are written double: *scuoppa* 'scale' vs. *huobun* 'heaved', *hācco* 'hook' vs. *lāgun* 'lay'. When the double spellings were given up, however, and we find *scuopa*, *hāco*, it would seem more reasonable to analyze the contrast as one of fortis /p k/ vs. lenis /b g/. This latter analysis is almost forced upon us (if we are to believe the scribes' spellings) when we consider the postconsonantal reflexes: *uulpa* 'she-wolf' vs. *selbo* 'self', *rinka* 'brooch' vs. *singan* 'sing'. If we accept these new fortis stops /p/ and /k/, the question then

⁷⁷ Cf. J. Fourquet, *Les mutations consonantiques du germanique* 101 (1948): 'Le fait essentiel [of the HG sound shift] est l'évolution de la corrélation de sonorité vers une corrélation d'aspiration.'

⁷⁸ It would be interesting to know whether we are dealing here with a 'drag-chain' or a 'push-chain' (André Martinet, *Word* 8.11 [1952]). Did the aspiration of /p t k/ 'drag' along the unvoicing of /b d g/? Or did the unvoicing of /b d g/ 'push' the aspiration of /p t k/? I have presented it as a 'drag-chain' because the aspiration of /p t k/ covers a far wider area than the full unvoicing of /b d g/. Cf. Fourquet, *Mutations consonantiques* 85-6, 104-5.

⁷⁹ For these OHG spellings, see Braune-Helm, *Ahd. Gram.* §96 Anm. 1; and Josef Schatz, *Althochdeutsche Grammatik* 105-34 (1927). In most words these original long stops following long vowels, diphthongs, and consonants were analogically replaced by the short stops of related forms: 2nd sg. *galaubis*, 3rd sg. *galaubit*, and hence also infinitive *galauben* for *galauppen*. See Braune-Helm, *Ahd. Gram.* §§358, 359 Anm. 1. This analogical substitution of short stops must have been very widespread, because remarkably few of the long stops have survived.

arises: how do we now interpret *crippa* and *brucca*? Do they contain phonemically long /pp kk/, or is a fortis consonant automatically long after a short vowel?

I believe that the analogy of the dentals gives us an answer to this question. In the dental order we find a three-way contrast after short vowels: (1) the reflex of /dd/ appears as *tt*: *bittu* < */biddju/ 'I ask'; (2) the reflex of /d/ appears as *t*: *tritu* < */tridu/ 'I step'; and (3) the reflex of /þ/ appears as *d*: *quidu* < */kwíþu/ 'I speak'.⁸⁰ Here we have a clear contrast between long fortis /tt/ in *bittu*, short fortis /t/ in *tritu*, and short lenis /d/ in *quidu*. After long vowels and diphthongs the same three contrasts are preserved in the early documents: /tt/ in *wāttan* < */wá:ddjan/ 'clothe', /t/ in *rātan* < */rá:dan/ 'advise', and /d/ in *gināda* < */ga+ná:þa/ 'mercy'; but very soon the long /tt/ is shortened: *wāten*. After consonants, long /tt/ was apparently shortened in pre-documentary times: *gerta* < */gárdджа/ 'switch' like *uarta* < */wárda/ 'watcher' (fem.), both contrasting with lenis /d/ in *erda* < */érþa/ 'earth'.

On the analogy of the dentals we may now analyze *crippa* and *brucca* as containing long fortis /pp kk/. These are then in double contrast, as both long and fortis, with the short lenis /b g/ of *geba*, *bogo*. Early *scuoppa* and *hācco* have /pp kk/, soon shortened to /p k/ (*scuopa*, *hāco*), but still contrasting with lenis /b g/ in *huobun* and *lāgun*. After consonants we find only short fortis /p k/ in *uulpa*, *rinka*, contrasting with lenis /b g/ in *selbo*, *singan*.⁸¹

The second great change of the Pre-OHG period affected the aspirates [p' t' k'] < PGmc. /p t k/. Initially, in gemination, and after consonants they became affricates; after vowels they became (presumably by way of affricates) fortis spirants.⁸² In the dental order this introduced a new spirant phoneme /ʒ/, contrasting with inherited /s/; see below. In the labial and velar orders, however, no new phonemes were introduced, since the new fortis spirants merged with the inherited geminate spirants: *treffan* < */drépan/ 'hit' like *heffen* < */háffjan/ 'heave'; *sahhan* < */sákan/ 'fight' like *hlahhen* < */hláxxjan/ 'laugh'; etc.

The resulting affricates are most easily analyzed as clusters consisting of stop plus spirant⁸³: *pfad*, *tropfo*, *limpfan* /pfád, trópfo, límpfan/ 'path, drop, be

⁸⁰ The change of [þ] to (voiceless lenis) [d] seems to be just about completed in the earliest Bavarian documents. See J. Schatz, *Altbairische Grammatik* 70 ff. (1907), *Ahd. Gram.* 97-8, 127 ff. Inherited /þþ/ > /dd/ > /tt/: *feddhacho*, *feddah*, *fettah* 'wing'. See Braune-Helm, *Ahd. Gram.* §167 Anm. 10; Schatz, *Ahd. Gram.* 133.

⁸¹ Modern Swiss dialects prove that these fortis-lenis contrasts after long vowels, diphthongs, and consonants were real, and are not just a misinterpretation of clumsy scribal spellings: southern Swiss *arlöippa* < */ar+láwbbjan/ 'allow', *welpa* < */hwálbjan/ 'arch', *erpa* < */árbbjan/ 'inherit', contrasting with *bliba* < */bli:ban/ 'stay', *zelbar* < */kálbir/ 'calves', *stērbā* < */stérban/ 'die'. See Leo Jutz, *Die alemannischen Mundarten* 178-9 (1931); velar examples are given on 228-9.

⁸² Through the familiar exceptions to the HG sound shift, inherited /sp st sk tr ft xt/ gave OHG /sp st sk tr ft xt/, which I interpret as fortis stops and spirants.

⁸³ Leonard Bloomfield, *Lg.* 14.181 (1938): 'Before the HG consonant shift, the language had the WGic unvoiced stops [p, t, k], the voiced (later unvoiced lenis) stops [b, d, g], and the unvoiced spirants [f, þ, s, h]—or was [þ] already voiced? The shift of initial [p-, t-, k-] consisted in their replacement by a phoneme of the second series plus a phoneme of the third series; apart from conditions peculiar to the cluster and from later changes, this produced phonemically [bf-, ds-, gh-].' (In a footnote Bloomfield seems to change his [ds] to

fitting'; *zehan*, *nezzi*, *herza* /tʒéhan, nétʒi, hértʒa/ 'ten, net, heart'; *chorn*, *decchan*, *scalch* /kxórn, dékxan, skáلكx/ 'grain, cover, servant'. The fortis spirants were short finally, and became short medially after long vowels and diphthongs during early OHG, but remained long medially after short vowels: preterit *greif*, *beiz*, *weih* /gréjf, bėjʒ, wéjx/ 'grasped, bit, yielded', nominative *scif*, *haz*, *ioh* /skif, háʒ, jóx/ 'ship, hate, yoke'; but infinitive *grifan*, *bizzan*, *wihhan* /grí:ffan, bí:ʒʒan, wí:xxan/, later *grifan*, *bizan*, *wihan* (or *wichan*) /grí:fan, bí:ʒan, wí:xan/; genitive *sciffes*, *hazzes*, *iohhes* /skíffes, háʒʒes, jóxxes/.⁸⁴

Fortis /f/ contrasted medially with inherited /f/, so that the latter is best analyzed as lenis /v/. The relationship between the two seems to have been like that between fortis /p/ and lenis /b/. After short vowels we find the double contrast of long fortis /ff/ vs. short lenis /v/: *offan* /óffan/ 'open' vs. *ouan* /óvan/ 'oven'. After long vowels the early documents show the same double contrast: *slāffan* /slá:ffan/ 'sleep' vs. *grāuo* /grá:vo/ 'count'; later on the long /ff/ was shortened to /f/: *slāfan* /slá:fan/. We also find an /f—v/ contrast after consonant when *helphan* /hélbfan/ 'help' changed to *helfan* /hélfan/, contrasting with *uolwa* /wólva/ 'wolves'.

All of these contrasts pertain only to medial position. In final position the OHG documents show no written distinction, and it would appear that inherited /f/ had become fortis in this position: gen. *houes* /hóves/ 'court' contrasting with *sciffes* /skíffes/ 'ship', but nom. *hof* /hóf/ like *scif* /skif/; pl. *uolua* /wólva/ 'wolves' contrasting with inf. *helfan* /hélfan/ 'help', but sing. *uolf* /wólʃ/ like pret. *half* /hálf/.⁸⁵

[dʒ].) I differ from this in interpreting the initial clusters as /pf tʒ kx/, with fortis stops and spirants. Although there was no fortis-lenis contrast in these clusters initially, there seems to have been such a contrast after /l r/ in the labial order. Pre-OHG /lpp rpp/ > OHG /lpf rpf/: *gelpf* 'gay', *scarpf* 'sharp', with /lpf rpf/ surviving, beside (analogical?) /lf rf/, into MHG *gel(p)f*, *schar(p)f*. In contrast with this, Pre-OHG /lp rp/ gave OHG /lbʃ rbʃ/: *helphan* 'help', *uuerphan* 'throw'; but as early as the 9th century this became /lf rf/: *helfan*, *unerfan*. See Braune-Helm, *Ahd. Gram.* §131 Anm. 5. Cf. also southern Swiss *helffə*, *werffə* vs. *šarpf*; Jutz, *Alem. Mdaa.* 170-1.

⁸⁴ See Braune-Helm, *Ahd. Gram.* §§97, 132, 145, 160.

⁸⁵ Southern Swiss dialects seem to show a distribution of fortis and lenis that is even more archaic than that of the OHG documents. They have the fortis-lenis contrast not only medially (*offə* 'open' vs. *ofə* 'oven'), but also finally, even after long vowel (*šlɔ:ff* 'sleep' vs. *grɔ:f* 'count') and after consonant (*dɔ:rf* 'village' vs. *wolf* 'wolf'). Cf. Jutz, *Alem. Mdaa.* 170-1, 180-1; the above forms from Wilhelm Wiget, *Die Laute der Toggenburger Mundarten* (1916).

Since these Swiss forms can be analyzed in terms of the two-way contrast /ff—f/, we may ask whether the three-way contrast /ff—f—v/ is really needed for OHG. The answer depends on how much symmetry we think there must be in a phonological system. In dealing with the stop series we could perfectly well say that fortis /p/ and /k/ were allophonically long after short vowels, but short after long vowels, diphthongs, or consonants, thus avoiding the assumption of /pp/ and /kk/. By this analysis *crippa*, *brucca* would be /kxrípa, brúka/. I have chosen not to make this analysis because of the evidence of the dentals. The contrast *bittu*, *tritu*, *quidu* forces us to analyze *bittu* as /bíttu/; therefore *crippa*, *brucca* are also /kxríppa, brúkka/, with long /pp kk/—even though in these orders there is no contrast with short /p k/. The situation with the spirants is exactly the same. After the change

Fortis /ʒ/ differed qualitatively from inherited /s/,⁸⁶ and hence did not (yet) merge with it in any positions. Because of the new fortis–lenis contrast which we find in other spirants, we must interpret inherited *ss* (occurring only medially after short vowel) as fortis /ss/, final *s* as fortis /s/, and medial *s* as (with an unhappy choice of symbol) lenis /z/. Examples: *wizzun* /wíʒʒun/ 'they know', *wissun* /wíssun/ 'they knew', *wízzan* /wí:ʒʒan/ (later *wízan* /wí:ʒan/) 'punish', *wísan* /wí:ʒan/ 'avoid', *ezzan* /éʒʒan/ 'eat', *wesan* /wéʒan/ 'be', *hwaz* /hwáʒ/ 'what', *was* /wás/ 'was'.⁸⁷

Fortis velar /x/ also differed qualitatively from inherited lenis glottal /h/, as already described. The two were in contrast only between vowels. Examples: *brehhan* /bréxxan/ 'break', *sehan* /zéhan/ 'see', *wíhhan* /wí:xxan/ (later *wíhan*, *wíchan* /wí:xan/) 'yield', *līhan* /lí:han/ 'lend', but finally *brah*, *sah*, *weih*, *lēh* /bráx, záx, wéjx, lé:x/ 'broke, saw, yielded, lent'.

The above outline is sketchy indeed, and considerable further work needs to be done on it. To the extent that it is correct, it gives for OHG (Alemannic and Bavarian) a completely new phonological structure. Older /p t k/ have become either the clusters /pf tʒ kx/ or fortis spirants, long /ff ʒʒ xx/ (with which older /ff xx/ have merged) and short /f ʒ x/ (which with older final /f x/ have merged); older /ss/ has become long fortis /ss/, and older final /s/ has given short fortis /s/; older /bb dd gg/ have become the new fortis stops, long /pp tt kk/ (with which older /pʰ bʰ dʰ gʰ/ eventually merged) and short /p t k/ (with which older /d/ has merged); older /b ʰ g/ and non-final /f s h/ have become the lenis stops and spirants /b d g v z h/. The resulting structure shows a distinctive feature of tenseness, opposing fortis /p t k f s/ to lenis /b d g v z/; the spirants /ʒ/ and /x/ are fortis only, and the spirant /h/ is lenis only. Possibly we may also recognize a distinctive feature of occlusion, and set up a structure of equipollent intersecting contrasts: labial (p : b) : (f : v) = (p : f) : (b : v), apical (t : d) : (s : z) = (t : s) : (d : z), velar (k : g) : x = (k : x) : g, besides fortis dorsal /ʒ/ and lenis glottal /h/. Assuming a special affricate series for /pf tʒ kx/ would be gratuitous: we already have the stops /p t k/ and the spirants /f ʒ x/, and there are no contrasts between an affricate and a cluster.

9. The stops and spirants of Proto-Germanic. Having completed our analyses of the oldest Germanic dialects, we can now use the comparative method to

slāffan > *slāfan* had taken place, we could say that fortis /f/ was [ff] medially after short vowels, but [f] after long vowels, diphthongs, consonants, and in final position. It seems to me inconsistent, however, to analyze *bittu* [bittu] as /bittu/, but *offan* [óffan] as /ófan/. I therefore analyze it as /óffan/, and put it in double contrast with *ouan* /óvan/.

One might of course analyze the fortis as /ff/ and the lenis as /f/, and write /hóff, hófes, skíff, skíffes/ for the scribes' *hof*, *houes*, *scif*, *sciffes*. This would again conflict with the analysis of the dental stops. It would also mean that we would write nom. *haz*, *ioh*, gen. *hazzes*, *iohhes* 'hate, yoke' as /haʒʒ, jóxx, háʒʒes, jóxxes/, and no simple /ʒ x/ would ever occur.

⁸⁶ On the phonetics of these two spirants see, most recently, Martin Joos, *The medieval sibilants*, *Lg.* 28.222–31 (1952).

⁸⁷ Modern standard German *beissen* /báissen/ 'bite' and *weisen* /váizen/ 'show' still reflect clearly the difference between the fortis /ʒ/ of OHG *bízan* /bí:ʒan/ and the lenis /z/ of OHG *wíson* /wí:zo:n/.

reconstruct the stops and spirants of the proto-language. We may use the comparative method with confidence, because it has been well tested; but we must also be aware of its limitations. It is inevitably subject to error from at least two sources. First, it requires us to operate as if the proto-language had been uniform, when in fact (as experience with observable languages shows us) it was not. Secondly, it 'assumes and must assume that when several related languages have a common feature this feature is to be ascribed to the parent language unless subsequent study proves this to be impossible.'⁸⁸ But inheritance from the parent language is only one of three ways by which related languages may share a common feature: they may also develop the same feature independently of one another; or the feature may develop in one language and then spread into the others.

I shall use the comparative method with full realization of these limitations. I shall assume, for example, that PGmc. /b/ was a stop initially because all the dialects show a stop in this position; and I shall assume that it was a spirant after vowels because all the dialects show a spirant in this position (excepting only OHG, which is disqualified as a witness because of its extensive restructurings). In actual fact, some areas of Proto-Germanic may have had an initial stop and others an initial spirant; and the initial stop may then have developed independently in the original spirant areas, or it may have spread to them from the original stop areas. Unfortunately the comparative method does not allow us to consider such possibilities. If, to make things easier for ourselves, we assume for both initial and postvocalic position a Proto-Germanic spirant [b] which just happened to develop into an initial stop in all the dialects, then there is nothing to prevent us from assuming that the whole shift of PIE /bh/ to [b] and [b] just happened to take place independently in all the dialects. As this *reductio ad absurdum* shows, any such exception, from the first one on, immediately destroys the whole comparative method of reconstruction, and with it the whole concept of a proto-language. We must apply the comparative method either strictly or not at all.

One more fundamental question needs to be asked and answered. In a good many cases we shall find that the evidence of the various dialects, even excluding OHG, is conflicting. After vowels, for example, PGmc. /d/ yields a spirant in Gothic, runic Norse, and Old Icelandic, but a stop in Old English and Old Saxon. What, then, do we assume for Proto-Germanic? The answer depends on whether we think it was the stop or the spirant which was an innovation. If we accept the minority view that PIE /bh dh gh/ became stops in Pre-Germanic, then Proto-Germanic had a stop in this position, and the spirant dialects show an innovation. If we accept the majority view that PIE /bh dh gh/ became spirants in Pre-Germanic, then Proto-Germanic had a spirant in this position and the stop dialects show an innovation.

I myself incline to the majority view for the same reasons that led Hermann Paul to propound it some eighty years ago.⁸⁹ First, the minority view makes the theory of Verner's Law unnecessarily complicated. If PIE /bh dh gh/ gave Pre-Germanic [b d g], then Verner's Law implies for the [ð] of such a word as

⁸⁸ Edgar H. Sturtevant, *The Indo-Hittite laryngeals* §21 (1942).

⁸⁹ Zur Lautverschiebung, *Beiträge z. Gesch. d. dt. Spr. u. Lit.* 1.147-201 (1874), esp. 170-1.

Gothic (gen.) *stadis* 'place' < PIE */statí-/ the following changes: (1) [t] > [p]; (2) [p] > [ð] by Verner's Law; (3) [ð] > [d] and merged with [d] < /dh/; (4) [d] > [ð]. The majority view accomplishes this in half the steps: (1) [t] > [p]; (2) [p] > [ð] by Verner's Law and thereby merged with [ð] < /dh/. Secondly, the minority view forces us to assume that OHG, which shows so many innovations in the treatment of PGmc. /p t k/, was the most conservative of all in its treatment of /b d g/, since it kept them as stops. This is poor theory; and it conflicts with the observable change of [b] to [β] that we can follow in the tribal name *Suēwī* > *Swābe*, *Swāpe* 'Swabian'.

From our analysis of the several dialects it is obvious that we shall assume for Proto-Germanic a labial, a dental, and a velar order. Must we also assume a glottal /h/, distinct from the velars? We may be sure that there was a stage in Pre-Germanic when no such glottal /h/ existed, since the spirantal reflexes of PIE /k/ which became voiced through Verner's Law merged with the spirantal reflexes of PIE /gh/.⁹⁰ This is sufficient proof that the spirant shared with [g] the feature of velar articulation, and that it therefore must have been velar [x] before voicing.

All of this applies to Pre-Germanic. What of Proto-Germanic, which by definition must contain any feature common to all the daughter languages? By a strict application of the comparative method (and we have shown that no other application is permissible), we must assume that initially this Pre-Gmc. velar [x] had become Proto-Gmc. glottal [h], since this is what all the daughter languages show. We must therefore set up a separate phoneme, Proto-Gmc. /h/. It occurred only in initial position,⁹¹ and it was in complementary distribution with /x/, which never occurred in initial position. Nevertheless, we cannot group the two together as allophones of a single phoneme, since the only distinctive features which they shared (voicelessness and spirantal articulation) were not common to them alone, but were shared by /f þ s/ as well.

In gemination, the only evidence that we have indicates a velar and not a glottal spirant;⁹² we may therefore assume Proto-Gmc. /xx/ in this position. Finally after consonants and vowels we find OE and OHG /x/ (OS /x/ finally

⁹⁰ See above, note 18.

⁹¹ We must at least consider the possibility that this initial /h/ never was velar, but evolved directly from PIE /k/. Such an assumption would violate the working hypothesis which I believe we all use: that phonemic change involves only one distinctive feature at a time. For example, where we find that an earlier back rounded /u/ has become a front unrounded /i/, we never assume a direct change, but always back rounded /u/ to front rounded /ü/ to front unrounded /i/. This same hypothesis would force us to reject the theory that the velar stop /k/ changed directly to the glottal spirant /h/. Instead, we would assume the change of velar stop /k/ to velar spirant /x/ to glottal spirant /h/.

Evidence that PIE initial /k/ gave Pre-Gmc. initial /x/ is to be found in the prefix *ga-*, if we accept the etymology which connects this with Latin *co-* etc. This would mean that PIE */ko-/ gave Pre-Gmc. */xa-/ and then, by Verner's Law, Proto-Gmc. */ga-/, proving that the spirant of */xa-/ shared with /g/ the feature of velar articulation.

⁹² OE *cohhetan*, ME *coughen*, MLG *kuchen*, MHG *küchen* 'cough'; OE *ceahhetan*, OHG *kachazzen* 'laugh'; etc. The absence of inherited /xx/ in Gothic and Norse is crucial. For Gothic and runic Norse we have only negative evidence; for Old Icelandic we have the definite assurance of the First Grammarian. If /xx/ had survived in any of these last three dialects, it would have produced a three-way contrast in the velar order and have given it the same phonological structure as the labials and dentals.

TABLE 6. THE STOPS AND SPIRANTS OF PROTO-GERMANIC

[illegible]

after stressed short vowels), so that here also we may set up Proto-Gmc. /x/ and consider the development to /h/ to be an innovation. So also with the positions before /s/ and /t/. Medially before vowels, however, all the dialects show /h/ (or its reflexes), so that it would at first appear that we must assume Proto-Gmc. /h/. Further study shows that this is not the case. We have just spoken of Proto-Gmc. /x/ in 'final position'. It probably WAS final in (for example) the 1st sg. pret. of strong verbs: */fálx/ < */pólka/ 'hid', */jáx/ < */jóka/ 'spoke'. (Forms without the final /a/ in these words must be ascribed to Proto-Germanic because all the daughter languages show such forms.) But in other forms which show OE, OS, and OHG final /x/, the consonant was NOT final in Proto-Germanic, but had a vowel after it: OE *sealh*, *mearh*, *scōh* 'willow, horse, shoe' < Proto-Gmc. nominative */sálxaz, márxaz, skó:xaz/. Unless we wish to assume that Pre-Gmc. [x] changed to Proto-Gmc. [h] and then back again to [x] in OE, OS, and OHG, we must set up Proto-Gmc. /x/ in these forms. Since Pre-Gmc. [x] was kept here medially before a vowel, it must also have been kept medially before a vowel in other forms, and not have changed to /h/. We shall therefore set up /x/ in such a form as Proto-Gmc. */téuxanan/ 'lead', and assume that the change of /x/ to /h/ (with subsequent developments) was an innovation in the prehistory of the daughter languages: Gothic *tiuhan*, OIc. *tiða*, OE *tēon*, OS *tiohan*, OHG *ziohan*.²²

With the status and distribution of Proto-Gmc. /h/ and /x/ established, our only remaining task is to see what allophones can be identified for the stops and spirants in terms of distinctive features. For the voiceless stop series /p t k/ there is no evidence of allophones at all; the OE palatalization is clearly an innovation. For the voiceless spirant series /f þ x h/ there is likewise no evidence of allophones; the shifts of /x/ to /h/ and the various voicings are all innovations. There remains the voiced series /b d g/, which we take up individually.

Proto-Gmc. /b/ was a stop initially, in gemination, and after nasal (Table 6, columns 1-4), since all dialects which give evidence on these positions show stops. It was a spirant after /l/, /r/, and vowels (columns 5-10), since most of the dialects show spirants in these positions; the stops in Gothic (after /l/ and /r/) and in OHG are innovations.

Proto-Gmc. /d/ was a stop initially, in gemination, after nasal, after /l/ (columns 1-6), and also after /z/, since all the dialects show stops in these positions. It was a spirant after /r/ and vowels (columns 7-10), since runic Norse and Old Icelandic show spirants in these positions; the stops in Gothic (after /r/ and /g/) and in West Germanic are innovations.

²² Proto-Gmc. intervocalic /x/ was still the voiceless counterpart of intervocalic /g/ at the time of the spirantal dissimilation in Gothic; see above, note 18. Since this phenomenon seems to belong to the prehistory of Gothic, this is added proof that we must set up intervocalic /x/ and not /h/ for Proto-Germanic. We may also posit Proto-Gmc. /x/ before /j/ in */hláxjanan/ 'laugh', and assume that it remained velar up to the time of the WGmc. consonant doubling. Cf. the /xx/ of OE *hlæhhan*, OHG *hlahhen*.

The necessity of assuming Proto-Gmc. medial /x/ before vowels is an apt illustration of Sturtevant's qualifying phrase (note 88 above) that a feature common to several daughter languages must be ascribed to the parent language 'unless subsequent study proves this to be impossible'.

Proto-Gmc. /g/ was a stop in gemination and after nasals (columns 2-4), since all dialects show stops in these positions; the palatalization in Old English is an innovation. Initially (column 1) it was a spirant because of the OS evidence (spirant before consonants and non-front vowels, /j/ < [g] before front vowels) and the OE evidence (/j/ < [g] before front vowels); the OE stops before consonants and non-front vowels, and the stops of the other dialects, are innovations. /g/ was also a spirant after /l/, /r/, and vowels (columns 5-10) because of the runic Norse, OIc., OE, and OS evidence; the stops of Gothic and OHG are innovations.

Our analysis shows the following structure for the stops and spirants of Proto-Germanic: (p : f) : b, (t : þ) : d, (k : x) : g, h; to this we may add the voiceless-voiced pair s : z. The positions of occurrence and the allophones of /b d g/ are given in Table 6, in terms of our 12 environments. To this should be added the environment 'after /z/', since here also the evidence of all the dialects indicates that Proto-Gmc. /d/ was a stop.⁹⁴

⁹⁴ [The foregoing article was offered as a contribution to the Edgerton number of *LANGUAGE* (29:3), but reached the Editor too late to be included in that number.]

PRAKRIT *vaṇadava* 'TREE SAP, SELF-CONTROL'

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In the Jaina Māhārāṣṭrī tales excerpted by the late Hermann Jacobi from the monk Devendra's commentary on the Jain canonical work Uttarādhyayana Sūtra, and published by him in his *Ausgewählte Erzählungen in Māhārāṣṭrī* (Leipzig, 1886), there is a puzzling stanza in the story of King Bambhadatta (Sanskrit Brahmadatta), for which I wish to offer an interpretation. This story is from the commentary on Chapter 13 of the Uttarādhyayana Sūtra, which is entitled Citta and Saṃbhūya (Sanskrit Citra and Saṃbhūta), the names of two Jain monks. One of these, Saṃbhūya, has been reborn as the universal emperor Bambhadatta, and the other is again a Jain monk; the chapter consists of a conversation between the two. The context, however, is the story of the two in a previous existence when they were the twin brothers Citta and Saṃbhūya.

The souls of these two, it happened, had been companions, usually twins, in a series of previous existences. In the one with which we are here concerned they had been born in the city of Benares as the twin sons of a Cāṇḍāla (Untouchable) chief. Their father saved from death the king's Chief Minister named Namuci, who had committed an offense, evidently adultery with the queen. The Cāṇḍāla sheltered Namuci in his underground home, where the latter taught the two boys all the polite arts (*kalā*). However, he also seduced the Cāṇḍāla's wife, and was discovered; but the two boys, grateful for his tutoring, helped him escape. He fled to Hastināpura, the city of the universal emperor Sanatkumāra, whose minister he became. When, through a series of circumstances, the two baseborn but now highly educated boys had become disgusted with the world, they met a Jain monk, who preached to them the profession of monks (*sāhu-dhamma*) taught by the prince of Jinas, which is capable of burning up the forest of karma (*kamma-vaṇa-dahaṇa-sahaṇa*). They took the monks' vows, became fully competent in their duties as monks, and won the supernatural powers. In their wanderings they came to the outskirts of Hastināpura, and one day the monk Saṃbhūta entered the city to beg his breakfast after a month's fasting. There the minister Namuci saw and recognized him, and fearful of being betrayed sent his men to beat and drive him from the city. Saṃbhūta was filled with anger, a defiling emotion which as a Jain monk he should have conquered. He resorted to his supernatural powers and emitted from his mouth a fire to burn up his attackers. Great clouds of smoke darkened the whole city; the townspeople came in fear to beg relief, and King Sanatkumāra came as well to propitiate him; but Saṃbhūta would not be appeased. His brother Citra also saw the smoke and came to Saṃbhūta, saying: 'Saṃbhūta, quench, quench the fire of your anger. Great sages have calmness as their chief characteristic. Even when wronged they do not give way to anger; for anger comes to an evil end, is the cause of all misfortunes, the flame of a forest fire for the wood of pious restraint

(*caran'-indhana-davāṇalo*). And it is said—' At this point Citra speaks the verse which is the subject of this note (3, lines 17-18):

*jaha vaṇa-davo vaṇa-davaṃ davassa jalio khaṇeṇa niddahai
evaṃ kasāya-pariṇao jīvo tava-saṃjamam dahai*

Jacobi was baffled by the stanza, and included it among his 'dunkle Stellen' (158). Other scholars have been perplexed too. P. E. Pavolini, *Giornale della Società Asiatica Italiana* 6.119 (1892), adopted an emendation suggested to him by Ernst Leumann, reading *jaha vaṇa-davo vaṇam dava-davassa jalio ...*, and translated: 'Como l'incendio rapido divampa, la selva distrugge, rea di peccato l'anima l'asceti de' sensi rifugge.' John Jacob Meyer, *Hindu tales* 13-4 (London, 1909), rejecting the emendation, translates: 'As the forest fire kindled by the forest fire burns the essence of the forest in a moment, so the soul changed by passions consumes asceticism and self-control.'

The difficulty is with the word *vaṇa-dava*, which appears twice in the stanza. Jacobi, in his *Wörterbuch*, derives this from Skt. *vana-dava* and gives the meaning 'Waldbrand.' Pavolini and Leumann accept this meaning for the first occurrence of *vaṇa-dava*, but by emending the text find in *dava-davassa* a derivative from Skt. *dru* 'run' and a meaning 'quickly' ('rapidamente' in Pavolini's footnote). Meyer, in a long footnote, after rejecting the emendation, obliquely takes a tip from Leumann and Pavolini about the meaning of *dava*, and remarks: 'In the first two pādas the second *vaṇadava* may be = *vanadrava*, taking *drava* as "juice, essence." ' This seems to me a start toward the right interpretation, but Meyer then wanders into a byway. He proceeds to describe an area ravaged by a forest fire, which he sees as a fit symbol of the soul ravaged by passions, and goes on to say: 'One passion, if indulged in, engenders other passions, the corrupted soul begets, so to speak, a soul still more corrupted, just as the fire is propagated by the fire (*davassa jalio* lit., kindled of the fire). So *vaṇadava* would be a punning word and not a bad one if we bear in mind that the soul itself destroys the soul, just as *vaṇadavo vaṇadavaṃ*.' Then he quotes Dhammapada 240, which speaks of rust sprung from metal as destroying the metal, and in line with this suggests an alternative translation of the Prakrit stanza: ' "Just as the forest fire burns up the forest fire," i.e. consumes itself, "so the depraved soul destroys the true self of the soul: asceticism and self-control." '

The objections to these various renderings seem to me to be the following. First, the emendation is unnecessary. Though manuscript C, obtained by Jacobi after publishing his *Erzählungen* (see Meyer 290), apparently supports it by reading *jaha vaṇadavo vaṇam davavassa jalio*, support is illusory, since this reading itself needs emendation to make meter and sense.

Secondly, the rendering of *vaṇa-dava* as the equivalent of Sanskrit *vana-dava* involves a comparison of *jīva* ('being or soul') with a forest fire, which though possible seems somewhat inappropriate. Further, since *dava* itself normally means 'forest fire' in Sanskrit, the compound *vana-dava* would seem probably, though not necessarily, to mean 'forest forest-fire' and thus to be tautological.

The Prakrit *vaṇadava* seems to me more likely to represent Sanskrit *vana-drava*. That is, *dava* corresponds to Sanskrit *drava*, as suggested by Leumann,

Pavolini, and Meyer (see above), and means 'fluid, essence, sap'. But the specific meanings of *vana-drava* are, I believe, different from anything those scholars had in mind. These meanings I take to be two: (1) 'tree sap', especially 'turpentine, resin', and (2) *saṃyama* 'control, restraint of the senses, self-discipline'.

The word *drava*, though not recorded in the dictionaries as compounded with *vana*, appears in composition with the words *sarala* (masc.) and *saralā* (fem.), which are variously defined as the long-leafed pine (*Pinus longifolia*, see PW, with numerous citations from medical and other literature) or the *devadāru* (*Cedrus lebanii*, *Cedrus deodara*) or deodar (*Abhidhāna Rājendra* 7.526, column 1). The neuter *sarala* is defined as the resin of *Pinus longifolia* (PW, with literary citations), and so too is the masc. *saraladrava*, which PW cites from the *Amarakośa*, the *Trikāṇḍaśeṣa*, and Hemacandra's *Abhidhānacintāmaṇi*. The *Hindī Śabda Sāgara* identifies the *sarala* tree with the *cīṛa* or *cīṛha*, which is the long-leafed pine,¹ and under *cīṛha* mentions the use of its wood for torches by the hill folk and of its resin, or turpentine, in ancient India as an antidote for coughs, acidity, and catarrh.

The word *drava*, besides having the meaning of 'fluid, juice, sap', and some additional secondary meanings, has among the Jains the special meaning of *saṃyama* 'restraint, control, self-discipline'. In the *Abhidhāna Rājendra* 4.2458 (top of column 2), *dava* (= *drava*) is defined as *saptadaśavidhe saṃyame*, i.e. 'the seventeen varieties of *saṃyama*'.² The term *drava* is applied to *saṃyama*, the author states, because it turns solid karma to fluid (*karmakāṭhinyadravaṇa-kāritvāt*), and he refers in support to various passages in the Jain canon. Note also the word *daviya* (Skt. *dravika*) in the same lexicon (4.2459) defined as *drava, saṃyama*.

Both meanings of *drava*, 'sap' and 'self-control', seem to me to be present in the compound *vaṇa-dava*, which I understand to represent Sanskrit *vana-drava*. It is necessary to consider the meaning of *vana* in the compound. The word means both 'forest' and 'tree.' In connection with *drava* as 'sap' it may be taken as 'tree'; that is, *vana-drava* is 'tree sap,' equivalent to *sarala-drava* 'turpentine,' which is inflammable and could therefore be ignited by a forest fire (*davassa jalio*). When so ignited, it blazes up suddenly and quickly burns itself out; and though, as sap, it is the life of the tree, it destroys that life in destroying itself.

When *vana* is used with *drava* as a synonym for *saṃyama*, it is to be taken as 'forest', and is susceptible of at least two possible interpretations. It may refer to the fact that dwelling in the forest (*vaṇavāsa*) is most suitable for a monk engaged in the cultivation of his religious duties. Just following the stanza which we have been discussing there appear in Jacobi's text (3, lines 20-6) two stanzas which proclaim the futility of fasting, forest-dwelling (*vaṇavāsa*), study, meditation, chastity, and a life by begging, if a monk is overcome by anger. The other possible interpretation of *vana* is as an equivalent of karma, a frequent comparison among the Jains, appearing in this very story (Jacobi 2, line 29) in the phrase *kamma-vaṇa*, already cited above. In the latter case the figure of speech would

¹ On the use of these Hindi names for this tree cf. Upendranath Kanjilal, *Forest flora of the School Circle, N-WP 351* (Calcutta, 1901).

² For these seventeen varieties see *Abhidhāna Rājendra* 7.88, column 2 s.v. *saṃjama*.

involve the power of *drava* (= *saṃyama*) to turn the hard wood of karma to fluid and melt it away, and *vana-drava* would mean 'solvent for the wood [of karma]'. In whatever fashion *vaṇa-dava* (*vana-drava*) has come in this stanza to mean *saṃyama* 'self-control', the application of it to Sambhūta's case seems clear. Through *saṃyama* the monk masters the hard battle of the faith, which makes him overcome the power of karma and win release from rebirth. *Saṃyama* is then the sap, as it were, of religion. But *saṃyama* is also the means by which the fully accomplished monk acquires the supernatural powers that Sambhūta had mastered, and these are inflammable by the four passions (*kaṣāya*)—anger (*krodha*), pride (*mana*), deception (*māyā*), and greed (*lobha*). When ignited by anger, *saṃyama* destroys itself, just as does turpentine in the pine tree when ignited by the forest fire, and so brings ruin to the individual.

The stanza then seems to mean: 'As the sap of a tree (turpentine), when ignited by a forest fire, in an instant burns out the sap of the tree itself (punningly, as self-control, when ignited by a forest fire [of anger] in an instant burns up the self-control itself), so a being, transformed by passion, burns up his self-control acquired by asceticism.'

The results for Sambhūta were mixed. By virtue of his religious accomplishments he won gratification of his dying resolve to be reborn a universal emperor, and so became King Brahmadaṭṭa and had a magnificent seraglio. But by having yielded to anger he was, when reborn as Brahmadaṭṭa, possessed of a fatal flaw in his character which caused him to cherish anger and practise cruelty; and when he died, after living 716 years, he fell to the seventh hell, where he must live 33 sāgaras, that is 33×10^{15} palyopamas, of years.³

³ [The foregoing article was offered as a contribution to the Edgerton number of LANGUAGE (29:3), but reached the Editor too late to be included in that number.]

THE ROLE OF CONTEXT IN THE PERCEPTION OF STOPS

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[Recent experiments with synthetic speech have shown that the perception of voiceless stops depends in large measure on the context, not on the characteristics of the consonants alone. The same phenomenon was investigated in actual speech by recording voiceless stops on tape before certain vowels, cutting them away, and splicing them back before other vowels. The change in context caused certain instances of [k] to be perceived as [p] or [t]. The experiment shows that in actual speech the context of an initial voiceless stop is an important factor in its perception; the results agree strikingly with those obtained for synthetic speech. It is further suggested that aspiration ought properly to be regarded as part of the following vowel rather than of the preceding consonant.]

The question under consideration here is that of the role played by context in the perception of the stop consonants [p, t, k].¹ Recent experimentation with synthetic (machine-produced) speech has indicated that identification of synthetically produced prevocalic stops does not depend solely upon the characteristics of the consonant, but depends also on the context in which the consonant is heard. The attempt to produce synthetic [p, t, k] initial in the syllable shows that in certain cases, identical acoustic cues are heard as different consonants before different vowels; and that in certain cases, in order for the same consonant to be heard before different vowels, different cues must be used.²

That in synthetic speech it is the consonant IN RELATION TO THE FOLLOWING VOWEL which determines how it will be perceived, suggests that in actual speech an artificially produced change in the context of a consonant might effect a resulting change in its perception. If the consonant were heard, for instance, before a vowel other than the one before which it was actually pronounced (a situation easily produced by cutting apart and recombining segments of magnetic tape recordings), we would expect, in certain cases, a change in the perception of the consonant. The purpose of the experiment to be discussed here was to determine to what extent such changes in perception occur when stop consonants, recorded on magnetic tape, are cut away from the vowels before which they were pronounced and are spliced back before other vowels. I shall review first the synthetic speech experiment, and then describe the procedures and results of dealing with the same problem in actual speech.

1. Synthetic speech experiment.³ The speech used in this experiment was

¹ A shorter version of this paper was presented before the Modern Language Association of America in Boston, December 1952. The research on which the paper is based was supported in part by the Signal Corps, the Air Materiel Command, and the Office of Naval Research, and in part by the Carnegie Corporation.

² Liberman, Delattre, and Cooper, The rôle of selected stimulus-variables in the perception of the unvoiced stop consonants, *American journal of psychology* 1952.497-516.

³ This experiment was carried out by Liberman, Delattre, and Cooper; see the reference in fn. 2. The work with actual speech was a direct outgrowth of their experiment with synthetic speech; it was undertaken to test the validity of inferences about actual speech based on the results obtained with synthetic speech.

produced by a playback machine designed to convert spectrograms (similar to those produced by the Kay Electric Sona-Graph) into sound. Simplified hand-painted patterns closely resembling spectrograms can also be converted into sound by the playback machine, yielding 'speech' of a fairly high degree of intelligibility. It was with such painted patterns that the syllables were produced in this experiment.

The experiment was concerned with the perception—as [p], as [t], or as [k]—of a brief burst of noise presented in initial position in the syllable. Bursts at twelve different frequency positions were used, each one paired with seven synthetic vowel sounds to form the test syllables. Fig. 1 shows these bursts of noise (1A) and the vowel sounds (1B); Fig. 1C illustrates one of the test syllables: the burst at 1440~ combined with the vowel [a].

These 84 syllables (the twelve bursts paired with the seven vowels) were presented in random order to a group of subjects, who were asked to identify the initial consonant sound as [p], [t], or [k]. Fig. 2 shows the subjects' identifications of the bursts in each one of the syllables. Bursts at frequencies above

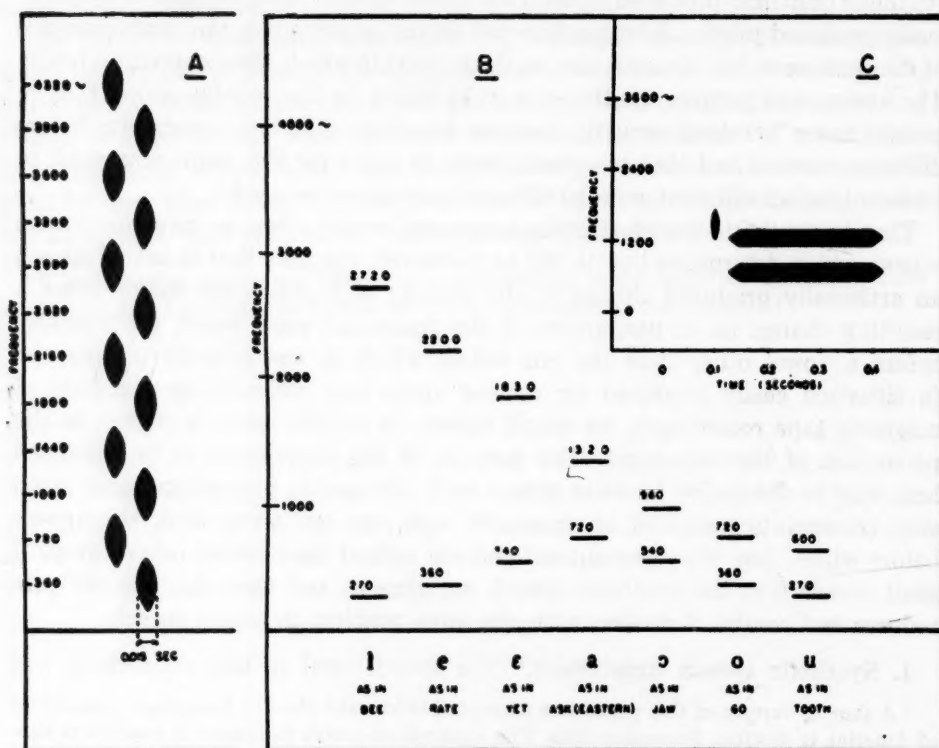


FIGURE 1

Stimulus patterns used in determining the effect of burst positions on the perception of voiceless stops. A: Frequency positions of the twelve bursts of noise. B: Formant-frequency positions of the vowels with which the bursts were combined. C: One of the composite syllables. (By Liberman, 1952.)

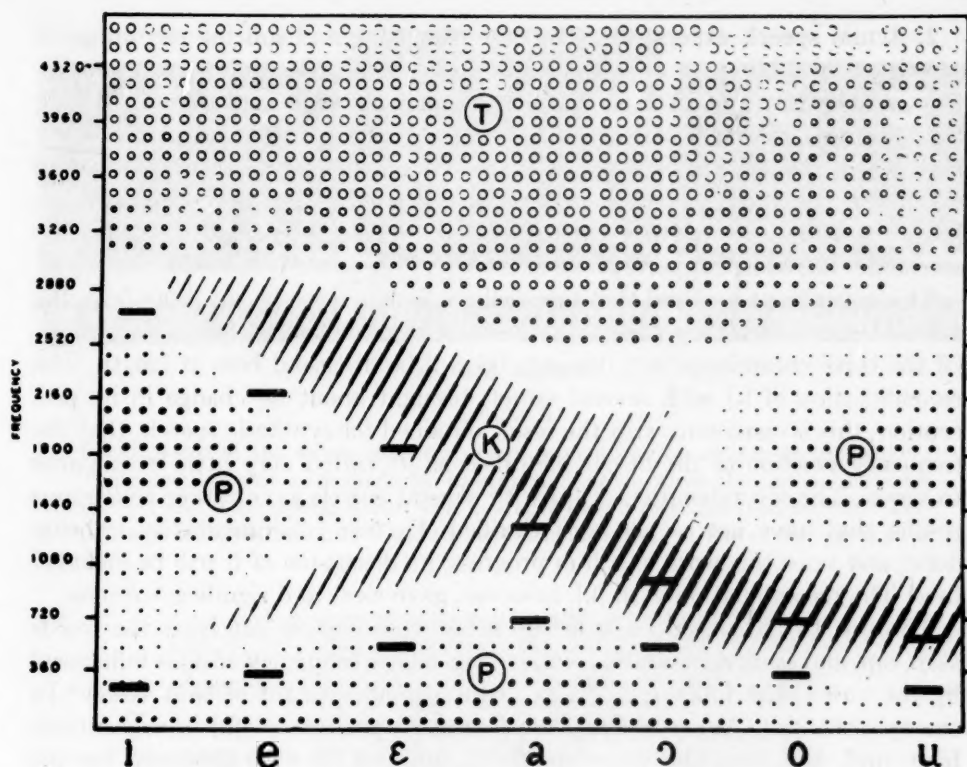


FIGURE 2

Map of the areas in which the judgment [p], [t], or [k] predominates. Center frequencies of the bursts of noise are represented along the vertical axis; the vowels with which they were combined are placed along the horizontal axis. (By Liberman, 1952.)

3000~ were identified as [t] before every vowel. In this range the conclusion can be drawn that the frequency position of the burst determines how it will be heard, regardless of context. Below 3000~, however, whether the burst was heard as [p], [t], or [k] depended on the combination of burst plus vowel, not on the burst alone. The bursts which yielded [k] responses varied from a frequency of 2880~ before [i] to as low a frequency as 720~ before [u]. The 720~ burst was heard as [p] before [a], as [k] before [u]. The 1440~ burst before [i] and [u] was heard as [p], and before [a] as [k]. The 2880~ burst before [i] was heard as [k], before [a] as [t].

These results indicate clearly that in the frequency range under 3000~, identification of the consonants depends not only on the burst, but on the burst in relation to the vowel which follows it. The consonant-vowel combination is of primary importance in the perception of these synthetic stops: placing a burst before two different vowels changes the way in which that burst is perceived. It is precisely this point which was to be tested in actual speech, by placing a recorded consonant before several recorded vowels in turn and observing the effect of these changes on the perception of the consonant.

2. Actual speech experiment. The ease of cutting and splicing recordings of speech on magnetic tape makes possible the recombinations of segments necessary in an experiment such as this. By running the tape very slowly (turning the tape reels by hand) it is possible to detect aurally the exact point on the tape where the voicing of a vowel begins after a voiceless consonant, and to determine just where the explosion of a stop is located. Once the desired segments are isolated by cutting, they can be spliced together in such a way that the mechanics introduce no perceptible effect.

This experiment required that prevocalic voiceless stops be cut away from the vowels before which they were pronounced and spliced back before other vowels. Of the three consonants [p, t, k], only [k] will be discussed here at length. The recombination of [t] with several vowels brought about no change in its perception; this is consistent with the result obtained for synthetic speech, that the frequency position of the bursts identified as [t] varied very little from vowel to vowel. The recombination of [p] with several vowels gave rather ambiguous results that have not as yet been clarified. Further experimentation is being done; and since this work is still in progress, no discussion of it will be included here. The recombination with [k], however, gave clear and significant results.

The [k]s to be spliced back before other vowels were cut from the words *keep*, *cop*, and *coop*. As is well known, in English the frequency of [k] is influenced by the vowel that follows; it varies as the second formant of that vowel.⁴ In the syllables [ki], [ka], and [ku], the frequency position of [k] is respectively high, mid, and low. The three vowels [i], [a], and [u] were also used for the recombinations, after [k] had been cut away from the pronounced syllable; for (as shown in Fig. 2) the frequency area of [k] before any one of these vowels does not extend into the frequency area of [k] before either of the other two. Each vowel has a [k] area of its own, and the three areas occupy discrete positions on the frequency scale. By using [k]s from before any one of these vowels and combining them with the other two, we set up a situation which is most likely to yield changes in consonant perception.

Tape recordings were made of an American speaker's utterances of the words *keep*, *cop*, *coop* and *heap*, *hop*, *hoop*. The initial [h]s were to insure a smooth onset for the three vowels to be used in the recombinations (since [h] produces no transitional effect on the vowel formants such as might, if present, influence the perception of the preceding consonant). The vowel was followed in each case by [p], so that the speaker would have real words to pronounce instead of nonsense syllables, and that real words would result when initial consonants were added to the vowels. The [k]s were then cut away from the words in which they had been pronounced. The exact point at which this cut was made is shown on the oscillograms of *keep*, *cop*, and *coop* in Fig. 3. As can be seen there, the words consist of an initial burst (*a-b*) followed by a period of aspiration (*b-c*); at *c* the voicing of the vowel begins. In this set of cuttings, everything was retained up to *c*, where the aspiration ends—about 85 milliseconds from the

⁴ Potter, Kopp, and Green, *Visible speech* 96 (New York, 1947).

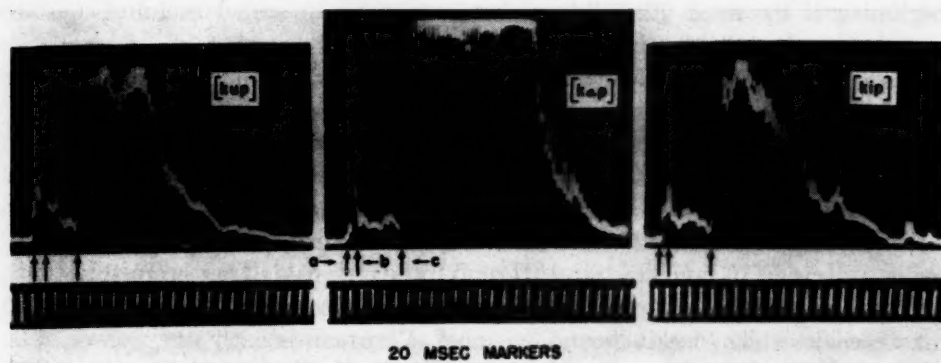


FIGURE 3

Oscillograms of the words *coop*, *cop*, *keep*, pronounced by an American speaker. These oscillograms were made by passing the speech wave through a full-wave rectifier and a smoothing circuit that averaged over a 10-millisecond interval. The [k] burst is shown from *a* to *b*, and the aspiration from *b* to *c*. The vowel begins at *c*.

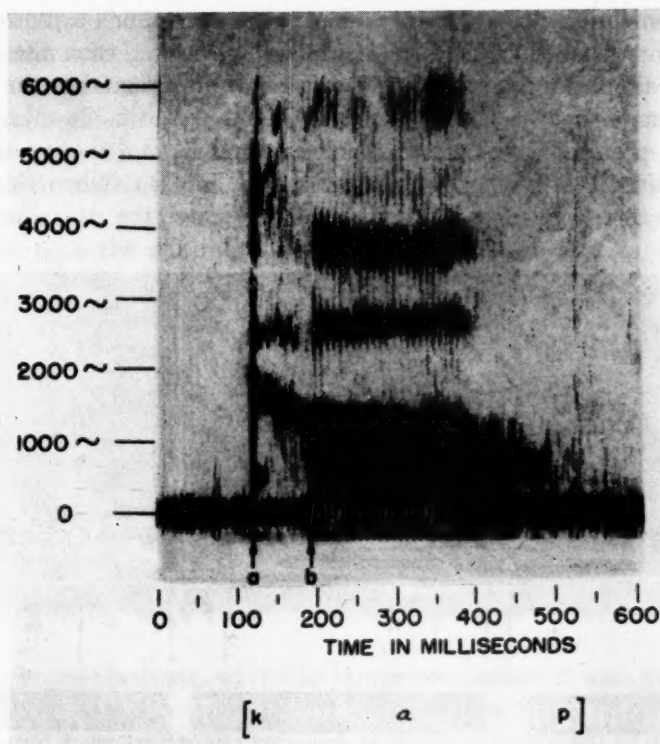


FIGURE 4

Spectrogram of the word *cop*, in which *a* marks the [k] explosion and *b* marks the beginning of voicing in the vowel. Concentrations of energy appear in the aspiration between *a* and *b* at approximately 1500~ and 2500~.

beginning of the word. Each [k] was cut away from its own vowel and spliced back before the syllables [ip], [ap], and [up].

These combinations yielded extremely unnatural sounding syllables, in which the consonants did not closely resemble any naturally produced English sounds. This method of cutting was therefore abandoned, and a new method, to be described below, was undertaken. However, before abandoning these syllables completely, we decided to run a preliminary test with them to see how subjects would identify the consonants when instructed that they *MUST* make a decision in favor of [p] or [t] or [k] in every case. They were told that although the consonant might sound very little like any of these three, they must decide which one it resembled most. Very consistently they chose [k], whereas the synthetic speech experiment suggests that other consonants might be heard.

This can be explained by considering where, in the syllables [ki], [ka], and [ku], the cuts were made. The consonant-vowel sequence consists, as we have seen, of an initial burst, an aspiration, and a vowel. Fig. 4 shows a spectrogram of [kap], in which the consonantal explosion occurs at *a* and voicing begins at *b*. In the aspiration between these two points, clearly distinguishable areas of energy concentration show up which look very much like extensions of the vowel formants. Although they are considerably weaker than the vowel formants (since the latter are voiced), these 'voiceless formants' in the aspiration are prominent enough so that the vowel is easily identifiable even when the entire voiced portion of the syllable has been removed and the voiceless part is heard alone (the cut having been made at *b* in Fig. 4). When this segment, identifiable by itself, is spliced in before another vowel, the desired objective of

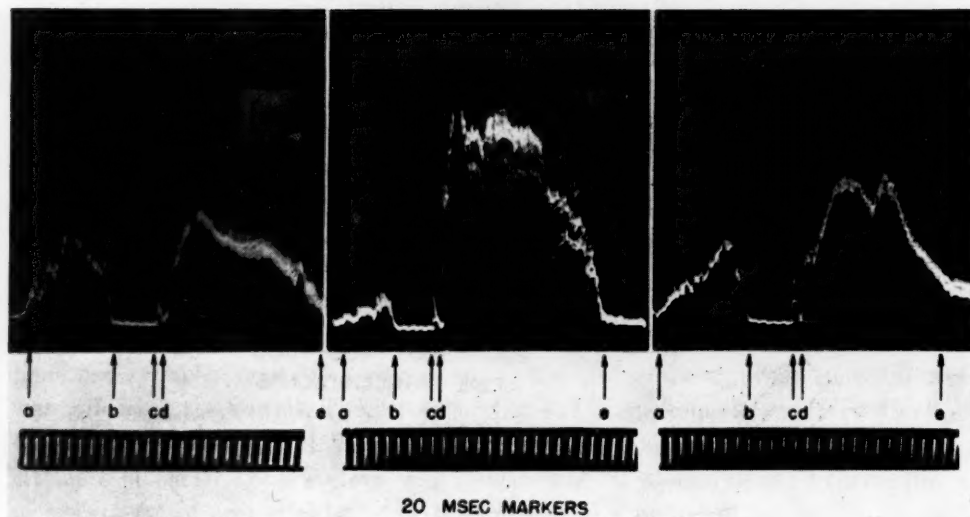


FIGURE 5

Oscillograms of the syllables [sku], [ska], [ski], pronounced by an American speaker, obtained by the same technique as those in Fig. 3. The [s], *a-b*, is followed by silence, *b-c*. The [k] explosion, *c-d*, is followed directly by the vowel, *d-e*.

having [k] heard before a different vowel is not really achieved. In such a combination, [k] is heard followed first very briefly by its own vowel, voiceless, and then by the other vowel, voiced. Hence the unusual effect.

Since our purpose was to combine [k] with a different vowel from the one before which it was pronounced, and since the aspiration contains evidence of the original vowel, the next step was to eliminate the aspiration and make combinations using the consonantal burst alone. Since [k] is not aspirated after [s] in English, we decided to work with the [sk] segment from the syllables [ski], [ska], [sku]. These syllables are shown on oscillograms in Fig. 5. The [s] extends from *a* to *b*, and the silence preceding the [k] burst from *b* to *c*. Since there is no aspiration, the [k] burst, *c* to *d*, is followed immediately by the vowel, *d* to *e*. When the cut is made at the beginning of the vowel, at *d*, and the [sk] segment is heard alone, there is now no indication of the vowel before which the [k] was originally pronounced. The [k] burst is in each case between 10 and 15 milliseconds long. The bursts from [ski] are centered in frequency at about 3000~, those from [ska] at 1800~, and those from [sku] at 1200~.

The [sk] segment was cut away from the vowel in the recorded syllables [ski], [ska], [sku]. Each [sk] was spliced back before each of the three sequences [id], [ar], and [ul]. (The final consonants were included so that real words would result no matter which stop was perceived in the initial segment: *skied*, *steed*, *speed*; *scar*, etc. As before, [h]s were pronounced initially and then cut away.) The three [sk] segments (from [ski], [ska], and [sku]), each combined with the three vowels, yielded a total of nine combinations. Five samples were made up of each of these combinations, and the 45 syllables were presented in random order to 20 subjects, who were asked to identify the initial cluster as [sk], [sp], or [st]. This time the subjects' identifications included reports of all three stops. In some positions the preferred identification of the consonant was [p], in some positions [k], and in one position [t]. These responses are summarized in Fig. 6.

Looking first of all at the responses for the syllables in which the [k] burst was combined with the vowel which it originally preceded ([ki] burst plus [id], [ka] burst plus [ar], [ku] burst plus [ul]), we see that the burst was perceived as [k] in every case. That the burst was heard as [k] before its own vowel is important, and any contextual significance attributed to changes in perception in other positions must rest on this fact; for the only way we have of being sure that changes in perception are due to position and not to some alteration of the sound due to cutting, is that the burst still sounds like a [k] before its own vowel.

In the other combinations, when the burst was combined with vowels other than the one before which it was pronounced, [k] was not the preferred judgment. The burst from [ski] was perceived as [t] when combined with [a] and as [p] when combined with [u]. The burst from [ska] was perceived as [p] when combined with both [i] and [u]. The burst from [sku] was perceived as [p] before [a] and as both [p] and [k] (half each) before [i]. (Before [i], the responses of EACH subject included both [k]s and [p]s.) These responses indicate that the perception of a spoken [k], as [k] or as some other stop, depends very much on

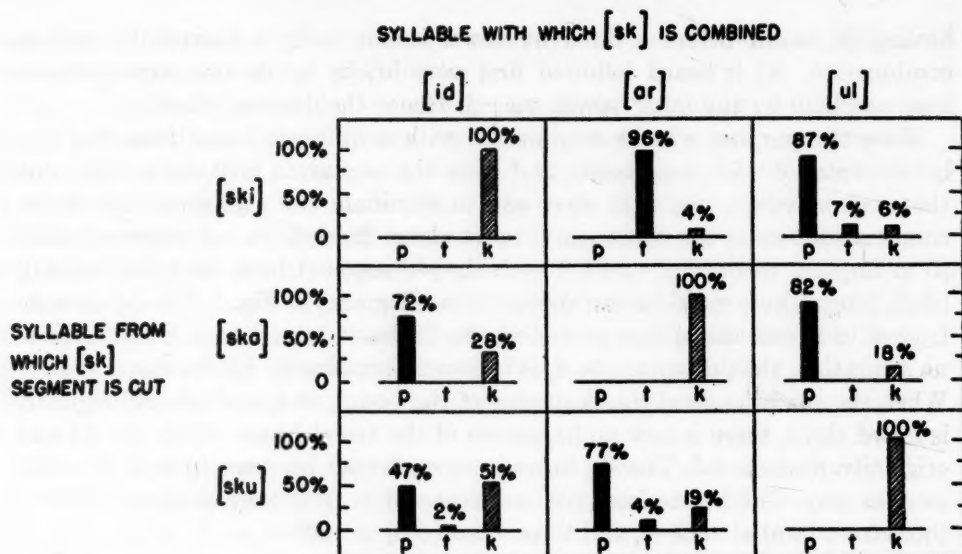


FIGURE 6

Stop consonants identified in syllables made up of initial [sk] segments and the vowels [i], [a], and [u]. The figures indicate percentages among the responses of 20 subjects to 5 samples of each combination.

the vowel following and not on the [k] alone. A [k] whose frequency is centered around 3000~, as in [ski], does not sound like [k] before [a], where the frequency of [k] in speech is centered around 1800~, or before [u], where the [k] frequency in speech is even lower, 1200~. The same holds true for a [k] at 1800~, as in [ska]: it does not sound like [k] before [i], where the [k] frequency in speech is higher, or before [u], where the [k] frequency in speech is lower. Likewise, the 1200~ burst from [sku] is not heard as [k] before [a], and is heard only partially as [k] before [i], since the [k] frequency in speech before both of these vowels is higher than 1200~.

Since the results so far show that perceptual changes occur with unaspirated [k]s but not with aspirated ones, the problem is to find some correlation in the behavior of these two kinds of [k]. Our next step was to work again with the aspirated [k] from *keep*, *cop*, and *coop*, but to cut away the aspiration and retain only the initial burst (a-b in Fig. 3), which is acoustically very similar to the burst after the [s] in [ski], etc. This burst when heard alone gives no indication of what vowel it originally preceded; it is scarcely identifiable as a *k*, but sounds like a sharp tick with no particular consonantal quality. Nevertheless, when the bursts from [ki], [ka], and [ku] are isolated, and several from [ki] (at 3000~), from [ka] (at 1800~), and from [ku] (at 1200~) are heard in succession, differences in pitch between the three groups are clearly noticeable: the listener can identify a series of high ticks, followed by some that sound lower, followed by a set that sounds lower still.

These bursts, then, were cut from the words *keep*, *cop*, and *coop* and were spliced back before [ip], [ap], and [up], to form a new set of test syllables. To

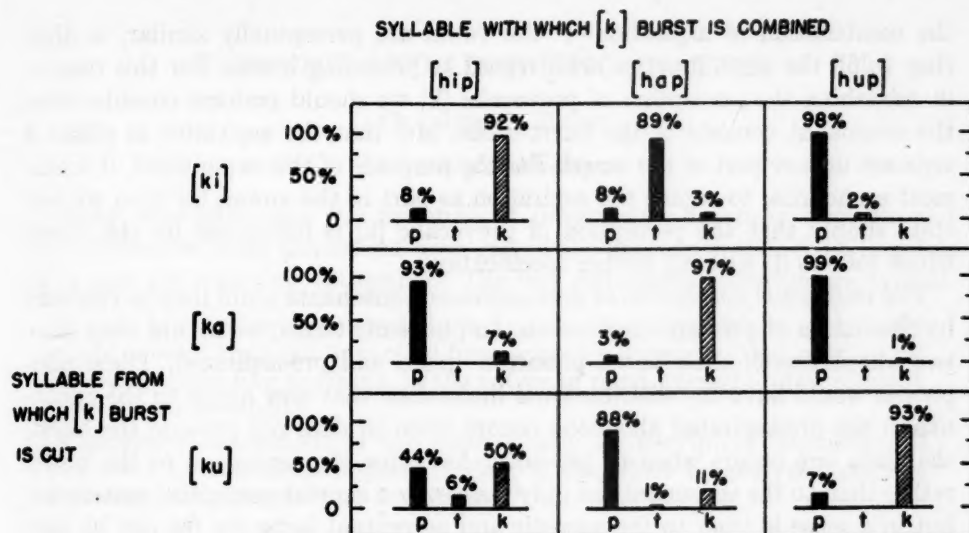


FIGURE 7

Stop consonants identified in syllables made up of initial [k] bursts and the sequences [hi], [ha], and [hu]. The figures indicate percentages among the responses of 20 subjects to 5 samples of each combination.

listeners accustomed to aspirated initial stops, however, the combination of burst plus vowel, with no aspiration between the consonantal explosion and the onset of voicing, sounded so unnatural that no consonantal identifications could be made. It was necessary, therefore, to find some way of reintroducing the aspiration that had been cut away. An impression of aspiration, as it turned out, could be produced by combining the bursts with *heap*, *hop*, *hoop* instead of with the vowels alone. Actually, the [h] as pronounced introduces too much 'aspiration', as it is considerably longer than the aspiration after a [k]. The initial portion, therefore, was cut away to shorten the [h] to about 60 milliseconds, approximately the length of the aspiration. The combination of this shortened [h] and the burst yields a completely normal-sounding stop.

Combinations were now spliced together, joining the initial bursts of the three [k]s with *heap*, *hop*, *hoop* (the [h] having been shortened). Again five samples were made of each combination and the 45 syllables were presented in random order to 20 subjects. This time, as for the unaspirated stops, the subjects' identifications included reports of all three consonants. The distribution of judgments, as shown in Fig. 7, closely matches that obtained for unaspirated [k] (Fig. 6). The burst from [ki] yielded [ki], [ta], [pu]; the burst from [ka] yielded [pi], [ka], [pu]; and the burst from [ku] yielded [pi] or [ki], [pa], [ku].

It is clear, then, that in identifying a prevocalic [k] we are often guided not only by the characteristics of the initial burst but also by the context in which the burst is heard. Our results show that this applies both to unaspirated [k] and to the initial burst of aspirated [k]; essentially these two are perceived in the same way. They are affected by what follows them—in one case the vowel, in the other case aspiration + the vowel. By the same token, the vowel and

the combination of aspiration + the vowel are perceptually similar, in that they fulfill the same function with regard to preceding bursts. For this reason, in describing the perception of prevocalic [k] we should perhaps consider that the consonant consists of the burst alone, and that the aspiration is either a separate unit or part of the vowel. For the purposes of this experiment, it seems most economical to regard the aspiration as part of the vowel; for then we can state simply that the perception of prevocalic [k] is influenced by the vowel which follows it, without further specifications.

The traditional conception of post-aspirated consonants could thus be replaced by the notion of pre-aspirated vowels. In phonemic terms, we should then have two allophones of each vowel phoneme—plain and pre-aspirated. These allophones would have the distributional limitations that now apply to the consonants: the pre-aspirated allophone occurs when [s] does not precede the burst, the plain one occurs when [s] precedes. Assigning the aspiration to the vowel rather than to the consonant not only makes for a simpler perceptual statement, but in a sense is truer to the acoustic and perceptual facts; for (as can be seen on the spectrogram in Fig. 4) the aspiration has, except for voicing, the same acoustic characteristics as the vowel, and the whole vowel is identified when only the aspiration which precedes it is heard.

Considering the consonant [k] to consist of only the burst, then, we can draw the conclusion that in identifying prevocalic [k] we are sometimes guided not only by the characteristics of the consonant, but also by the context in which this consonant is heard. When the context is changed, there may be changes in perception. The particular changes in perception which took place in our experiment agree fully with those illustrated in Fig. 2 for synthetic speech. The accompanying table shows how closely the two sets of data correspond:

—IN ACTUAL SPEECH—				—IN SYNTHETIC SPEECH—			
<i>syllables formed</i>				<i>syllables formed</i>			
[ki] burst	[ki]	[ta]	[pu]	2880~ burst	[ki]	[ta]	[pu, tu]
[ka] burst	[pi]	[ka]	[pu]	1440~ burst	[pi]	[ka]	[pu]
[ku] burst	[pi, ki]	[pa]	[ku]	720~ burst	[pi, ki]	[pa]	[ku]

MEANING AND LINGUISTIC ANALYSIS

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Many who have read the materials of present-day American linguists and have listened to their discussions have gained the impression that these linguists have cast out 'meaning' altogether.¹ The two statements following are typical.

Certain leading linguists especially in America find it possible to exclude the study of what they call 'meaning' from scientific linguistics, but only by deliberately excluding anything, in the nature of mind, thought, idea, concept. 'Mentalism' is taboo.²

A general characteristic of the methodology of descriptive linguistics, as practised by American linguists today, is the effort to analyze linguistic structure without reference to meaning.³

One can point to a variety of quotations from the writings of our American linguists, that seem to substantiate the views that these linguists not only condemn the 'use of meaning' in linguistic analysis (as indicated in the quotation from Carroll), but (as indicated in the quotation from Firth) even refuse to treat 'meaning'.

Concerning the supposed refusal to treat 'meaning', quotations such as the following have sometimes been offered in evidence.

The situations which prompt people to utter speech include every object and happening in the universe. In order to give a scientifically accurate definition of meaning for every form of a language, we should have to have a scientifically accurate knowledge of everything in the speakers' world. The actual extent of human knowledge is very small, compared to this.

The statement of meanings is therefore the weak point in language-study, and will remain so until human knowledge advances very far beyond its present state.

The signals can be analyzed but not the things signaled about. This reinforces the principle that linguistic study must always start from the phonetic form and not from meaning. ... the meanings ... could be analyzed or systematically listed only by a well-nigh omniscient observer.⁴

¹ When I set out to challenge anew the conventional uses of meaning as the basic tool of analysis in sentence structure and syntax—the area of linguistic study in which it has had its strongest hold—I felt very keenly an obligation to state as fully and as accurately as I could just what uses I did make of meaning in my procedures. This paper represents the result. Although the materials presented here use general terms, I should like to point out that my experience has dealt primarily with English. This statement of the principles and assumptions that have underlain and guided my studies of English may not have equal relevance to the problems presented by other languages.

² J. R. Firth, General linguistics and descriptive grammar, *Transactions of the Philological Society* 1950.82 (London, 1951).

³ John B. Carroll, *A survey of linguistics and related disciplines* 15 (Cambridge, Mass., 1950).

⁴ Leonard Bloomfield, *Language* 139, 140, 162 (New York, 1933).

Concerning the alleged condemnation of the 'use of meaning' in linguistic analysis the evidence usually consists of quotations like the following.

Theoretically it would be possible to arrive at the phonemic system of a dialect entirely on the basis of phonetics and distribution, without any appeal to meaning—provided that in the utterance of the dialect not all the possible combinations of phonemes actually occurred.

... our approach differs in some respects from Bloomfield's—chiefly in that Bloomfield invokes meaning as a fundamental criterion ...⁵

In the present state of morphemic analysis it is often convenient to use the meanings of utterance fractions as a general guide and short-cut to the identification of morphemes. This is especially so in the case of languages that are more or less well known to the analyst, and has been true for most morphemic work done up to now. When we are confronted, however, with a language that we know little about in terms of the relations of the linguistic behavior, it becomes clear that meaning can be of little help as a guide. The theoretical basis of analysis then becomes evident: it consists of the recognition of the recurrences and distributions of similar patterns and sequences. The analyst must therefore constantly keep in mind this theoretical basis, and must be aware that his hunches about what goes with what are really short-cut conclusions about distributional facts.⁶

In exact descriptive linguistic work ... considerations of meaning can only be used heuristically, as a source of hints, and the determining criteria will always have to be stated in distributional terms. The methods presented in the preceding chapters offer distributional investigations as alternatives to meaning considerations.⁷

Some who are counted among our linguistic scholars have so vigorously condemned all 'uses of meaning' that for many linguistic students the word *meaning* itself has almost become anathema.

On the other hand, those who oppose the recent developments in the methods of linguistic study nearly all assume, as a matter of course, that all use of every type of meaning has been rigidly excluded from the linguistic studies made in accord with these methods, and often make that assumed fact the basis of their opposition and criticism.

Sometimes it is insisted that the so-called 'repudiation of meaning' in the work of American linguists stems from Leonard Bloomfield. This view rests not upon what Bloomfield has said about meaning (which seems to have been overlooked) but upon inferences drawn from a somewhat superficial reading of his discussions of mentalism and mechanism. Thus concerning Bloomfield there have been such assertions as the following.

mechanists cannot successfully speak of meaning because they undertake to ignore certain phases of human response.

The mechanist cannot consider the ethnologic features of meaning, such as connotative colorings or social levels.

The mechanists' definition of a plant-name ... cannot ... extend beyond the

⁵ Bernard Bloch, *A set of postulates for phonemic analysis*, *Lg.* 24.5 note 8, 24.6 (1948).

⁶ George Trager and Henry Lee Smith Jr., *An outline of English structure* 54 (Oklahoma, 1951).

⁷ Zellig S. Harris, *Methods in structural linguistics* 365 note 6 (Chicago, 1951).

definition which appears in a handbook of botany: it cannot deal with ethnically conditioned features of meaning.

Bloomfield's physicalism (mechanism, anti-mentalism), as it is expressed in his linguistic writings, was not a philosophy of the universe nor a psychological system, but solely, as he insisted over and over again, a matter of the method of scientific descriptive statement:

An individual may base himself upon a purely practical, an artistic, a religious, or a scientific acceptance of the universe, and that aspect which he takes as basic will transcend and include the others. The choice, at the present state of our knowledge, can be made only by an act of faith, and with this the issue of mentalism should not be confounded. It is the belief of the present writer that the scientific description of the universe, whatever this description may be worth, requires none of the mentalistic terms, because the gaps which these terms are intended to bridge exist only so long as language is left out of account.⁸

Bloomfield strove vigorously to avoid mentalistic terms (*concept*, *idea*, etc.) in the statement of his linguistic materials and believed that every truly 'scientific statement is made in physical terms'.⁹ But his efforts to achieve statements in physical rather than 'mentalistic' terms do not lead to the conclusion that he 'ignores meaning' or that 'he takes no account of meaning'. He and many of his followers have pointed to certain uses of meaning in linguistic analysis as constituting unscientific procedures, but he and many of his followers have constantly insisted that meaning cannot be ignored. Pertinent quotations from Bloomfield's *Language* are abundant:

Man utters many kinds of vocal noise and makes use of the variety: under certain types of stimuli he produces certain vocal sounds, and his fellows, hearing these same sounds, make the appropriate response. To put it briefly, in human speech, different sounds have different meanings. To study this coordination of certain sounds with certain meanings is to study language. (27)

The study of significant speech-sounds is phonology or practical phonetics. Phonology involves the consideration of meanings. (28)

Only two kinds of linguistic records are scientifically relevant. One is a mechanical record of the gross acoustic features, such as is produced in the phonetics laboratory. The other is a record in terms of phonemes, ignoring all features that are not distinctive in the language. Until our knowledge of acoustics has progressed far beyond its present state, only the latter kind of record can be used for any study that takes into consideration the meaning of what is spoken. (85)

It is important to remember that practical phonetics and phonology presuppose a knowledge of meaning: without this knowledge we could not ascertain the phonemic features. (137)

Only in this way will a proper analysis (that is, one which takes account of the meanings) lead to the ultimate constituent morphemes. (161)

⁸ Bloomfield, *Linguistic aspects of science* 12 (Chicago, 1944).

⁹ Bloomfield, *Language or ideas?*, *Lg.* 12.92 note 6. Bloomfield followed Weiss in objecting to the term *behaviorism* and believed that *physicalism* indicated much better the essential quality of the kind of descriptive statements he sought.

Let me add to these quotations an excerpt from a letter dated 29 January 1945, written by Bloomfield to a friend of mine:

It has become painfully common to say that I, or rather, a whole group of language students of whom I am one, pay no attention to meaning or neglect it, or even that we undertake to study language without meaning, simply as meaningless sound. ... It is not just a personal affair that is involved in the statements to which I have referred, but something which, if allowed to develop, will injure the progress of our science by setting up a fictitious contrast between students who consider meaning and students who neglect or ignore it. The latter class, so far as I know, does not exist.

With Bloomfield, no serious study of human language can or does ignore 'meaning'. It is my thesis here that on all levels of linguistic analysis certain features and types of meaning furnish a necessary portion of the apparatus used. In what I shall say, however, I do not mean to defend the common uses of meaning as the BASIS of analysis and classification, or as determining the content of linguistic definition and descriptive statement, though such uses have characterized much of the study of language since the time of the Greeks. The issue is not an opposition between NO use of meaning whatever, and ANY and ALL uses of meaning.

With many others, I believe that certain uses of meaning in certain specific processes of linguistic analysis and in descriptive statement are unscientific, that is, that they do not lead to satisfactory, verifiable, and useful results. The more one works with the records of actual speech the more impossible it appears to describe the requirements of sentences (for example) in terms of meaning content. The definitive characteristics distinguishing those expressions which occur alone as separate utterances from those which occur only as parts of larger units are not matters of content or meaning, but matters of form, different from language to language. In the defining of 'subjects' and 'objects', of the 'parts of speech', of 'negation', we have not successfully approached the problems by seeking criteria of meaning content. Only as we have been able to find and to describe the contrastive formal characteristics have we been able to grasp grammatical structures in terms that make prediction possible. Structures do signal meanings, it is true, and these meanings must be described. But the meanings cannot serve successfully to identify and distinguish the structures. Not only does each structure usually signal several different meanings, but—what is more important—there is probably in present-day English no structural meaning that is not signalled by a variety of structures.¹⁰

¹⁰ The meaning 'performer of the action' is one of the meanings signaled by the structure we call 'subject'. We cannot, however, expect to DEFINE the structure 'subject' as 'performer of the action', for this meaning is signaled by a variety of other structures that are not 'subjects'. For example, in each of the following sentences, the word *committee* has the meaning 'performer of the action (of recommending)'; but only in the first sentence is this word in the structure of 'subject': *The committee recommended his promotion; His promotion was recommended by the committee; The recommendation of the committee was that he be promoted; The committee's recommendation was that he be promoted; The action of the recommending committee was that he be promoted.* The structure of 'subject', on the other hand, signals at least five different meanings—four in addition to that of 'performer'—each distinguished by special formal arrangements. See C. C. Fries, *The structure of English* 176-83 (New York, 1952).

This challenging of certain uses of meaning, as I have said, does not constitute a repudiation of all meaning in linguistic analysis. Meaning of some kind and of some degree always and inevitably constitutes part of the framework in which we operate. If that is so, then for clarity and understanding, as well as for rigorous procedure on each level of analysis, we must state as completely as possible the precise uses of each type of meaning that our procedures require and assume.¹¹

How much and just what of linguistic analysis can be accomplished without the use of any kind or degree of meaning at all? Certain meanings seem essential to making the very first step—the setting up of the material to be worked with, to be analyzed and described. There must be a 'meaning frame' within which to operate. We must know or assume, for example, (a) that the sound sequences we attempt to analyze are indeed real language utterances and the utterances of a single language, not those of several languages—a dozen or a hundred; (b) something of the range of possibilities in human language behavior and the significance of certain techniques or methods of procedure for linguistic analysis; and (c) a practical control of a language (usually another language) in which to grasp and record the processes and the results of the analysis. (Many difficulties arise from the fact that often the language of the descriptive record of the analysis differs greatly in its range of meanings and way of grouping experience from that of the language being analyzed.)

Under (b), for example, we assume that all languages have some type of meaningful units—morphemes; that all languages have bundles of contrasts of sound that function in separating or marking out or identifying these morphemes; that generally the lexical items have some contrastive formal features that make possible their classification into structurally functioning units; that all languages have formal arrangements of some sort of these structurally functioning units into contrastive patterns that have significance as structural signals of certain features of meaning; and that the linguistically significant patterns of structural arrangement are limited, much fewer than the total number of morphemes.

¹¹ To accuse linguists of deliberately refusing to treat meaning at all is to ignore the facts. The number of thoroughly trained linguists is very small indeed, and, although there are more positions for such linguists in the academic world than there were twenty years ago, there are not enough such positions to support a sufficient number of linguists to carry on the linguistic studies that are needed in every part of the field. For the past twenty-five years the really live issues that have claimed the attention of linguists have centered about linguistic structure. The new views of the significance of structure and the success of new procedures of structural analysis applied to various aspects of language have aroused such enthusiasm that most linguists have devoted their studies to these matters. Although the present center of liveliest interest in linguistics is structure rather than meaning, some portions of the general problems of meaning have received attention. (See for example Bloomfield's *Linguistic aspects of science*, 1939; *Philosophical aspects of language*, 1942; *Language or ideas?*, *Lg.* 12.89-95 [1936]; *Meaning*, *Monatshefte für deutschen Unterricht* 35.101-6 [1943].)

Even with respect to the lexicon, we should, for the record, note that the American linguists whose center of interest has been the English language have given constant support to the labors of those who have been struggling with the problems of producing the various period dictionaries proposed by Sir William Craigie. The *Oxford dictionary* itself was an effort to apply practically what was at that time called the 'new philological science'.

We must approach every linguistic analysis with a large body of 'meaning' in hand. The question is not, then, whether we can dispense with all meaning in linguistic analysis but rather, more specifically, whether we can proceed with a valid and useful analysis without some knowledge or some control (e.g. through an informant) of the meanings of the language forms which we are analyzing.

In respect to this specific question, we must face the problem of distinguishing, with some precision if possible, several varieties of meaning. I do not refer especially to the tremendous diversity of meanings which attach to the word *meaning*¹² itself, although that diversity often prevents fruitful discussion. A few quotations will show a little of this diversity:

The meaning of any sentence is what the speaker intends to be understood from it by the listener.¹³

By the meaning of a proposition I mean ... the ideas which are called to mind when it is asserted.¹⁴

What we call the meaning of a proposition embraces every obvious necessary deduction from it.¹⁵

Meaning is a relation between two associated ideas, one of which is appreciably more interesting than the other.¹⁶

To indicate the situation which verifies a proposition is to indicate what the proposition means.¹⁷

The meaning of anything whatsoever is identical with the set of expectations its presence arouses.¹⁸

Meaning is the fact of redintegrative sequence ... the evocation of a total response by a partial stimulus.¹⁹

... the word 'meaning' has established itself in philosophical discourse because it conveniently covers both reason and value.²⁰

The meaning of certain irregularities in the motion of the moon is found in the slowing up of the motion of the earth around its axis.²¹

'Meaning' signifies any and all phases of sign-processes (the status of being a sign, the interpretant, the fact of denoting, the signification) and frequently suggests mental and valuational processes as well.²²

¹² See Leo Abraham, What is the theory of meaning about?, *The Monist* 46.228-56 (1936). In this article are gathered more than fifty typical quotations from philosophical and psychological writers, in each of which the term *meaning* is used in a different sense. At the end of these quotations he concludes, 'There is clearly nothing both common and peculiar either to all the various disparate senses, or to only the more familiar among them, which itself bears, or should bear, the name "meaning" ... A subject matter for the "theory of meaning" cannot, accordingly, be obtained by abstraction from all or some of the entities revealed by the linguistic phenomenology of the term "meaning".'

¹³ A. Gardiner, The definition of the word and the sentence, *British journal of psychology* 1922.361.

¹⁴ N. Campbell, *Physics: The elements* 132 (1920).

¹⁵ C. S. Peirce, *Collected papers* 5.165 (1934).

¹⁶ F. Anderson, On the nature of meaning, *Journal of philosophy* 1933.212.

¹⁷ A. J. Ayer, Demonstration of the impossibility of metaphysics, *Mind* 1934.333.

¹⁸ C. W. Morris, Pragmatism and metaphysics, *Philosophical review* 1934.557.

¹⁹ H. L. Hollingworth, Meaning and the psycho-physical continuum, *Journal of philosophy* 1923.440.

²⁰ W. E. Hocking, *Philosophical review* 1928.142.

²¹ M. R. Cohen, *Reason and nature* 107 (1931).

²² C. W. Morris, *Signs, language, and behavior* 19 (New York, 1946).

We come then to the conclusion that meaning is practically everything. We always see the meaning as we look, think in meanings as we think, act in terms of meaning when we act. Apparently we are never directly conscious of anything but meanings.²³

As speakers of English have employed it, the word *meaning* has thus represented a great range of content. In English usage the word *meaning* has signified such diverse matters as 'the denotation of a name', 'the connotation of a symbol', 'the implications of a concept', 'the neuro-muscular and glandular reactions produced by anything', 'the place of anything in a system', 'the practical consequences of anything', 'the usefulness of anything', 'that to which the interpreter of a symbol does refer', 'that to which the interpreter of a symbol ought to be referring', 'that to which the user of a symbol wants the interpreter to infer', 'any object of consciousness whatever'. This great diversity of statement arises out of an attempt to describe the specific content of the situations in which the word *meaning* appears. Even more difficult and controversial has been the effort to classify and define the various kinds of meaning in terms of the meaning content of utterances in general. Often these various 'meanings' are grouped under two general headings: (1) the scientific, descriptive, representative, referential, denotive, cognitive kind of meaning, and (2) the emotive, expressive, non-cognitive kind of meaning.²⁴

The following quotation summarizes a portion of an analysis of utterances in terms of meaning content.

Thus in the case of certain sorts of indicative, interrogative, imperative, and optative sentence-utterances ... it seems possible to distinguish a number of factors, each of which may be and has been referred to as the meaning or part of the meaning of the utterance. These are: (1) the primary conceptual content symbolized, i.e. presented and evoked; (2) the propositional attitude (with regard to this) expressed and evoked; (3) the secondary conceptual content presented and evoked; (4) the propositional attitudes (regarding this) expressed and evoked; (5) the emotions and conative attitudes expressed; (6) the emotional tone; (7) the emotions and attitudes revealed; (8) other kinds of effects; (9) the purpose.²⁵

Let me turn away from these attempts to classify and describe the different kinds of meaning in terms of the meaning content itself to a classification based upon the kinds of signals a language uses in fulfilling its social function. I am concerned here solely with language as it provides a connection between two nervous systems. Such a use of language is not by any means limited to the com-

²³ W. B. Pillsbury, *Meaning and image*, *Psychological review* 1908.156.

²⁴ C. L. Stevenson, *Ethics and language* 33 (New Haven, 1944): 'The emotive meaning of a word is the power that the word acquires, on account of its history in emotional situations, to evoke or directly express attitudes, as distinct from describing or designating them.' Id. 73: 'The independence of emotive meaning can be roughly tested by comparing descriptive synonyms which are not emotive synonyms. Thus to whatever extent the laudatory strength of "democracy" exceeds that of "government where rule is by popular vote", the emotive meaning of the former will be independent.'

²⁵ William Fankena, *Cognitive and non-cognitive aspects of language*, *Language and symbolism* [unpublished tentative report] 5.27, 28 (1952).

which correlate with recurring sames of stimulus-situation features, and which elicit recurring sames of response features.²⁸

In general, for linguists,²⁹ the 'meanings' of an utterance consist of the correlating, regularly recurrent sames of the stimulus-situation features, and the regularly elicited recurring sames of response features.³⁰ These meanings are tied to the patterns of recurring sames of vocal sounds. In other words, the patterns of recurring sound sequences are the signals of the meanings. The meanings can be separated into various kinds or layers in accord with the several levels of patterns in the recurring sound sequences which do the signalling. Utterances will have then at least the following types or 'modes' of meaning.

(a) There is the automatic recognition of the recurrent sames that constitute the lexical items. The lexical items selected for a particular utterance are distinguished from others that might have been selected by sharp patterns of sequences of sound contrasts. One layer of the meaning of the utterance is determined and signaled by the particular lexical items selected and thus recognized. This recognition covers both the identification of the item itself by its contrastive shape, and the situation and response features with which this shape correlates in the linguistic community. If the stress and intonation, as well as the social-cultural situation, are kept constant, the meaning of the utterance *The point of this pen is bent over* differs from that of each of the following only because the lexical items differ: *The point of this pin is bent over*, *The cover of this pan is bent over*, *The top of that pen was sent over*. One of the separable layers of the meanings signaled by our utterances is thus the lexical meanings.

One other feature of lexical meaning must be noted. In addition to the recognition of the shape or forms of the lexical item itself, identified by contrastive

²⁸ A linguistic community consists of those individuals that make the 'same' regular and predictable responses to the 'same' patterns of vocal sounds. The language function is fulfilled only in so far as it is possible to predict the response features that will regularly be elicited by the patterns of vocal sound. For the discussion here I am not concerned with what might be called 'personal' meaning—the special non-recurrent or not regularly recurring response features that mark individual differences.

²⁹ For many others the meaning of a text or a sequence of utterances has often been considered a function of (a) the 'words' (as items of sound patterns which experience has connected in some way with reality), and (b) the 'context'. This context has included both the so-called 'verbal context' or linguistic context (not specified further) and the 'context of situation'—the circumstances in which the utterance occurs. Firth has pushed the analysis of 'context' much farther in his dealing with 'formal scatter' and 'meaning by collocation'. See his *Modes of meaning, Essays and studies* (English Assn.) 1951.118-49, and *General linguistics and descriptive grammar, Transactions of the Philological Society* 1951.85-7; cf. his earlier *Technique of semantics, Trans. Philol. Soc.* 1935.36-72, and *Personality and language in society, Sociological review* 1950.

³⁰ In the study of the language records of a former time we have, because of the nature of the evidence, usually had to try to arrive at the meanings of the language forms by connecting them with recurring elements of the situations in which they were used. In the study of living languages it is often possible to observe directly the responses which particular language forms elicit in a speech community. We assume that if a particular response regularly follows the utterance of a language pattern, then this pattern 'means' this response. Upon such regular recurrences rests the kind of prediction that makes possible the social functioning of language.

patterns of sound sequences, there is also the automatic (and sometimes more conscious) recognition of the distribution of each lexical item with 'sets' of other lexical items as they occur in the complete utterance unit.³¹ There is a 'lexical scatter' as well as a 'formal scatter'. It is this recognition of the particular set in which the lexical item occurs³² that stimulates the selection of the specific 'sense' in which that item is to be taken, the specific stimulus-response features for that utterance.

(b) In addition to the layer of lexical meaning there is the automatic recognition of the contrastive features of arrangement in which the lexical items occur.³³ These contrastive features of arrangement regularly correlate with and thus signal a second layer of meanings—structural meanings. The difference in meaning between the following sentences depends solely upon the contrastive features of arrangement, assuming that the stress and intonation as well as the social-cultural situation are kept constant: *There is a book on the table, Is there a book on the table.* Structural meanings are not vague matters of 'context', so called. They are sharply defined and specifically signaled by a complex system of contrastive patterns.

Together, LEXICAL MEANINGS and STRUCTURAL MEANINGS constitute the LINGUISTIC MEANING of our utterances. Linguistic meaning thus consists of lexical meanings within a frame of structural meanings—that is, of the stimulus-response features that accompany contrastive structural arrangements of lexical items.

But the linguistic meaning is only part of the total meaning of our utterances. In addition to the regularly recurring responses to the lexical items and structural arrangements there are also throughout a linguistic community recurring responses to unique whole utterances or sequences of utterances. Rip Van Winkle's simple utterance, *I am a poor quiet man, a native of the place, and a loyal subject of the King, God bless him!*, almost caused a riot, not because of the linguistic meaning signaled by the lexical items and structures, but because the unique utterance as a whole, now, after the Revolution, meant to the group that he was a confessed enemy of the newly established government. The statement, *Bill Smith swam a hundred yards in forty-five seconds*, would have not only the linguistic meaning attaching to the lexical items and the structures, but also the significance of the unique utterance as a whole, that this man had achieved a new world record. The petulant child's insistence at bed-time that he is hungry often means to the mother simply that he is trying to delay the going to bed.

³¹ 'Complete utterance unit' here means the total span of talk of one person in a single conversation or discourse.

³² As we record more specifically the details of the experience of language learning, we realize increasingly that we 'learn' not only the shape of a lexical item and the recurrent stimulus-response features that correlate with it, but also the sets of other lexical items with which it usually occurs. Perhaps when psychologists explore the 'free association' of words for an individual, they are really dealing with these sets of lexical distribution.

³³ In English the functioning units of the contrastive arrangements that signal meanings are not lexical items as such, but rather classes of these items. A variety of formal features make possible the classification of lexical items into a very small number of form classes the members of which each function as structurally the same. Linguistic analysis must discover and describe these form classes as a means of dealing with the structures themselves.

Meanings such as these I call 'social-cultural' meanings.³⁴ Linguistic meaning without social-cultural meaning constitutes what has been called 'mere verbalism'. The utterances of a language that function practically in a society therefore have both linguistic meaning and social-cultural meaning.

In general the meanings of the utterances are tied to formal patterns as signals.³⁵ In respect to linguistic meanings, I have assumed as a basis for study, that all the signals are formal features that can be described in physical terms of form, arrangement, and distribution. As I see it, the task of the linguistic analyst is to discover, test, and describe, in the system in which they occur, the formal features of utterances that operate as signals of meanings—specifically, (1) the contrastive features that constitute the recurrent *sames* of the forms of lexical units—the bundles of contrastive sound features by which morphemes are identified, (2) the contrastive markers by which structurally functioning groups of morphemes can be identified, and (3) the contrastive patterns that constitute the recurrent *sames* of the structural arrangements in which these structurally functioning classes of morphemes operate. In describing the results of the analysis, only verifiable physical terms of form, of arrangement, and of distribution are necessary. Whenever descriptive statements must depart from such formal matters, the fact is evidence of unsolved problems.

In the process of discovering just what formal features constitute the linguistic signals I can see no merit in denying ourselves access to any sources of suggestions concerning the nature of the materials that are significant. The more we know of the diverse characteristics of languages in general and of the processes that have marked language history, the more fruitfully prolific will be our suggestions. The less we know about language, the more frequently we may be led into blind alleys or follow the superstitions of the past.

In the process of testing these suggestions, however, and of proving the validity of our insights concerning the precise formal features that are significant, we need all the rigor that scientific procedure can offer. The real question centers upon the validity of the procedures through which we use techniques of distribution and of substitution. There must be some rigorous way of establishing 'sameness' of frame and 'sameness' of focus, as well as what constitutes 'difference' in each case.

In carrying out these tasks certain uses of particular kinds of 'meaning' within the utterance seem necessary and legitimate. (1) In testing the contrastive features that constitute the recurrent *sames* of lexical forms it is necessary to control in some way enough of the lexical meaning to determine whether forms showing certain differences of sound features are, for the particular language, 'same'

³⁴ The term 'social-cultural meaning' is not wholly satisfactory, but it is the best I have found to cover all the varieties of predictable meaning other than linguistic meaning. As indicated above, I have excluded from the discussion here the personal meaning of individual differences.

³⁵ This is true even of many of the varieties of social-cultural meaning—for example, the set of deviations from the norm of the sound segments that signal the meaning that a speaker is drunk, the whispering of an utterance that signals the meaning that the content of it is secret, and the unusual distribution that is the cue to a metaphor.

or 'different'.³⁶ (2) In testing the contrastive patterns that constitute the recurrent sameness of structural arrangements it is necessary to control in some way enough of the structural meaning to determine whether particular variants are substitutable, leaving the arrangement the 'same' for the language, or constitute such a change as to make the arrangement 'different'. Note that lexical meaning does not form part of the apparatus in which to test structural arrangements.

Social-cultural meanings which attach to the unique utterance as a whole or to a sequence of utterances do not seem to form any part of the frames in which to test either lexical forms or structural forms. Although a certain control of specific kinds of meaning seems to me essential for the various parts of linguistic analysis I should like to insist that as a general principle any use of meaning is unscientific whenever the fact of our knowing the meaning leads us to stop short of finding the precise formal signals that operate to convey that meaning.³⁷

³⁶ Sometimes it is insisted that we use 'differential' meaning, not 'referential' meaning. Perhaps this statement means that the linguistic analyst seeks basically to establish the fact whether two instances differ in meaning content or not. He does not need to know what that content is or in what ways the two may differ. If they differ in meaning he assumes that there must be some difference in formal features, and sets out to find, prove, and describe that difference.

³⁷ [The foregoing article was offered as a contribution to the Edgerton number of *LANGUAGE* (29:3), but reached the Editor too late to be included in that number.]

REVIEWS

Prolegomena to a theory of language. By LOUIS HJELMSLEV, translated by FRANCIS J. WHITFIELD. (Indiana University publications in anthropology and linguistics, Memoir 7 of the *International journal of American linguistics*, Supplement to Vol. 19, No. 1.) Pp. [iv], 92. Baltimore: Indiana University (under the auspices of Linguistic Society of America [and] American Anthropological Association), 1953.

Reviewed by PAUL L. GARVIN, *Georgetown University*

The views of the Prague School have been known and noted among American structuralists for some time. Now, the publication of the English translation of Hjelmslev's *Omkring sprogteoriens grundlæggelse* (Copenhagen, 1943; abbreviated *OSG*) makes another important current in structural linguistics accessible to the English-speaking public. Francis Whitfield is to be congratulated on a brilliant job of translation, and the thanks of the profession are due to whoever it was that made the publication of this Memoir financially possible. Hjelmslev himself contributed to ironing out some of the difficulties of translation, especially as regards terminology, during his stay at the Linguistic Institute at Indiana University in 1952. As a result, the English version not only is a workmanlike accomplishment, but has the authenticity of the original work.

Although the group around Hjelmslev had been known since the thirties to have some new ideas about linguistics, it was not until the publication of *OSG* that their views became the subject of more detailed review and discussion in Europe. American linguists took note of Hjelmslev's 'glossematics' especially upon publication of the Hjelmslev volume of the *Travaux du Cercle linguistique de Copenhague* (Vol. 5, 1949),¹ but the latter was 'neither designed nor suited ... to be used as an introduction to Hjelmslev's ideas' (Wells, *Lg.* 27.544). Now that the more complete source is finally available in English, one may expect a more extensive American reaction to 'glossematics'.

The *Prolegomena* deal essentially with three major interrelated topics: the general criteria for a theory of language (sections 1-9), the specifics of linguistic theory (sections 10-20), and the relationship of language to non-language (sections 21-23). The presentation is couched in exceedingly concise and rigorous terms; beginning with a new name for the science ('glossematics' instead of linguistics), it introduces its own complex though very carefully defined terminology. The original *OSG* had a list of terms with page references to definitions in the text; the translation has been provided with a separate appended list of definitions, unfortunately not in alphabetical order (83-7), and an extensive index (89-92).

I shall try to give a running commentary on Hjelmslev's system, and then to come back to some of its salient points and discuss these in more detail, to-

¹ See the reviews by R. S. Wells, *Lg.* 27.554-70 (1951); P. L. Garvin, *IJAL* 17.252-5 (1951); C. F. Hockett, *IJAL* 18.86-99 (1952); and G. L. Trager, *SIL* 8.99 (1950).

gether with some of the points that have been raised in the discussion following the appearance of *OSG*.²

1. **General criteria for a theory of language** (§§1-9, pp. 1-7; *OSG* 5-26). 'Linguistics,' says Hjelmslev, 'must attempt to grasp language, not as a conglomerate of non-linguistic (*e.g.*, physical, physiological, psychological, sociological) phenomena, but as a self-sufficient totality, a structure *sui generis*' (2, *OSG* 7). That is, linguistic theory must no longer be 'transcendent' but must become 'immanent', in order to find 'a constant which is not anchored in some "reality" outside the language' (4, *OSG* 9): language is the self-contained subject matter of linguistics. A separation of the linguistic from the extra-linguistic is not alien to American structuralism; one need only think of Trager's recent 'microlinguistics'³ as one of several attempts. Many Americans, however, including Trager, would draw the limits between the two quite differently.

The 'constant underlying the fluctuation', already referred to, is the 'system underlying the process' (5, *OSG* 11); although Hjelmslev, as we shall see below, makes no ontological assumptions about 'existence' or 'reality', he deals with the system as a primary (though not immediately observable) datum, and thus differs considerably from many Americans. I have discussed 'hocus-pocus' vs. 'God's truth' linguistics⁴ elsewhere in this journal,⁵ and have attempted to place Hjelmslev's views in that context.

In order to describe its immanent object properly, linguistic theory must observe the empirical principle; that is, the description it yields must meet the requirements of self-consistency, exhaustiveness, and simplicity, in that order of precedence (6, *OSG* 12). Well formulated though it is, the empirical principle is no linguistic novum. The requirements of self-consistency and exhaustiveness seem to flow—if I am not misinterpreting the history of linguistics—ultimately from the Neogrammarians' famous 'Ausnahmslosigkeit der Lautgesetze'. As to the criterion of simplicity, this in turn—as has repeatedly been pointed out in recent discussions—presupposes a criterion of what is simple. Hjelmslev himself attempts to remove this difficulty by stating as his simplicity principle that 'among several possible methods of procedure, that one shall be chosen that results in the simplest possible description. If several methods yield equally simple descriptions, that one is to be chosen that leads to the result through the simplest procedure' (10-1, *OSG* 18). The difficulty is not obviated, however, since a criterion still has to be given for what is the simplest description: is it the one yielding the smallest number of units, or the one yielding the simplest kind of patterning—*e.g.* the greatest symmetry—though with a larger number of units?⁶

² All reviewers of *OSG* comment on the difficulty of summarizing it; I agree. If I nevertheless attempt an outline of the *Prolegomena*, it is because the editorial policy of *LANGUAGE* generously allows much more space for reviews of important works than comparable journals in the field.

³ *The field of linguistics* 47 (*SIL*, Occasional papers, No. 1; 1949).

⁴ For these terms see F. W. Householder Jr., *IJAL* 18.260-1 (1952).

⁵ See my review of Jakobson-Fant-Halle, *Preliminaries to speech analysis*, *Lg.* 29.472-81 (1953).

⁶ If indeed simplicity is always a desideratum. See N. A. McQuown's comments on 'economy' in his review of Harris' *Methods in structural linguistics*, *Lg.* 28.497 (1952).

The object of linguistic investigation is 'the as yet unanalyzed *text* in its undivided and absolute integrity' (7, *OSG* 13). 'Text' is used by Hjelmslev in the broadest sense; it includes any corpus (or potential corpus; see below) at the disposal of the linguist, whether oral, written, or available in any other form. Since the text is the original datum, the method has to be deductive in the Hjelmslevian sense;⁷ that is, it has to consist of a progressive breaking down of the text into smaller and smaller segments, 'until the analysis is exhausted'. Harris' discourse analysis⁸ is probably the only concrete procedure suggested by any linguist that applies to the segmentation of an entire text. Here the similarity ends; as far as I can see, Harris intends this particular approach to be of limited applicability (*Lg.* 28.5), whereas Hjelmslev makes it central to his conception. Nonetheless, it seems to me (and I am supported by some colleagues in this view) that the concept of 'utterance' used in much linguistic discussion in places where Hjelmslev's 'text' might apply, is not clearly enough defined to be analytically useful. Specifically, the usual definition of the utterance as a single act of speech⁹ leaves the limits of an utterance undefined. Where does one utterance (= one act of speech) end and the next one begin? Pauses, formerly thought of as the borders of utterances, have now been included in the definition of analytical units.¹⁰ Perhaps one contribution of 'glossematics' will be to replace the utterance by the H text as the raw datum of analysis; unlike the utterance, the H text needs no H transcendent definition. As far as I can see, an act of speech can only be defined in psychological terms as a unit of speech behavior; and to my knowledge psychologists have not yet been able to define structurally the limits of a unit of behavior (in the sense which I have stressed above: ONE unit of behavior as opposed to two or three).

Linguistic theory, like any good theory, is both arbitrary and appropriate. It is arbitrary in the sense that its corollaries follow from its premises, but the theory 'is in itself independent of experience' (8, *OSG* 14): this means, to me at least, that even a theory based on premises contradicting the evidence of the senses, or totally unverifiable, may still be a 'good' theory, provided the corollaries are logically deduced from the premises; but, unless it is appropriate as well, its interest will be purely esthetic. A theory is appropriate when its premises 'satisfy the conditions for application to a large number of experimental data' (*ibid.*; here, incidentally, I would have preferred to translate the original *erfaringsdata* by something like 'empirical data', since 'experimental' all too often connotes 'verifiability by experimentation', which is of doubtful usefulness for linguistics). Linguistics therefore 'includes no existence postulate' or axiom, and its definitions must be strictly FORMAL OR OPERATIONAL, NOT REAL (12, *OSG* 20)—that is, without reference to metaphysical 'reality'. This makes it 'possible in most instances to replace pure existence postulates by theorems in the form of

⁷ Whenever the context appears to require it, I shall use a capital H before a term to indicate that it is to be understood in the framework of Hjelmslev's terminology. Thus 'H deductive' stands for 'deductive in the Hjelmslevian sense'.

⁸ *Lg.* 28.1-30, 474-94 (1952).

⁹ See B. Bloch, A set of postulates for phonemic analysis, *Lg.* 24.7 (1948).

¹⁰ See G. L. Trager and H. L. Smith Jr., *Outline of English structure* 41-52 (*SIL*, Occasional papers, No. 3; 1951). But see also the attempt by C. C. Fries to give an operationally valid definition of 'utterance unit', *The structure of English* 23 ff. (New York, 1952).

conditions' (13, *OSG* 21) or logical implications (11, *OSG* 14): not 'A is', but 'if A then B', or 'A implies B'.

Further on, Hjelmslev states what this implies operationally. Let me cite the crucial paragraph without paraphrase (10, *OSG* 17-8):

By virtue of its appropriateness the work of linguistic theory is empirical, and by virtue of its arbitrariness it is calculative. From certain experiences, which must necessarily be limited even though they should be as varied as possible, the linguistic theoretician sets up a calculation of all the conceivable possibilities within certain frames. These frames he constructs arbitrarily: he discovers certain properties present in all those objects that people agree to call languages, in order then to generalize those properties and establish them by definition. From that moment the linguistic theoretician has—arbitrarily, but appropriately—himself decreed to which objects his theory can and cannot be applied. He then sets up, for all objects of the nature premised in the definition, a general calculus, in which all conceivable cases are foreseen. This calculus, which is deduced from the established definition independently of all experience, provides the tools for describing or comprehending a given text and the language on which it is constructed. Linguistic theory cannot be verified (confirmed or invalidated) by reference to such existing texts and languages. It can be controlled only by tests to show whether the calculation is self-consistent and exhaustive.

This theoretical approach not only enables us to describe self-consistently and exhaustively the texts at our command (i.e. our actual corpus) but, in addition, 'we shall be able to construct any conceivable and theoretically possible texts in the same language' (10, *OSG* 17), because the knowledge obtained from our description 'concerns not merely or essentially the *processes* or *texts* from which it is abstracted, but the *system* or *language* on which all texts of the same premised nature are constructed, and with the help of which we can construct new texts' (*ibid.*). If we know the CODE, we can send any MESSAGE. The question which Hjelmslev leaves unanswered is: how do we know that we have the complete code, when we have derived it from a finite number of messages only? The problem arises, I believe, even if we do not worry about lexical completeness and are concerned with an exhaustive statement of grammar only.

A description meeting the requirements and yielding the results indicated above can be obtained, however, only if the analysis is 'conducted so that it conforms to the mutual dependences between [the] parts' (13, *OSG* 21-2); for 'both the object under examination and its part have existence only by virtue of these dependences' (13, *OSG* 22). This is indeed the crucial point of Hjelmslev's argument, which is developed throughout the rest of the *Prolegomena*. I shall return to this point in more detail when I discuss the Hjelmslevian conception of H form and H substance; let me here merely point out the increasing attention to relationships (which is more or less the same as H dependences) in American linguistic thinking. As early as 1948 I spoke of 'relevant relationships between the specifics',¹¹ and Pike has recently devoted a very astute paper to a relational conception of linguistics;¹² similar points have been made by others.

¹¹ Structure and variation in language and culture, *Indian tribes of aboriginal America* 216 (ed. Sol Tax; Chicago, 1952).

¹² Operational phonemics in reference to linguistic relativity, *JASA* 24.618-25 (1952).

The basic H dependences can be of three sorts: INTERDEPENDENCES, in which the two terms mutually presuppose each other; DETERMINATIONS, in which 'one term presupposes the other but not vice versa'; and CONSTELLATIONS, in which 'two terms are compatible but neither presupposes the other' (15, *OSG* 23). Of these, both interdependences and determinations have been commonly listed in American structural statements, but have not always been kept clearly separate.¹³ On the other hand, the equally significant relationship of mutual exclusion (for my own use of it, see fn. 13) is not noted in the Hjelmslevian listing at this point; closest to it comes H correlation (23-4, *OSG* 35-67), which fits into another glossematic context. Other important relationships, such as fixed relative order,¹⁴ are not treated at all.

At this point (15, *OSG* 23-4) Hjelmslev first introduces his general technique of using three parallel sets of terms for each type of relationship: one term for the relationship as such, to be applied indiscriminately to the given relationship in either the H text or the H system; one term for that relationship as occurrent in the H text; and a third term for it as occurrent in the system. This triadic system of definitions runs throughout the *Prolegomena*; it is designed to add precision to the discussion, but it also makes for great terminological complexity.

2. Specifics of linguistic theory (§§10-20, pp. 17-65; *OSG* 26-90). The details of 'glossematics' consist in a description of the H dependences occurrent in H text and H system, of the entities defined in terms of these H dependences, and of the procedures used in arriving at such a description. The order of presentation is: descriptive procedures in general, types of dependences, morphemes and phonemes, levels of language, linguistic units and procedures for isolating them, langue and parole, neutralization, interpolation of 'missing links' of the structure, extreme units of the analysis. I have used non-Hjelmslevian headings for these topics; in the following summary of this part of the *Prolegomena*, I shall attempt to confine myself, at the risk of some inaccuracies, to the most crucial of Hjelmslev's terms. Since the *Prolegomena* themselves are a summary presentation of an extensive body of conceptualization, any secondary condensation will at times assume the shape of an abbreviated abbreviation.

2.1. Descriptive procedures in general. (§10, pp. 17-20; *OSG* 21-31). The initial linguistic datum is the virgin H text; therefore the 'glossematic' procedure is H analysis: segmentation of the H text in terms of 'the uniform dependences of other objects on it and on each other' (18, *OSG* 27). 'The text is a chain [here I would have preferred the common term 'sequence' as a translation of *kæde*], and all the parts (*e.g.*, clauses, words, syllables, and so on) are likewise chains, except such eventual ultimate parts as cannot be subjected to analysis' (18, *OSG* 28). The analysis must be carried forward 'through a constantly continued partition until it is exhausted' (*ibid.*), in order to meet the requirement of an exhaustive description contained in the empirical principle. Such a set of procedures going from the whole to its parts is an H deduction; upon completing it, the reverse procedure, H induction, may be employed, leading from the parts to the whole and consisting of a series of H syntheses. 'No new results will be

¹³ Cf. my 'obligatory pairing', Kutenai III: Morpheme distributions, *IJAL* 14.171-87 (1948). I use this term for either kind of H dependence.

¹⁴ Used, for instance, to define the word in Kutenai, *IJAL* 14.171 (1948) and 17.84 (1951).

gained, but only a new point of view which it may sometimes be appropriate to adopt for the same resultants' (19, *OSG* 29).

The above reflects only partially what seems to me common linguistic practice, particularly in dealing with languages of which the linguist has no command: a corpus is obtained, which is segmented by a variety of field procedures (H deduction); the segments so obtained are in turn recombined in tentative classes (H induction). These procedures are in the nature of working hypotheses; they are tested by further segmentation of the same or an expanded corpus. Then the original classification of the segments (obtained by H induction) is revised wherever necessary, and the classes are retested by deduction, and these operations are repeated until further revisions and retesting yield no significant results. In strictly Hjelmslevian terms, field work should be all H deduction and writing a grammar all H induction: although I could nowhere find a direct statement by Hjelmslev to this effect, it seems to me to follow logically from this part of the *Prolegomena*. In practice, field work involves a continuous shifting back and forth from H deduction to H induction, with much of the preliminary H analysis accomplished by eliciting short stretches of speech from the informant.

2.2. Types of H dependences (§11, pp. 20-5; *OSG* 31-7). 'A dependence that fulfils the conditions for an analysis we shall call a *function* ... The terminals of a function we shall call a *functive*' (20, *OSG* 31). 'By a *constant* we shall understand a functive whose presence is a necessary condition for the presence of the functive to which it has function; by a *variable* we shall understand a functive whose presence is not a necessary condition for the presence of the functive to which it has function' (21, *OSG* 32). The previously stated H dependences, reinterpreted as H functions, are now redefined in terms of constants and variables; the functives of these functions are then named in terms of their particular role in the kinds of H functions which they contract.

In addition, another set of H functions is defined: H correlation is the 'either-or function ... between the members of a paradigm' (23, *OSG* 35), and H relation is the 'both-and function' (*ibid.*) between the parts of a sequence. 'And on this basis we can define a *system* as a correlational hierarchy [i.e. class of classes], and a *process* [= H text] as a relational hierarchy' (24, *OSG* 36).

There is also a function between process and system: 'The process determines [i.e. presupposes] the system' (*ibid.*). This statement strikes me as one of the cornerstones of Hjelmslev's theoretical approach; most of this section leads up to it. Let me quote his argument (*ibid.*) in full:

The decisive point is not the superficial relationship consisting in the fact that the process is the more immediately accessible for observation, while the system must be ordered to the process—discovered behind it by means of a procedure—and so is only mediately knowable insofar as it is not presented to us on the basis of a previously performed procedure. This superficial relationship might make it seem that the process can exist without a system, but not vice versa. But the decisive point is that the existence of a system is a necessary premise for the existence of a process: the process comes into existence by virtue of a system's being present behind it, a system which governs and determines it in its possible development. A process is unimaginable—because it would be in an absolute and irrevocable sense inexplicable—

without a system lying behind it. On the other hand, a system is not unimaginable without a process; the existence of a system does not presuppose the existence of a process.

This particular passage is the most lucid I have seen in the recent literature in reference to the 'hocus-pocus' vs. 'God's truth' discussion (cf. fn. 4); I shall let it stand without comment as a sign of my complete agreement.

2.3. Morphemes and phonemes (§12, pp. 25-9; *OSG* 37-44). The analyses that enter into the procedure of deduction yield inventories at each stage 'of the entities that have the same relations, *i.e.*, that can take the same "place" in the chain' (26, *OSG* 38). 'When we compare the inventories yielded at the various stages of the deduction, their size will usually turn out to decrease as the procedure goes on. If the text is unrestricted, *i.e.*, capable of being prolonged through constant addition of further parts, as will be the case for a living language taken as a text, it will be possible to register an unrestricted number of sentences, an unrestricted number of clauses, an unrestricted number of words [since the vocabulary is capable of expansion, presumably]. Sooner or later in the course of the deduction, however, there comes a point at which the number of inventoried entities becomes restricted, and after which it usually falls steadily' (26, *OSG* 38-9). That is, the number of syllable types, of types of syllable nuclei and margins, and of phonemes is restricted. This is a fundamental fact, for 'if there were no restricted inventories, linguistic theory could not hope to reach its goal, which is to make possible a simple and exhaustive description of the system behind the text. If no restricted inventory appeared however long the analysis were continued, an exhaustive description would be impossible' (26, *OSG* 39). The purpose of linguistic analysis thus is to reduce the infinity of the observable speech events to a finite inventory of structural units. The final units themselves at which Hjelmslev arrives at this stage of the presentation are sign-expressions (28, *OSG* 41-2) and *figurae* (29, *OSG* 43), which are very roughly the equivalents of morphemes and phonemes.

One-word sentences, single-phoneme morphemes, and the like are treated by Hjelmslev by introducing a 'special "rule of transference"', which serves to prevent a given entity from being further divided at a too early stage of the procedure and which ensures that certain entities under given conditions are transferred undivided from stage to stage, while entities of the same degree are subjected to division' (25, *OSG* 38).

The entities resulting from the division, and if necessary 'transferred undivided from stage to stage', are quite analogous to the well-known immediate constituents as lately treated by Fries (*Structure* 256-73); the 'transferred' entities remind one most closely of Harris' groups of morphemes substitutable for single morphemes.¹⁵

All along the line, we thus deal with a single H deduction which first yields a breakdown into complex morphemic units, then H sign-expressions (roughly morphemes) and finally *figurae* (roughly phonemes). All of this is to be carried out, as American linguists would say, on a single level of analysis. American linguistic practice of the last few years, on the other hand, has placed increasing

¹⁵ From morpheme to utterance, *Lg.* 22.161-83 (1946), esp. 165.

emphasis on the clear-cut separation of phonological and morphological levels. Even if one admits, with Pike,¹⁶ a degree of relatedness of the two, this does not deny the fact that morphological and phonological segmentations need not, and often do not, coincide: syllables are not coextensive with morphemes, contours with words, phonemic phrases with grammatical phrases, and so on.¹⁷ It is for this reason that I cannot agree with Hjelmslev's assertion that the ultimate units of an essentially morphological procedure of analysis will be phonemes.

There is no disagreement, of course, with the statement that H sign-expressions are composed of H figurae—that is, if I may equate, morphemes of phonemes—provided one keeps in mind that this is another manner of 'being composed' than that, for instance, in which a word is composed of morphemes. With this and one other reservation, one may accept Hjelmslev's final statement in this context: 'Languages, then, cannot be described as pure sign systems. By the aim usually attributed to them, they are first and foremost sign systems; but by their internal structure they are first and foremost something different, namely systems of figurae that can be used to construct signs' (ibid.). The other reservation relates to the extent to which H figurae can be used to 'construct signs': granted that the morpheme inventory (primarily rather the inventory of lexical morphemes) of a language is susceptible of continuous expansion (which I suppose is implied by the statement about 'constructing signs'), still (a) this expansion is quite limited (barring nonce words and borrowings), and (b) the phonemic inventory is also capable of expansion, though on a much smaller scale.

2.4. Levels of language (§13, pp. 29–38; *OSG* 44–55). This is probably the best-known section of Hjelmslev's argument; it has been dealt with extensively by Wells in *Lg.* 27.558–61, and has been commented on by most reviewers of *OSG*. Here we find the important Hjelmslevian dichotomies of expression versus content, form versus substance.

Hjelmslev's expression and content are roughly analogous to what linguists usually call form (not, of course, H form) and meaning. Expression and content are used by Hjelmslev 'as designations of functives that contract ... the sign function' (30, *OSG* 44). 'Expression and content ... necessarily presuppose each other. An expression is expression only by virtue of being an expression of a content, and a content is content only by virtue of being a content of an expression' (30, *OSG* 45). This is essentially and admittedly (29, *OSG* 44), an elaboration of the Saussurian conception of the sign as an 'entité ... à deux faces',¹⁸ the two aspects of which exist only by virtue of each other. It is thus a fundamentally different conception from that of a number of American linguists, who would consider only form properly linguistic, and meaning extralinguistic or, as Trager puts it, 'metalinguistic'.

Expression and content are, however, by no means to be equated with speech

¹⁶ Grammatical prerequisites to phonemic analysis, *Word* 3.155–72 (1947); More about grammatical prerequisites, *Word* 8.106–21 (1952).

¹⁷ Cf. also K. L. Pike, A problem in morphology-syntax division, *Acta linguistica (AL)* 5.125–38 (1945/49).

¹⁸ *Cours de linguistique générale* 99 (Paris, 1949).

sounds and extralinguistic reality. There exists, to be sure, an 'amorphous "thought-mass"' (32, *OSG* 48)—which the anti-mentalists among us are free to equate with extralinguistic reality as perceived by the speaker. This 'thought-mass' is the 'unformed purport ... formed differently in each language' (*ibid.*); it is content-purport which is formed by the content-form into content-substance. Likewise, we may speak of an expression-purport, which may, for instance, consist of 'the vocalic continuum and the median profile of the roof of the mouth' and be 'formed differently in different languages, depending on the specific functions of each language and ... thereby ordered to [the] expression form as expression-substance' (35, *OSG* 51).

Purport thus is the amorphous physical and psychological continuum (Trager's 'prelinguistic' for expression-purport, 'metalinguistic' for content-purport), upon which the H form is projected as an organizing principle—'the sign function and the functions deducible therefrom' (32, *OSG* 48). This segments the purport into pieces underlying each H formal entity. The purport so organized is the H substance.

Hjelmslev's example for the different H forming of content-purport in different language systems is the varying segmentation of the visible spectrum by color terminologies (33, *OSG* 48-9); his example for the different H forming of expression-purport in different language sequences is the English, German, Danish, and Japanese name of the city of Berlin (35, *OSG* 52). 'When a person familiar with the functional system of a given language (*e.g.*, his mother tongue) has perceived a content-purport or an expression-purport, he will form it in that language. An essential part of what is popularly called "speaking with an accent" consists in forming a perceived expression-purport according to predispositions suggested by functional facts in the speaker's mother tongue' (*ibid.*). Hjelmslev could have added here that misused ranges of meaning of foreign vocabulary are often due to forming a content-purport according to similar predispositions. It might also be suggested here that Whorf's famed observations on language and culture¹⁹ in Hjelmslevian terms deal with different manners in which content-purport can be formed in different languages.

2.5. Linguistic units and procedures for isolating them (§14, pp. 38-48; *OSG* 55-68). Hjelmslev formulates in two 'principles', the 'principle of economy' and the 'principle of reduction' (38, *OSG* 55-6), the requirement that each step of the analysis should lead to the registration of all the elements encountered at that particular step, and that the number of these elements should be the lowest possible. 'In order to satisfy this requirement we must have at our disposal a method that allows us under precisely fixed conditions to *reduce* two entities to one, or, as it is often put, to *identify* two entities with each other' (38, *OSG* 56). This is Hjelmslev's phrasing of the question of likeness²⁰ or sameness (*cf.* Bloch, *Lg.* 24.7-8) of two partials at any level of the analysis: what is meant by saying that we 'have "one and the same" sentence, "one and the

¹⁹ Reprinted as *Collected papers on metalinguistics* by the Foreign Service Institute, Department of State (Washington, 1951).

²⁰ *Cf.* Leonard Bloomfield, *A set of postulates for the science of language* (1926), reprinted *IJAL* 15.196 (1949).

same" clause, "one and the same" word, etc.' (39, *OSG* 56). Many specimens of each occur, 'these specimens we shall call *variants*, and the entities of which they are specimens, *invariants* ... Whereas the variants are registered mechanically by a mechanical division of the chain, at each stage of the analysis we must be able to infer from variants to invariants with the help of a specially prepared method that establishes the necessary criteria for such a reduction' (*ibid.*). We are here confronted with the familiar problem of the assignment of allophones to phonemes and of allomorphs to morphemes. The only attempt of an analogous assignment of variants to units of a higher order is Trager and Smith's of allologs to words (*Outline* 59); a generalized statement comparable to Hjelmslev's on variants and invariants is my own on items and units (*cf. fn. 11*).

As an example of reduction of variants to invariants, Hjelmslev considers the phoneme as discussed by Daniel Jones and the Prague School. He favors the Prague School criterion of distinctive opposition (41, *OSG* 59) over Daniel Jones' criterion of 'the same "place" in the chain' (40, *OSG* 58), but reproaches all of 'conventional linguistics' that H reduction 'has been worked out seriously, however, only for the *figurae* of the expression plane. But to understand the structure of a language and to prepare the analysis, it is of the greatest importance to realize that this principle be extended so as to be valid for all other invariants of the language as well, irrespective of their degree or, in general, of their place in the system' (41, *OSG* 59). The 'principle' involved in the above is what Hjelmslev has since 1938 been calling the 'commutation test': 'There is a difference between invariants in the expression plane when there is a correlation (*e.g.*, the correlation between *e* and *a* in *pet*—*pat*) to which there is a corresponding correlation in the content plane' (*ibid.*), and vice versa for content invariants (42, *OSG* 60); 'if such a relation is not present, that is precisely the criterion for deciding that there are not two different signs, but only two different variants of the same sign' (41, *OSG* 60). 'The difference between signs and *figurae* in this respect is only that, in the case of signs, it will always be the same difference of content that is entailed by one and the same difference of expression, but in the case of *figurae* one and the same difference of expression may, in each instance, entail different changes between entities of content (*e.g.*, *pet*—*pat*, *led*—*lad*, *ten*—*tan*)' (41-2, *OSG* 60).

Bloomfield's definition of sameness as 'same vocal features' and 'same stimulus-reaction features' is the H substantive analog to the above strictly H formal identification procedure. Similarly, the Prague School's definition of the morpheme as a 'minimum meaningful unit' and the phoneme as 'minimum differentiative unit' are H substantive analogs of the H formally stated difference between H signs and H *figurae*.

Morphemes and phonemes are, however, analogous to signs and *figurae* on the plane of expression only. The division of signs into *figurae* must be conducted on the content plane as well, and 'the method of procedure will be exactly the same for the content plane as for the expression plane' (42, *OSG* 61). 'Just as exchanges between *sai*, *sa*, and *si* can entail exchanges between three different contents, so exchanges between the content-entities 'ram', 'he', and 'sheep' can

entail exchanges between three different expressions. 'Ram' = 'he-sheep' will be different from 'ewe' = 'she-sheep', just as *sl* will be different from, say, *fl*, and 'ram = he-sheep' will be different from 'stallion = he-horse', just as *sl* will be different from, say, *sn*. The exchange of one and only one element for another is in both cases sufficient to entail an exchange in the other plane in language' (44, *OSG* 63-4). The analysis of the content plane will have to be continued, similarly to that of the expression plane, 'until all inventories have been restricted, and restricted as much as possible' (45, *OSG* 65).

'Till now, such an analysis of the sign-content into content-figurae has never been made or even attempted in linguistics,' says Hjelmslev (42, *OSG* 61); that is why 'the analysis of content has appeared to be an insoluble problem' (*ibid.*). Many American linguists, following Bloomfield's lead, would agree to the latter statement, although their reasons for the 'insoluble problems' in H content analysis would probably be different from Hjelmslev's, and more like Bloomfield's.²¹ As regards the initial assertion, I would point to Jakobson's *Beitrag zur allgemeinen Kasuslehre*²² as an example to the contrary, where the attempt to dissolve grammatical meanings (roughly equivalent to H sign-content) into semantic features (roughly equivalent to H content-figurae) has been made, although his procedure and conclusions remain controversial.

The 'correlation and ... exchange within a paradigm that have relation to a corresponding exchange within a paradigm in the other plane of language' (46, *OSG* 66) is a commutation; correspondingly, 'we can imagine a relation and a shift within a chain in the other plane of language' (*ibid.*)—a permutation. Hjelmslev cites no examples of permutation; I should think that the positional difference between subject and object in English is a case in point (although Hjelmslev, in one of his least lucid passages, [*ibid.*] considers subject and object in a different light): the H sign-expressions *The man loves the boy* permuted to *The boy loves the man* entail a corresponding shift in H content (in this case, grammatical meaning). In grammar at least I would be willing to venture that H commutation can be applied to morphemes with what Voegelin calls 'uniquely marked' and 'paradigmatically marked' meaning, and permutation primarily to 'inferentially marked meaning'.²³

2.6. Language and parole (§§15-16, pp. 48-54; *OSG* 67-76). Hjelmslev's point of departure is here the purport, which in itself 'is unformed; ... if boundaries should be found here, they would lie in the formation, not in the purport' (48, *OSG* 69). It is therefore, 'impossible to take the purport ... as the basis for linguistic description' (*ibid.*), and the 'non-linguistic stuff, the so-called substance' (49, *OSG* 70) has to be described independently of the form which is the only subject of linguistic description. 'Linguistics must then see its main task in establishing a science of the expression and a science of the content on an internal and functional basis' (50, *OSG* 71); it must become a discipline 'whose

²¹ *Language* 139 (New York, 1933): 'In order to give a scientifically accurate definition of meaning for every form of a language, we should have to have a scientifically accurate knowledge of everything in the speaker's world.'

²² *TCLP* 6.240-88 (1936).

²³ Linguistically marked distinctions in meanings, *Indian tribes of aboriginal American* 230-3.

science of the expression is not a phonetics and whose science of the content is not a semantics. Such a science would be an algebra of language' (ibid.). In order to 'mark its difference from previous kinds of linguistics ... we call it *glossematics*' (51, *OSG* 72).

These formulations are reminiscent of recent American points of view; Hjelmslev himself cites several American references for 'a description of categories of the expression on a purely non-phonetic basis' (50 fn. 1). In *The field of linguistics*, Trager excludes both phonetics and semantics from linguistics proper ('microlinguistics') and places them in 'prelinguistics' and 'metalinguistics' respectively; similar views, though perhaps less concisely stated, have of course been prevalent for more than a decade. Trager's criterion for inclusion into microlinguistics is 'formal' and 'distributional', which approximates H expression-form, but does not take in H content-form. Algebraic and quasi-algebraic treatments of language abound in American linguistics—one need only think of Harris, of Hockett's Potawatomi,²⁴ and of Voegelin's morphological index method;²⁵ but I am not sure whether all of them would be purely H formal by Hjelmslev's criterion. Recent techniques of identifying allomorphs without regard to phonemic shape, by distribution alone, certainly come close to being H formal in the strictest sense.

Purport is formless, however, only from the standpoint of linguistics; the 'non-linguistic analysis of the purport ... leads ... to ... a "form" essentially of the same sort as the linguistic "form", although of non-linguistic nature' (51, *OSG* 72), and finally 'to the recognition of a non-linguistic hierarchy, which has function to the corresponding linguistic hierarchy' (51, *OSG* 73). The non-linguistic hierarchy, called 'linguistic usage', is said to 'manifest' the linguistic hierarchy, called 'linguistic schema'. This is reminiscent of the conception of allophones 'implementing' phonemes in Jakobson-Fant-Halle's *Preliminaries to speech analysis*;²⁶ it is also, although Hjelmslev does not say so here, the closest Hjelmslevian analog to the Saussurian langue-parole distinction (*Cours* 28–32).²⁷

Hjelmslev subsequently discusses 'variants in the linguistic schema' (§16, pp. 52–4; *OSG* 73–6), which he divides in the customary manner into bound, called 'varieties', and free, called 'variations'. This division is important not only on the plane of expression, but on the plane of content as well. 'All so-called contextual meanings manifest [NB] varieties, and special meanings beyond these manifest variations' (52, *OSG* 14). This recognition of contextual and free variation of linguistic meaning may aid in a linguistic statement of meaning by leading towards an isolation of the H invariants of meaning, i.e. possible structural meaning units. Karl Bühler's 'feldfremd' (roughly context-derived) characteristics of the sign²⁸ are a step in that direction; American linguists have

²⁴ *IJAL* 14.1–10, 63–73, 139–49, 213–25 (1948).

²⁵ As exemplified in my Kutenai III, *IJAL* 14.171–87 (1948), and in W. L. Wonderly's Zoque III, *IJAL* 17.137–62 (1951).

²⁶ MIT technical report No. 13 (1952); see also my review, cited in fn. 5.

²⁷ See also R. S. Wells, De Saussure's system of linguistics, *Word* 3.15–8 (1947).

²⁸ *Sprachtheorie* 183–4 (Jena, 1934).

in general focused on the contextual variability of meaning rather than any possible invariance (cf. Fries, *Structure* passim, for listings of H varieties on the plane of content). Recently, Henri Frei²⁹ has discussed an analog of H content variants in a strictly Saussurian setting, under the heading of 'variétés sémantiques'.

A point on which Hjelmslev differs from many Saussurians is the assignment of variants to the 'linguistic schema'; phonetic variants, for instance, are called 'sons de la parole' by Trubetzkoy.³⁰ Hjelmslev's point of view is not quite clear to me either; he calls his variants 'specimens' of the invariants (39, *OSG* 56), which would seem to imply that only the invariants are properly H formal, i.e. part of the schema, whereas the variants are H substantive instances of the occurrence of the invariants.

2.7. Neutralization (§§17-18, pp. 54-60; *OSG* 76-83). This favored concept of the Prague school³¹ appears in Hjelmslev's glossematics under the name 'suspension' and is defined as 'the fact that the commutation between the two invariants may be suspended under given conditions' (56, *OSG* 78), much as happens with the neuter nominative and accusative in Latin, or with final devoicing in Danish (or, for that matter, in German or Czech). A 'suspension' in the sequence is called an 'overlapping'; 'the category that is established by an overlapping we call (in both planes of language) a *syncretism*' (56, *OSG* 79). On the phonemic level, the closest to the Hjelmslevian concept is the 'archiphoneme' of the Prague School; but H suspension includes the morphological analog as well, as evidenced by the Latin example. The only parallel to this that I can think of is my own 'neutralisation grammaticale';³² the Prague School has to my knowledge not extended the 'neutralization' concept beyond phonemics, and American linguists as a rule disregard neutralization altogether.³³

The conditioning factor of an overlapping is a variant. For instance, 'the entity whose presence is a necessary condition for the overlapping between *p* and *b*, is the variety of central part of a syllable that is solidary with a following *p/b*' (ibid.); the variant is then said to 'dominate' the overlapping. I understand this to mean that overlapping occurs in a given position, which by Hjelmslev is stated in terms of the particular positional variant adjacent to the position in question.

On this basis, Hjelmslev differentiates between 'an *obligatory* dominance ... in which the dominant in respect of the syncretism is a *variety* [i.e. positional variant], and a *facultative* dominance ... in which the dominant ... is a *variation* [i.e. free variant]' (57, *OSG* 80). This avoids giving a 'real' definition which for

²⁹ Langue, parole et différenciation, *Journal de psychologie* 1952.137-57, esp. 144-5.

³⁰ *Grundzüge der Phonologie* = *TCLP* 7 (Prague, 1939). Here cited from the French translation by J. Cantineau, *Principes de phonologie* 5 (Paris, 1949). All page references are to the latter version.

³¹ Cf. Trubetzkoy, *Principes* 246-61; A. Martinet, Neutralisation et archiphonème, *TCLP* 6.46-57 (1936).

³² L'obviation en Kutenai—échantillon d'une catégorie grammaticale amérindienne, *BSL* 47.1.212 (1951).

³³ An extreme opinion: W. B. S. Smith, *SIL* 8.6 (1950).

'concepts like facultative and obligatory would ... necessarily presuppose a concept of sociological norm, which proves to be dispensable throughout linguistic theory' (ibid.).

Here is one statement of Hjelmslev's with which one can take issue. Most American linguists have accepted as one of their basic assumptions the statement that language is part of culture;³⁴ this implies some assumption of a 'sociological norm'—'cultural' would probably be the preferred adjective—determining the habit pattern which constitutes or underlies speech behavior. Hjelmslev's general point of view, incidentally, is not irreconcilable with the concept of 'norm'. I have recently pointed out (*Lg.* 29.474–5) that linguistic structure can be considered a set of 'social norms' in the sense in which the social psychologists use the term; as far as I can see, H form is quite analogous to 'structure' in this sense, and hence the equation H form = 'social norm' is not impossible. The question whether such an equation can be dispensed with is not soluble, I believe, within the Hjelmslevian system; it hinges on the larger problem of the place of linguistics in the sciences. I shall return to this later.

2.8. Interpolation of 'missing links' of the structure (§§19–20, pp. 60–1; OSG 83–6). Since 'the analysis consists in the registration of functions ... the possibility must be foreseen that [this] may, by virtue of the solidarity [i.e. mutual presupposition] between function and functive, oblige us to interpolate certain functives which would in no other way be accessible to knowledge. This interpolation we call *catalysis*' (60, OSG 84–4). The specific function which is here concerned is H determination, i.e. the presupposition of one term by another but not vice versa. Hjelmslev's example is the Latin preposition *sine*, which governs the ablative: 'the presence of an ablative in the text is a necessary condition for the presence of *sine* (but not *vice versa*)' (60, OSG 84). If an ablative form is absent by some 'incalculable accident in the exercise of language' (ibid.), such as a 'damaged ms., this 'prerequisite for *sine* may be interpolated' (ibid.), provided no more is interpolated 'in the text than what there is clear evidence for' (ibid.). This will in most cases be 'not some particular entity, but an irresoluble syncretism [i.e. one of which the specific representative cannot be inferred] between all the entities that might be considered possible in the given "place" in the chain' (61, OSG 85). In the case of *sine*, this would be an ablative, but not some particular ablative. Catalysis applies not only to cases of textual reconstruction as implied above, but also to 'both aposiopesis and abbreviation' (60, OSG 84), and, I suppose, any other kind of 'incomplete utterance'. I have proposed an alternative interpretation of 'elliptic speech' or 'incomplete verbal responses' in an article called *Referential adjustments and linguistic structure*,³⁵ namely in terms of the 'field-derived characteristics' of the linguistic sign (i.e. those inferred from the context and the speech situation); this is based on Bühler's conceptual system (cf. fn. 28) and is undoubtedly H transcendent. Later (and admittedly influenced by OSG) I used interpolation to analyze 'syntactically isolated' cases of a morpheme that normally presupposes a

³⁴ Most emphatically Hockett, *Language 'and' culture: A protest*, AA 52.113 (1950).

³⁵ *AL* 4.59 (1944), issued 1948.

governing construction of some sort ('l'obviateur syntaxiquement isolé', *BSL* 47.203-4); it is a useful concept.

Catalysis is, however, not just another useful concept, but the 'kernel of [the] procedure' (61, *OSG* 86) by which, through textual analysis, H form is recognized 'behind the substance immediately accessible to observation by the senses, and behind the text a language (system)'. The form is interpolated on the basis of the substance, and the system on the basis of the text. This follows clearly from the two H determinations that substance presupposes form (which to me is implied by the fact that the purport is FORMED into substance; 32, 35, *OSG* 48, 51), and that the process presupposes the system (so stated by Hjelmslev, 24, *OSG* 36).

This point is not much elaborated by Hjelmslev, but deserves attention since it has bearing on the recent 'hocus-pocus' versus 'God's truth' discussion. There can probably be general agreement on the assertion that the linguist indeed interpolates, or infers, a linguistic system (an H system and an H form) on the basis of observed speech behavior or written records (an H text and an H substance). The discussion I believe revolves around the status of this inference: is it based on the linguist's judgment only, or is it implicit in the data themselves? If one accepts the non-uniqueness of linguistic solutions, then the linguistic system has only 'hocus-pocus' status and varies with the investigator; this is certainly not Hjelmslev's point of view, since his 'catalysis' is based on the logical primacy (if I am interpreting H determination correctly) of H form and H system respectively. To Hjelmslev of course, since theory includes 'no existence postulate' (8, *OSG* 14), logical primacy does not imply 'existence' or 'reality' of any sort; but this is a matter of epistemology, and with a different theory of cognition the implication can be asserted.

Finally, H catalysis implies to me a statement of linguistic predictability: given a set of conditions (a context, stretch of speech, H substantive manifestation), we can predict which class of possible interpolated entities can occur, and which cannot. If prediction is the major desideratum for science, linguistics can strengthen its case for being considered a science by justly claiming for itself the ability to predict patterns on the basis of seemingly random occurrences of noise, and to predict pieces of pattern on the basis of previously ascertained pieces of pattern.

Extended beyond the limits of synchrony (treated in a Hjelmslevian manner or otherwise), something akin to catalysis has been used to almost predict linguistic change on the basis of properties of the system which indicate the absence of PRESUPPOSING elements ('gaps in the pattern') and which thus create the conditions for interpolating units not yet there.³⁶ Units soon to be lost,³⁷ I suppose, could be 'predicted' by a sort of 'reverse catalysis' based on the

³⁶ Cf. A. Martinet, Function, structure, and sound change, *Word* 8.1-32 (1952); Roman Jakobson, Prinzipien der historischen Phonologie, *TCLP* 4.247-67 (1931).

³⁷ Cf. Martinet, *Word* 8.1-32 (1952); and a recent case study by Josef Vachek, Foném h/x vývoji angličtiny [The phoneme h/x in the development of English], *Sborník prací Filozofické fakulty Brněnské university, Ling. series* 1.121-34 (1952).

absence or weakenings of PRESUPPOSED elements or functions in the system. Such treatments of linguistic history are as yet far from generally accepted, but in this context it seems important that they can be fitted into the Hjelmslevian scheme of things linguistic.

2.9. Extreme units of the analysis (§20, pp. 61-5; *OSG* 86-90). The linguistic H deduction must be carried out with equal exhaustiveness at every step: 'the analysis must move from the invariants that have the greatest extension conceivable to the invariants that have the least extension conceivable, so that between these two extreme points as many derivative degrees are traversed as possible' (62, *OSG* 87). In this, H analysis 'differs essentially from the traditional one. For the latter is concerned neither with those parts of the text that have very great extension nor with those that have very small extension. An explicit or implicit tradition has it that the work of the linguist begins with dividing sentences into clauses, while it is thought possible to refer the treatment of large parts of the text, groups of sentences and the like, to other sciences' (*ibid.*).

The criticism applies in full to prestructuralist linguistics, less so to the Prague School and (more recently) to American structuralists. The ties of the Prague linguists to literary criticism and the analysis of the 'language of literature'³⁸ lead them to deal with entire texts. Harris' discourse analysis of course postdates *OSG*; Harris deals with whole texts, but not with texts 'of a very large or unrestricted extension' (63, *OSG* 87). A. A. Hill, who in this country has approached literature from a linguistic point of view, has likewise mainly dealt with specific texts of limited extension.³⁹

The important question is, however, not one 'of practical division of labor, but of the placing of objects by their definitions' (*ibid.*). That is, 'unrestricted texts' have to be subjected to H analysis (which is to say, linguistic analysis proper) as well as parts of the text, such as sentences, clauses, and the like.

The various attempts by other linguists to deal with large or even unrestricted texts (the Prague School even with entire literary traditions) do not use a treatment comparable to H analysis—and, I believe, with good reason. I think it can properly be asserted that linguistic analysis in the strict sense stops at the sentence boundary; this is certainly implicit in Bloomfield's definition of the sentence as a 'minimum free utterance', which has at least in part been empirically validated by Fries (*Structure* 9-28). Anaphoric and similar dependences are often cited as evidence of the grammatical relationship of one sentence to another; it can be countered, however, that such dependences can exist quite similarly between a single sentence and the extralinguistic situation (e.g. anaphoric 'he' can refer to either a preceding sentence or a situation). A detailed analysis of a set of syntactic relationships in Kutenai has led me, from a vague definition of the sentence as some sort of independent unit, to defining it speci-

³⁸ Cf. Jan Mukařovský, *Jazyk spisovný a jazyk básnický* [Standard language and poetic language], *Spisovná čeština a jazyková kultura* 123-56 (Prague, 1932); *id.*, *Kapitoly z české poetiky* [Chapters from Czech poetry] (3 vols.; Prague, 1948). For some recent comment in English, see Viktor Erlich, The Russian formalist movement, *Partisan review* 1953.282-96.

³⁹ Cf., most recently, A sample literary analysis, *Georgetown University monograph series on languages and linguistics* 4.87-93 (1953).

fically as the maximum framework for one of the significant grammatical agreements in the language (*BSL* 47.177-8 and 207). Whereas, therefore, Hjelmslev admits an initial partition of an unrestricted text on the basis of all three of his fundamental H dependences (interdependence, determination, constellation), I would venture to say that the sentences in a text are in a relationship of H constellation only: none of them presupposes another, since each is potentially at least a self-contained unit. This does not mean to deny that in large, specially organized texts, such as essays and poems, dependences other than constellation exist beyond the framework of the sentence; but the basic function in terms of which they are constituted has, by the Prague linguists for instance, been differentiated from the ordinary communicative sign function and identified as the special 'poetic function' (cf. Mukařovský, *Jazyk* 126-8).

In the other direction, linguistic analysis should likewise 'lead ... to entities of smaller extension than those which up to now have been viewed as the irreducible invariants. This is true not merely in the content plane, where we have seen that conventional linguistics is very far from having carried the analysis to the end, but also in the expression plane' (63, *OSG* 88). On both planes, the ultimate units of the segmental analysis will be 'taxemes', which on the expression plane are roughly equivalent to phonemes. These taxemes can, however, be further partitioned: 'when a taxeme inventory is "set up into a system" the logical consequence is a further partition of the individual taxeme' (64, *OSG* 89): the resulting 'end-points' are 'glossemes', 'and if we assume that one taxeme of expression is usually manifested by one phoneme, then a glosseme of expression will usually be manifested by a part of a phoneme' (*ibid.*).

Although Hjelmslev—to judge from his statement that 'traditional [analysis] ... is [not] concerned ... with those parts ... that have very small extension' (62, *OSG* 87)—does not seem to think so, it strikes me that his 'glossemes of expression' correspond rather closely to the American 'simultaneous components'⁴⁰ or the Prague School's 'distinctive features'.⁴¹ 'Distinctive features' differ from glossemes, of course, by being H substantive in character (as Prague School phonemics are in general);⁴² nonetheless, they constitute ultimate units of a comparable H derivative degree.

One part that remains totally unclear to me in the discussion of 'taxemes' and 'glossemes' is where the previously defined 'figurae' fit in. They seem to be the same as 'taxemes', but are they?

3. The relationship of language to non-language (§§21-23, pp. 65-82; *OSG* 90-112). The *Prolegomena* conclude with a discussion which—under such names as 'metalinguistics', 'exolinguistics', 'language, culture, and personality'—is now again in the center of American linguistic interest. Having begun with the clarion call for an 'immanent' linguistics, Hjelmslev now attempts to integrate it 'into a more general epistemological setting' (65, *OSG* 90).

⁴⁰ Cf. Z. S. Harris, Simultaneous components in phonology, *Lg.* 20.181-205 (1944).

⁴¹ Cf. Jakobson-Fant-Halle, *Preliminaries*, and my review thereof.

⁴² See, for instance, J. Vachek, Yaleská škola strukturalistické fonologie [The Yale school of structuralist phonology], *Slovo a slovesnost* 11.36-44 (Prague, 1949), with strong emphasis on the phonetic characteristics of phonemes.

First of all, since linguistic theory 'is so constructed that linguistic form is viewed without regard for "the substance" (purport)' (65, *OSG* 91), it can be applied to 'any structure whose form is analogous to that of a "natural" language' (*ibid.*)—that is, to other sign systems.

Second, from this 'it further follows that "substance" cannot in itself be the definiens for a language' (66, *OSG* 91): the definiens is form. 'We must be able to imagine as ordered to one and the same linguistic form substances which ... are essentially different; the arbitrary relation between linguistic form and purport makes this a logical necessity' (*ibid.*). Thus, 'it is possible to replace the usual sound-mimicry-gesture substance with any other that offers itself as appropriate' (66, *OSG* 92), such as writing and flag codes.

Hjelmslev counters the objection that writing is secondary to speech by stating that 'the fact that a manifestation is "derived" in respect of another does not alter the fact that it is a manifestation of the given linguistic form' (67, *OSG* 93): the objection that 'a different "substance" is accompanied in many instances by a changed linguistic form' (66, *OSG* 92) is considered 'irrelevant because it does not alter the general fact that a linguistic form is manifested in the given substance' (67, *OSG* 93), but is held 'interesting ... in showing that different systems of expression can correspond to the same system of content' (*ibid.*).

Because of the arbitrary relation of H form and H substance, 'various phonetic usages and various written usages can be ordered to the expression system of one and the same linguistic schema' (*ibid.*); there is an apparent contradiction between this statement and the immediately preceding one regarding 'different systems of expression' corresponding to 'the same system of content'. Thus, purely phonetic changes can occur without affecting the 'expression system', and purely semantic changes without affecting the 'content system'. 'Only this way is it possible to distinguish between *phonetic shifts* and *semantic shifts* on the one hand, and *formal shifts* on the other' (67, *OSG* 94).

All this is possible because linguistic analysis is to be a 'general calculus' where 'there is no question whether the individual structural types are manifested, but only whether they are manifestable and, *nota bene*, manifestable in any substance whatever' (68, *OSG* 94); therefore linguistic theory must 'consider as [its] subject, not merely "natural", everyday language, but any semiotic—any structure that is analogous to a language' (*ibid.*), that is, any sign system. Sign systems other than 'natural' languages presumably differ in that they are 'manifested', or are capable of being 'manifested', in a different H substance, while essentially having H forms analogous to those of 'natural' languages. What Hjelmslev intends by his insistence on 'any substance whatever' is not clear to me, since it would follow from the glossematic principles (or should be included in them, whichever you prefer) that not every substance is capable of being formed linguistically, or being formed in a manner analogous to the linguistic.

The inclusion of 'natural' language among the semiotics is admittedly a development of de Saussure's conception of linguistics as part of 'la sémiologie' (*Cours* 33); but Hjelmslev seeks to rid this more general discipline of the 'essen-

tially sociological and psychological basis' (69, *OSG* 96) on which de Saussure had placed it, and to establish it 'on an *immanent* basis' (ibid.). This will lead to 'both the possibility and the necessity ... of an intimate collaboration between linguistics and logistics' (ibid.). 'In a new sense, then, it seems fruitful and necessary to establish a common point of view for a large number of disciplines [dealing with various semiotic aspects of culture] ... concentrated around a *linguistically defined setting of problems*' (ibid., italics mine).

There is a remarkable similarity between this and Trager's equally language-centered conception of 'metalinguistics': 'Metalinguistics is then a greatly expandable field of science,' says Trager, 'which can come to serve as the means whereby linguistics, and language, can become the tool for the scientific description (= measurement) of all phenomena in the universe. Its data will serve to connect the physical and biological sciences on the one side with linguistics, and the latter with the other social sciences (and humanities) on the other side' (*Field of linguistics* 8). Certainly Hjelmslev has in this respect departed very far from de Saussure, who modestly subordinates linguistics to the more general discipline; he has done this because his is 'a theory which is minimally specific' (65, *OSG* 91) and therefore has wider applicability than the object for which it was originally designed, 'natural' language.

What, then, is the place of language among all the 'semiotics', and what differentiates 'semiotics' from 'non-semiotics'?

Language is the one semiotic 'into which all other semiotics may be translated—both all other languages and all other conceivable semiotic structures. This translatability rests on the fact that languages, and they alone, are in a position to form any purport whatsoever' (70, *OSG* 97). As Sapir puts it, 'language is a perfect symbolism of experience'.⁴³ This 'remarkable quality' of language is probably based on 'the unlimited possibility of forming signs and the very free rules for forming units of great extension' (ibid.).

The criterion that separates semiotics from closely similar non-semiotics ('quasi-semiotics') is 'whether an exhaustive description of them necessitates operating with two planes [as is the case with a true semiotic], or whether the simplicity principle can be applied so far that operation with one plane is sufficient' (71, *OSG* 99). The latter situation obtains if the plane of expression and the plane of content have 'a one-to-one relation between the functives of the one plane and the functives of the other' (72, *OSG* 99)—that is, if they are 'conformal'. Semiotics, including languages, are non-conformal; an object can be tested for being a semiotic by the 'derivative test', which establishes whether its possible 'planes' are 'conformal'. Languages and some other semiotics give negative results for the 'derivative test'; on the other hand, 'the derivative test has positive results for many of the structures which modern theory has favored calling semiotics ... [such as] pure games, in the interpretation of which there is an entity of content corresponding to each entity of expression (chess-piece or the like), so that if two planes are posited the functional net will be entirely the same in both' (72, *OSG* 100). For the latter type of structures the

⁴³ *Selected writings of Edward Sapir* 12 (ed. D. G. Mandelbaum; Berkeley and Los Angeles, 1949).

term 'symbolic systems' is proposed (*ibid.*); their basic characteristic is that they do not allow 'the further analysis into *figurae* that is characteristic of [the] signs [of a semiotic]' (73, *OSG* 100-1).

'Natural' language is a '*denotative semiotic*, by which [is meant] a semiotic none of whose planes is a semiotic' (73, *OSG* 101); there exist also 'semiotics whose expression plane is a semiotic and semiotics whose content plane is a semiotic. The former [shall be called] *connotative semiotics*, the latter *metasemiotics*' (*ibid.*).

'Connotative semiotic' serves as the heading under which Hjelmslev discusses the identifying function of style, social dialect, language, idiolect, and the like: 'it is the semiotic schema(*ta*) and usage(s) which we designate as the Danish language that are *expression for* the connotator [i.e. the content plane of a 'connotative semiotic'] "Danish". Likewise it is the semiotic schema(*ta*) and usage(s) which we designate as the linguistic physiognomy [roughly idiolect] N.N. that are *expression for* the real physiognomy N.N. (that person), and correspondingly in all other cases' (76, *OSG* 105). The 'connotative semiotic' can thus be described in non-Hjelmslevian terms as the linguistic, dialectal, idiolectal pattern itself, functioning as a signal of the non-linguistic cultural fact associated with it; the identification of a person's regional origin (or identity) by his speech is an example of a 'connotative semiotic' in its 'usage' (cf. Trager and Smith, *Outline* 82-6).

A 'metasemiotic', on the other hand, is roughly equivalent to the 'metalanguage' of the logicians, that is, 'a semiotic that treats of a semiotic; in our terminology this must mean a semiotic whose content is a semiotic. Such a metasemiotic linguistics itself must be' (*ibid.*).

Using the concept of 'operation', Hjelmslev then subdivides 'semiotics' into 'scientific semiotics', which are operations, and 'non-scientific semiotics', which are not; a 'connotative semiotic' is now redefined as 'a non-scientific semiotic one or more (two) of whose planes is (are) (a) semiotic(s), and a *metasemiotic* as a scientific semiotic one or more (two) of whose planes is (are) (a) semiotic(s). The case that usually occurs in practice is, as we have seen, that *one* of the planes is a semiotic' (77, *OSG* 106).

This allows Hjelmslev to 'define a *meta-(scientific semiotic)* as a metasemiotic whose object semiotic is a scientific semiotic (a semiotic that enters as a plane into a semiotic is said to be the *object-semiotic* of that semiotic)' (*ibid.*); this brings another aspect of symbolic logic within the reach of glossematics. Furthermore, 'in conformity with Saussure's terminology we can define a *semiology* as a metasemiotic whose object semiotic is a nonscientific semiotic. And finally, we can use the designation *metasemiology* of a meta-(scientific semiotic) whose object semiotics are semiologies' (*ibid.*). Thus, the operations of which mathematics consists would be a scientific semiotic, and the 'language of mathematics' would be a meta-(scientific semiotic); language, if I understand Hjelmslev correctly, is a non-scientific semiotic, and therefore linguistics is a semiology. This leaves us with the problem of more closely defining the 'metasemiology' of linguistics.

'Usually,' says Hjelmslev, 'a metasemiotic will be (or can be) wholly or partly

identical with its object semiotic. Thus the linguist who describes a language will be able to use that language in the description' (ibid.). In order to avoid a repetition of the results of 'semiology' (in this case, linguistics) by 'metasemiology' (should I say H metalinguistics?), the latter 'must therefore direct its interest, not toward the language, already described by semiology, which semiology uses, but toward the eventual modification of it or additions to it which semiology has introduced to produce its special jargon ... the special *terminology* of semiology' (78, *OSG* 107). What Hjelmslev is driving at, then, is a linguistic analysis of the terminology of, say, linguistics, which ultimately means 'to subject the minimal signs of semiology ... to a relational analysis according to the same procedure that is generally prescribed for the textual analysis' (79, *OSG* 108). This means that the final entities of the analysis, which are unanalyzable within the bounds of the 'semiology' (i.e. of linguistics) now become analyzable in 'metasemiological' terms: 'the ultimate variants of a language are subjected to a further, particular analysis on a completely physical basis. *In other words, metasemiology is in practice identical with the so-called description of substance*' (79, *OSG* 109), namely, 'of the *things* which appeared for semiology as irreducible individuals (or localized entities) of content and of the *sounds* (or written marks, *etc.*) which appeared for semiology as irreducible individuals (or localized entities) of expression' (80, *OSG* 109). This is analysis of cultural referents on one hand, and of phones or graphs on the other; it is to be continued until the 'sought-for clarification by reasons and causes must give way to a purely statistical description as the only possible one: the final situation of physics and deductive phonetics' (80, *OSG* 110). 'Metasemiology' thus becomes comparable to both Trager's 'prelinguistics' and perhaps, to coin a new term in Trager's vein, a kind of physical 'pre-anthropology'; the H deduction by which Hjelmslev arrives at it is not quite clear to me.

To each 'connotative semiotic' 'can and must also be added ... a metasemiotic, which further analyzes the final objects of the connotative semiotic' (ibid.). This metasemiotic will treat the 'content-purports attached to nation (as content for national language), region (as content for regional language), ... personality (as content for physiognomy ...) ... etc.' (ibid.); the social sciences will hence have the status of 'metasemiotics' of 'connotative semiotics'.

Thus Hjelmslev's theory, which first eliminates all extralinguistic considerations, reintroduces them by an extension of the H formal analysis to 'connotative' and 'metasemiotics': 'all those entities which in the first instance, with the pure consideration of the schema of the object semiotic, had to be provisionally eliminated as non-semiotic elements, are reintroduced as necessary components into semiotic structures of a higher order. Accordingly, we find no non-semiotics that are not components of semiotics [by being, for instance, their content], and, in the final instance, no object that is not illuminated from the key position of linguistic theory' (ibid.). This theory, however, is no longer properly linguistic in the sense that most American linguists attach to this word, but is probably closer to the logistician's conception of 'science as a language'.⁴⁴

⁴⁴ Cf. Rudolf Carnap, *The logical syntax of language* 281-4 (New York, 1937).

Hjelmslev ends the *Prolegomena* on a Hegelian note: 'instead of hindering transcendence, immanence has given it a new and better basis; immanence and transcendence are joined in a higher unity on the basis of immanence' (81, *OSG* 112)—thesis, antithesis, and synthesis!

4. Critique of glossematics. *OSG* has been reviewed and discussed by a number of European linguists⁴⁵ and by one American linguist then still in Europe.⁴⁶ Of these, Vogt takes an essentially wait-and-see attitude, noting the significance of the work and holding his judgment in abeyance until one can 'voir la théorie appliquée à l'analyse d'un état de langue donné' (98). Hammerich rejects glossematics entirely, considering it a 'recherche du temps perdu' (21). Other reviewers, while hailing *OSG* as an important contribution, complimenting its logical cohesiveness, and deploring its terminological complexity, take issue with Hjelmslev on the following points: the arbitrary relation of form and substance (Fischer-Jørgensen, Martinet, Hintze, Bazell), content *figurae* (Martinet, Fischer-Jørgensen, Bazell), rejection of the social norm (Hintze, Skalička). All of these points, I believe, deserve some comment; in addition, as Wells has already observed (*Lg.* 27.555), Hjelmslev, to be properly understood, must be viewed in his Saussurian setting.

4.1. Hjelmslev as a Saussurian. The three schools of thought that have, in part or entirely, taken their inspiration from de Saussure's *Cours*, have made different phases of it their point of departure: the Geneva group has adhered closely to the letter of the law (sometimes to the extent of an almost philological exegesis of the *Cours*), and has often stressed de Saussure's mentalistic psychology;⁴⁷ the Prague School has made the *langue-parole* distinction its center of interest, assigning phonetics and phonemics to 'langue' and 'parole' respectively, (cf. Trubetzkoy, *Principes* 4-7); Hjelmslev, finally, has concentrated on de Saussure's conception of language as a system of values, and has elaborated the Saussurian dictum that 'la langue' is 'une forme, non une substance' (*Cours* 169). It is clear, however, that this is a one-sided interpretation of the Saussurian concept of *langue*. Henri Frei, in a recent article (see fn. 29), has pointed out that the *langue-parole* distinction was by de Saussure intended to be dual: 'langue' contrasts with 'parole', on the one hand as a social institution versus individual behavior, and on the other as a system of values—distinctive elements—versus nondistinctive variants. Hjelmslev bases glossematics on the latter dichotomy only, and rejects the former. It is worth noting that the Prague School likewise rejects a crucial Saussurian concept, namely that diachronic linguistics deals with 'parole' (as early as 1928, in the Jakobson-Tru-

⁴⁵ Eli Fischer-Jørgensen (in Danish), *Nordisk tidsskrift for tale og stemme* 7.81-96 (1944); Hans Vogt (in French), *AL* 4.94-8 (1944); C. E. Bazell (in English), *Archivum linguisticum* 1.89-92 (1949); L. L. Hammerich, *Les glossématisistes danois et leurs méthodes*, *Acta philologica scandinavica* 21.1-21 (1950); Fritz Hintze, *Zum Verhältnis der sprachlichen 'Form' zur 'Substanz'*, *Studia linguistica* 3.86-105 (1949); V. Skalička, *Kodaňský strukturalismus a pražská škola*, *Slovo a slovesnost* 10.135-42 (1948).

⁴⁶ A. Martinet, *Au sujet des Fondements de la théorie linguistique de Louis Hjelmslev*, *BSL* 42.1.19-42 (1946).

⁴⁷ Cf. Charles Bally, *Le langage et la vie* 156 (Geneva, 1952): 'une linguistique qui s'inspire des idées saussuriennes doit—nous l'avons vu—tout ramener à la conscience intérieure que nous avons de la langue.'

betzkoy-Karcevski theses at the 1st International Congress of Linguists at the Hague).⁴⁸ The Geneva group alone accepts the entirety of de Saussure's teaching without criticism (and defends the 'Maître genevois' against the criticism of others).⁴⁹

Hjelmslev's Saussurian orientation, unlike that of some of his Swiss colleagues, but like that of the Prague linguists, is original and productive, leading to an elaborate systematization which draws initially on Saussurian thought, but gradually comes closer to the formulations of modern logicians.

4.2. The arbitrary relation of form and substance. Hjelmslev's conception of form as independent of substance is close to the logical concept of 'pure form', as defined, for instance, by Susanne K. Langer: "'Logical form" means "structure" or the way a thing is put together ... an orderly arrangement of parts ... distinguished from ... "content"' (= H substance).⁵⁰ This conception has been most consistently challenged by Hjelmslev's reviewers (see above); the most extensive critical analysis, from the standpoint of Gestalt psychology, is contained in Hintze's paper.

Two major arguments have been brought up against the glossematic conception: that two H substances (for instance, speech and writing) cannot equivalently manifest the same H form (Bazell, 91);⁵¹ and that the phonetic H substance in particular has a definite bearing on the H formal relationships that are manifested by it, as is evinced by phonetic classifications of phonemes such as correlations (Hintze, 95-6), and by the need of phonetic criteria for 'sameness' in phonemics (Fischer-Jørgensen, 91-2; Martinet, 37-8).

In regard to the non-equivalence of writing and speech, Bazell marshals evidence for both of the propositions that Hjelmslev rejects in this respect: writing 'may be regarded as secondary for purely synchronic reasons: for instance the fact that certain letter-combinations are not found in a given language may be immediately comprehensible if we know the acoustic features they symbolize, whereas a study of graphic features would throw no light on the possibilities of phoneme-combination' (91); also, in many languages 'we should have to deal with graphic and phonic systems that are asymmetrical to each other in the same way as both [are] asymmetrical to the content-system' (92). Both of Bazell's observations seem to me to apply to English, and to be valid criticisms.

Vachek—who does not refer to Hjelmslev in his paper—brings out two significant differences between the structure and function of speech and those of writing. (1) Both are systems of signs, but speech is manifested acoustically and its function is to respond to a stimulus dynamically (i.e. quickly and readily), whereas writing is manifested graphically and its function is to respond in a static way (i.e. permanently and deliberately; 67). This is an H transcendent observation, but nonetheless valid. (2) Spoken utterances are one-dimensional, written utterances are two-dimensional (sometimes even three-dimensional:

⁴⁸ *Actes du 1er Congrès international de linguistes* 35 (Leiden, 1929).

⁴⁹ See *Cahiers Ferdinand de Saussure* 4.65-9 (1944); 9.7-28 (1950).

⁵⁰ *Introduction to symbolic logic* 42 (New York, 1953).

⁵¹ See also the interesting discussion by Vachek, *Written language and printed language, Recueil linguistique de Bratislava* 1.67-74 (1948).

Vachek here undoubtedly is thinking of such things as script in relief; 68). Here the difference is H immanent. Thus, speech and writing are clearly two different SYSTEMS, i.e. represent different H forms, not just different substances.

Hintze uses the Prague School concept of correlations as proof that an important H formal characteristic of the phonemic pattern is based on phonetic substance, and is not manifestable in another substance: 'Denken wir einmal das angeführte Beispiel Hjelmslev's durch,⁵² indem wir für die Formenelemente der Sprache etwa farbige Flaggen wählen. Also etwa: "rot" für *m*, "grün" für *i*, "gelb" für *t*. Eine Flaggenfolge "rot-grün-gelb" wäre einwandfrei das deutsche Wort *mit*, "grün-rot" folglich *im*, usw. Wählen wir für das auslautende *e* "blau", and für das lange *ī* (doch offenbar ein von *i* verschiedenes Formelement) "violett". Die Folge "rot-grün-gelb-blau" wäre also *mīte* (Mitte), "rot-violett-gelb-blau" wäre also *mīte* (Miete). Welche Beziehung besteht nun zwischen "grün" and "violett", ausser der, dass sie verschieden sind? Wie kann in der anderen Substanz die Relation *i* : *ī*, die ja in der phonischen Substanz genau und eindeutig definierbar ist, zum Ausdruck gebracht werden?' (95). Hintze has here deliberately oversimplified his example, but the objection is valid for a fussier analysis of German as well. It could perhaps be countered by saying that Hintze's choice of colors was 'loaded'; he could have proposed light-green for *i* and dark-green for *ī*, and would then have had a correlation of 'darkness' corresponding one-to-one to the correlation of quantity which he adduces as evidence. But this counter is, to my mind, not valid, since other H substances in which the same language is manifested (such as writing, or real—not hypothetical—flag codes) use units which often cannot reflect correlative phonemic features in any way. Another alternative is to reject the concept of correlation; but most viable systems of phonemic classification are based at least in part on phonetic criteria, and such classifications could thus not be established 'bei einer Umsetzung in eine andere Substanzkategorie' (97).

The need for at least some phonetic criteria for the 'sameness' of phonemes is stressed by both Fischer-Jørgensen and Martinet. The former points out that 'the commutation test can lead us to recognize that a language has 15 different initial elements and 10 different final ones; but it cannot decide which goes together with which, whether, for instance, final *p* belongs with initial *p* or initial *t*' (92). Martinet accepts this, and adds to it that no one could predict whether a final *p* permuted into initial position (for instance by cutting and pasting a motion picture sound track) would remain identifiable (37).

A closely related point is made by Josef Vachek in another recent paper,⁵³ namely that the phonemes of speech and the graphemes of writing are differentiated on the basis of totally different features, inherent in the acoustic and in the graphic H substances respectively. Even if a case can be made out, in certain languages, for a general H formal equivalence of phonemes and (alphabetical) graphemes, there is no doubt that on the level of distinctive features

⁵² Cited from Hjelmslev, *Über die Beziehungen der Phonetik zur Sprachwissenschaft* (II), *Archiv für vergleichende Phonetik* 2.214 (1938).

⁵³ Some remarks on writing and phonetic transcription, *AL* 2.86-95 (1945/49).

we are dealing with two different kinds of systematizations, which must imply a separate H form for each H substance.

One other point. Hjelmslev includes navy flag codes as one of the possible H substances in which a 'natural' language can be manifested (66, *OSG* 92). It strikes me, however, that in the case of alphabetic flag codes and the like, we are not dealing with H semiotics at all, but with H symbolic systems. The plane of expression consists of flags and the like, while the plane of content consists of the letters of the alphabet, the two planes being H conformal. Hjelmslev makes no provision in his theory for a symbolic system whose plane of content is a semiotic.

For the Morse code, 'dot' and 'dash' could be considered *figurae* of the entire signs, which would make the two planes H non-conformal and the Morse code an H semiotic whose content plane is a semiotic (the letters of the alphabet)—i.e. a metasemiotic (cf. 73, *OSG* 101). On the other hand, since the Morse code, as far as I can see, is not an operation, it is also a non-scientific semiotic; and since it is a non-scientific semiotic one of whose planes is a semiotic, the Morse code is also a connotative semiotic (cf. 77, *OSG* 106). Thus, the Morse code introduces a serious logical contradiction into the part of Hjelmslev's theory that deals with language and non-language; for I imagine that the same object cannot very well at the same time be an H metasemiotic and an H connotative semiotic.

The Morse code can be a useful illustration from another standpoint as well. It is one of the few sign systems that can be manifested in any physically suitable H substance whatever, without any change in its H form: blinker light, marks on paper, different 'dot-and-dash' noises, lighter and harder knocks, etc. The Morse code is an H semiotic by virtue of having H *figurae* (the dots and dashes), but it differs significantly from other H semiotics in having completely unanalyzable H *figurae*, whereas the phonemes or graphemes of a language can be dissolved into some kind of distinctive features. As Vachek has pointed out, it is on the level of these features that phonemes and graphemes most clearly belong to different systems; it is here that the H substance most clearly asserts itself. We might therefore say that the relation of H form to H substance is COMPLETELY ARBITRARY (and subject to limitations of physical feasibility alone) ONLY FOR SUCH H SEMIOTICS AS LACK DISTINCTIVE FEATURES. In regard to other H semiotics, including language, I concur in the conclusions of Fischer-Jørgensen, Martinet, and Hintze, best formulated by the last: 'zwischen Form und Substanz besteht ein unauflösliches Wechselverhältnis' (101).

4.3. Content *figurae*. The criticisms leveled at the concept of 'content *figurae*' have perhaps been precipitated by Hjelmslev's choice of examples showing lexical (not grammatical) contrast. This is clearly stated by Bazell: 'The difficulty in such analyses [as Hjelmslev's 'ewe = she-sheep', 'ram = he-sheep'; 44, *OSG* 64] is that lexical oppositions are not minimal as are phonemic oppositions' (92). To this could be added that the H *figurae* shown in the examples are not ultimate units: 'she' could be divided into 'animate' and 'female' (animate as opposed to 'it', female as opposed to 'he'); also—and this is Bazell's

point—'he-sheep' does not exhaust the content of 'ram'. Bazell (*loc.cit.*) and Fischer-Jørgensen (89) also point out that substance plays a more important part in regard to content. 'In the lexical field,' says Bazell, 'matter dominates and even prejudices form'; while Fischer-Jørgensen observes that 'content includes a much greater area of substance, than expression.' She adds that from the standpoint of 'language as a form of cognition'—which is, of course, an H transcendent viewpoint—it is essential to know 'what is formed in a single sign'.

Martinet observes that the alleged content-figurae, in addition to being H content, always have or can have also expression (*šəval*, *fəmel* corresponding to the contents 'horse', 'female'), whereas expression-figurae (i.e. phonemes) only exceptionally have content (*o* = 'eau', 'aux', etc.; 39–40).

Both sets of objections can be partially met if we limit ourselves to an underlying analysis of grammatical content, as in Fischer-Jørgensen's example of the Latin suffix *-us* in *dominus*, which can be dissolved into the expression figurae /u/ and /s/, and the content-figurae 'nominative', 'singular', and 'masculine' (though the last, I believe, applies only to *-us* as an adjective suffix, cf. feminines such as *mālus*, *quercus*); 'the content entities that are here considered are limited in number, recur in many combinations, and are therefore easy to systematize' (*loc.cit.*).

But there still remains the fact that expression-figurae can be segmented both in the sequence and in the system, whereas content-figurae obviously cannot be segmented in the sequence, but exist simultaneously. This adds weight to Martinet's objection: 'Dans ces conditions, nous ne voyons pas comment maintenir, sur ce point central, le parallélisme des deux plans' (40).

4.4. Rejection of social norms. Except for Skalička, Hintze is the only European reviewer who seriously challenges Hjelmslev's contention that 'sociological norm ... proves to be dispensable throughout linguistic theory' (57, *OSG* 80). 'Die Sprache,' says Hintze, 'ist also ihrem Wesen nach eine soziale Institution, ein "fait social" in Sinne Durkheims ... In der Nichtberücksichtigung dieses wesentlichsten Kennzeichens der Sprache, nämlich ihres sozialen Charakters, scheint mir die eigentlich Ursache der sehr abstrakten Auffassung zu liegen, die Hjelmslev vertritt, einer rein formalen Theorie, die nur der kalkülmässigen Seite der geschichtlich und sozial gewordenen Sprache gerecht wird, nicht aber der sprachlichen Ganzheit in ihrer phänomenologischen Wirklichkeit' (102–3). Hintze's point of view, of course, implies an 'existence postulate' such as is rejected by Hjelmslev (8, *OSG* 14); the question thus arises whether an 'existence postulate', or for that matter all reference to a social norm, is indeed to be rejected as dispensable, as Hjelmslev claims.

Skalička, in an otherwise quite confused review, points out that even in a chess game 'the dependence on social manifestation is just as valid as in economic life. The chess game naturally changes according to whether it is a pastime for a few people or a mass phenomenon, etc. And this dependence on social manifestation is so much the more true for language' (138).

The discussion in the preceding two sections has indicated some of the serious operational difficulties arising from the purely formal approach advocated by

Hjelmslev. It seems more plausible to accept a formulation in terms of the 'interplay of form and substance' (Fischer-Jørgensen, 92). If we do so, however, we are no longer bound to the same extent by the desideratum of H immanence, and are free to accept some H transcendent factors—as part of the 'interplay', if you will—if they can help us otherwise to meet the requirements of the empirical principle. We have also, in accepting considerations of H substance, implicitly accepted the 'existence postulate'.

In the paper mentioned in fn. 29, Frei points out that in many languages there are phonemic variants which, though non-distinctive, are yet 'socially obligatory' (144-5); an example is English unaspirated [p] after /s/, or aspirated [p^h] initially. It would be difficult to describe this difference in purely H formal terms; it seems that the 'obligatory' nature of this variant should nonetheless be included in a description, to meet the requirement of exhaustiveness contained in the empirical principle. For such cases the H transcendent concept of social norm must therefore be included. Indeed, in all actual phonemic descriptions, 'preferred variants' and 'obligatory variants' are included as a matter of course as part of the statement of allophones.

Finally, most American linguistics (as already observed in §2.7) are biased (I believe justly) in favor of a culturalist approach to linguistics. This bias is due partly, as Hockett has said, to the anthropological training of many American structuralists (*IJAL* 18.89), partly to the very suggestive hypotheses that were formulated in the Sapir-Whorf tradition of 'language and culture'. It is no accident that the word 'conventional' or an equivalent appears in nearly all definitions of language by American linguists; it springs from their basic orientation, and has proved extremely fruitful in tying up loose ends of both language and culture. In the American conception, linguistics is a social science, because conventionality is part of the definiens of language. Hjelmslev, rejecting this definiens, would leave linguistics 'homeless' in the eyes of many Americans.

5. Glossematics and linguistics. The novum of the *Prolegomena* lies not in any one of the numerous detailed rules of procedure; many of these are currently in use, under one term or another, in American and European practice. Nor does it lie in the demand for an 'immanent' linguistics; many American linguists have been aware of the need for delimiting their discipline. Hjelmslev's outstanding merit lies in having pulled together, for the first time in modern linguistics, a tangle of theoretical details into a logically consistent, close-knit body of definitions and corollaries, based on a minimum of assumptions. In spite of some serious deficiencies, he has been able to assemble into a single deductive (not H deductive) system many insights and techniques of structural linguistics previously scattered through the literature. The *Prolegomena* can thus serve as a skeleton for a less one-sided, more far-reaching (and perhaps more definitive) general theory of language, a skeleton in which the 'pattern points' for additions and improvements are often already in place.

The *Prolegomena*, once understood, are an esthetic delight. Their usefulness for concrete linguistic analysis, on the other hand, is not immediately apparent. The few major attempts that have been made so far to apply Hjelmslev's theo-

ries in practical work have not been conspicuously successful,⁵⁴ less because of the defects of glossematics than because the investigators have not always been thoroughly familiar with either glossematic theory or the material at hand.

The inclusion of both H expression and H content in 'immanent' linguistics,⁵⁵ the rigorous handling of H text and system (especially in avoiding the ill-defined concept of utterance), the neat statement of function types and the clear distinction between H form and H substance, the technique of H catalysis, the discrimination between H semiotic and symbolic system, and finally the definition of an H connotative semiotic as one 'whose expression plane is a semiotic' (78, *OSG* 105)—all these aspects of Hjelmslev's theory can become useful tools of linguistic analysis. Not all of the theory is new; but its restatement in Hjelmslevian terminology clarifies the relationships, and resolves some of the apparent differences between American and European structuralist methods.

⁵⁴ Cf. Martinet's review of Knud Togeby, *Structure immanente de la langue française*, *Word* 9.78-82 (1953). For a more favorable opinion, see M. Fowler, *Lg.* 29.165-75 (1953).

⁵⁵ On this I agree with Eli Fischer-Jørgensen, *Remarques sur les principes de l'analyse phonémique*, *TCLC* 5.216-9 (1949).

Meaning, communication, and value. By PAUL KECSKEMETI. Pp. viii, 349. Chicago: University of Chicago Press, 1952.

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The outstanding characteristic of this book is its encyclopedic nature. This is at once an evil and a virtue. To level the crumbling walls which arbitrarily divide knowledge into independent departments not only is a desideratum but is, indeed, consonant with present trends of education. This is the virtuous aspect of the book. The evil is derived from the degree to which Kecskemeti exercises the new freedom of interdisciplinary mobility. He traverses so many fields of knowledge at such neckbreaking speed that most readers are certain to run out of breath in their efforts to follow the author and his arguments.

Here is a sketchy survey of topics dealt with. The Introduction (The Concept of Meaning) and Part I (Meaning and Situation, 1-97) cover subjects that are by and large pertinent to the captions; but the relation of the concept of meaning to the unity of science (10-3) is not very clear, and the section Meaningless Expressions and Philosophy (78-97) discusses a variety of matters which cannot easily be reconciled with the chapter title, Standards of Meaning—e.g. a critique of positivism (84), democratic attitudes (86), the nature of freedom and morality (87), the philosophy of science (90), a defense of Plato (92). Part 2 (Meaning and Behavior) includes chapters on Meaning and Consciousness (in which Kecskemeti comes to the conclusion that meaning has nothing to do with consciousness), and on Learning and Freedom. The latter is in essence the often heard attack on stimulus-response psychology. A subsection is devoted to the topic of freedom (119-21). The author states that the essence of freedom 'is elusive and mysterious. Freedom itself is free; it brooks no confinement within formulas.' Accordingly, this subsection is also on the elusive and mysterious side.

Part 3 (Meaning and Language) encompasses semantics, symbolic logic,

ethics, epistemology, philosophy of science, and sociology; Section 3 of Chapter 8 (The Language of Interpretation) has the title: Max Weber's and Vilfredo Pareto's Concepts of Sociology, and has no obvious relevance to meaning and language. Part 4 (Meaning and Value) covers, as might be expected, a vast range of topics—e.g. power in society (251–2), love (260), improvement of society (273), clinical psychology (278); the sections captioned Religious Orthodoxy, Truth, Justice, Beauty, Classical Utilitarianism, and Modern Utilitarianism, as far as I can tell cover just what their titles would indicate. The book ends with a thoughtful chapter called The Validation of Judgments of Value.

Obviously only a small part of the book is relevant to linguistic science. Of interest is Kecskemeti's concept of meaning and language. 'Meaning,' he holds (1), 'is anything that is or may be "interpreted."' Again (8), 'meaning [is] a construct or matrix, indicating not which response will actually occur in the presence of a stimulus but which response is "good" or "bad," respectively, by some standard, *if it occurs.*' These definitions indicate that Kecskemeti is concerned with considerably more than the symbol-referent relationship. His primary interest centers on interpretation in general, which he considers to be (8) an 'act in which a meaning is grasped.' It is not quite clear to me how far Kecskemeti distinguishes evaluation from interpretation. 'Evaluation,' he writes (239–40), 'is an act within the meaningful structure of a situation, and "value" is a situational category. ... thus, in evaluation, we have to single out a component of the meaningful situation and look at its particular role and merit within the context.' If I understand these quotations and their context correctly (and I am not sure that I do), evaluation is more or less synonymous with interpretation. Opposed to Kecskemeti's concept of meaning is his concept of 'brute fact', i.e. a datum which cannot be interpreted in terms of 'good-bad' scales. In three short subsections—majestically entitled The Concept of Meaning and the Unity of Science (10–3) Meaning and Mechanism (13–9), Science and Meaning (19–20)—the author explains, among many other things, that the natural sciences deal with brute facts but the humanities with meanings. Kecskemeti's treatment of meaning, viewed as a whole, suffers from the continuous introduction of new terms, distinctions, and concepts. Thus, in Chapter 1, The Categories of Meaning, he speaks of communicative meaning, situational meaning, linguistic meaning;¹ and of situational fitting, declarative communications, analytical communications, first and second levels of interpretation, meaning of situations, judgments, interpretation-in-behavior, and contextual interpretation. He also discusses a symbol cycle, establishment of circuits of confidence, a situational cycle, a contextual cycle, and a cycle of explanations; and later on (85) a cycle of information and a cycle of need-reduction. This terminology alone is enough to make one dizzy; while the frequent digressions

¹ Of linguistic meaning he says (27), 'although the "linguistic" meaning is *invariant* toward possible sets of "situational" meanings, the set of "linguistic" meanings is not self-contained, as it were. To "understand" a language does not mean merely familiarity with rules relating symbols to one another. It also means familiarity with ways in which the interpretation of symbols can be fitted into the nonlinguistic context of the "live" situation.'

from the main subject effectively mask the author's line of argument. Some of the statements on meaning mean remarkably little, at least to me; for instance (27): 'Isolation from human intercommunication by symbols creates anxiety, and, if it is severe and protracted, leads to grave trouble. Man's "symbol need" is so strong that he often uses symbols, as it were, for the pleasure of it, without the possibility of any immediate "fitting" into the live situational context.'

Part 3, Meaning and Language, is probably of greatest interest to linguists; Chapter 5, on Symbols, is especially stimulating. Kecskemeti's distinction between name languages and word languages is perhaps not very original in itself, but the thorough analysis of these notions and the many interesting formulations are well worth reading. Thus (131): '[Names] have a "situational" function *in themselves*, as names; this is another aspect of the peculiarity of names which we noted at the outset, namely, that they are not properly part of the various "languages." Names structure the social world, they organize the ways in which men form associations among themselves.' Or, with respect to word language (140): '"Referring" is not "pointing out" something as being present here and now; it is not an operation performed upon a thing. We can try to characterize it, rather, as a complex relationship of this form: "To say that a word *W*, used in a communication, "refers" to an object of the kind *P* means that, when the communication is fitted contextually into an actually prevailing situation, an object of the kind *P* will be found to characterize, in a specified way, one of the situation-classes to which that situation belongs, provided that the communication is true.'

Despite these and similar stimulating thoughts, it is very difficult to gather a comprehensive idea of Kecskemeti's contribution to our knowledge of language. This is partly due to the diversity of contexts in which he deals with language. For instance, Chapter 6 (Sentences) is a treatise on logic; it is unintelligible to anyone lacking thorough training in this discipline, and I suspect only partially intelligible to one who has it. Does a background in symbolic logic furnish a clue to the following sentence (162)? 'The second axiom is that of "exemplification"; it asserts that the occasional extension of a formal implication implies the implicative occasional transformation of the formal implication: $(h, x) fx \supset gx: \supset: h!fx \supset gx$.' The symbols, it is true, are explained in the preceding paragraphs; but does the English sentence make sense? In connection with the discussion of meaning, the word *language* appears in still another context (71): 'The "standards of meaning" in terms of which responses to communicative meaning may be said to be "good" or "bad" are rules of language. ... [There are] rules which merely concern mutual relations between the symbols as such and rules which refer to extra-linguistic, situational factors.' There is nothing new or exciting about these observations nor about the discussion that follows them. The confusing thing is that the phrase *rules of language* means in this context *rules of the game*. The sections entitled The Language of Convention, The Language of Immediately Felt Relevance, The Language of Utility, and The Language of 'Higher Values', use the word *language* in the sense of *discourse*.

Throughout the book there are passages which will cause the reader to raise his brows. 'The quickest and most comprehensive changes in language behavior

seem to occur when a literary or literate language is developed; this is usually the work of a single generation of legislators. After this legislative period, fluidity will decrease: speech becomes standardized' (193-4). Is this true? The section *Meaning and Mechanism* (13-9) is an attack on mechanistic viewpoints in scientific theories. I am not sure whether Kecskemeti is shadow-boxing, or whether he has misunderstood the intentions of the so-called mechanists. Do mechanists EXPLAIN organisms in terms of mechanisms, as Kecskemeti implies, or do they strive to construct the simplest model for a process whose cause they ignore? I believe the latter is the case.

Chapter 7, *The Language of Empirical Science*, contains a long and frankly mystifying discussion on the characteristics of history with special emphasis on historic laws (172 ff.). "History" may be defined as the systematic study of the *incidence* of laws ... The mere description of historical events in itself is sometimes an important goal of the efforts of historians, even if it is impossible to specify any laws of which those events are the "incidences," although the ultimate goal of the historian is to pass from description to explanation. The laws we need [for an understanding of the historical evolution of our society] are those governing the behavior of groups and individuals. These laws ... can be formulated only by explicit reference to sets of their incidences. We shall call these laws "historic" to be distinguished from the ordinary (non-historical) type of law' (174-5). I may well have missed the point of these and the ensuing statements, but they are counter to my conception of history; nor do I see their relevance to the language of empirical science.

A point of considerably less importance is the occasional cryptic use of footnotes, especially those that begin with *cf.* but do not end with a page reference. It would have been useful if Kecskemeti had told us what the position of the cited authors is with respect to the points he makes in the text.

The overall purpose of this book is to reinstate personal and intuitive judgment in its own right. It is therefore in order to conclude with a personal, intuitive, and (I might as well add) subjective evaluation of Kecskemeti's work. The book contains too many diversified thoughts to constitute an organic whole. If it is read as a collection of aphorisms, half of which touch on problems of meaning and evaluation, it is interesting for its many ingenious formulations and stimulating because of its occasional novel ways of looking at old problems. As far as linguistics and psychology are concerned, it neither asks any new questions nor adds anything of significance to theoretical or empirical knowledge in these fields. This judgment, of course, concerns only a part of the book. Let us look to philosophers and sociologists for a more favorable evaluation.

Studien zur indogermanischen Grundsprache. Ed. by WILHELM BRANDENSTEIN. (Arbeiten aus dem Institut für allgemeine Sprachwissenschaft, Heft 4.) Pp. 75, offset. Wien: Gerold & Co., 1952.

Reviewed by W. P. LEHMANN, *University of Texas*

This small volume of essays devoted to Proto-Indo-European is important to Indo-Europeanists for its indication of problems under investigation, for the

sympathetic survey of other publications, and for the information given us of the kinds of work being undertaken by an active group of Indo-European linguists. We also feel the 'mangelnde Übersicht über das gesamte Publikationswesen', which threatens to become even greater as pertinent articles are published in countries now just beginning serious work in Indo-European linguistics, such as India, and as Indo-European studies become ever more complex. Brandenstein's article on the provenience of the Indo-Europeans (23-5), with its reliance on climatology and geography, is a good illustration of this complexity, and requires so much non-linguistic knowledge that one hesitates to judge its results, though they are similar to those of V. Gordon Childe, *Prehistoric migrations in Europe* 151 (Oslo, 1950)—namely, that the 'Indo-European cradle' was in the steppes of eastern Europe or hither Asia. Further volumes of this type give us our only hope for keeping up with publications of various countries and various linguistic traditions.

The essays deal primarily with phonology and etymology. The first of these has been of greater interest to Indo-Europeanists of the past decades; yet each supplements the other. Accurate phonological analysis is based on adequate etymologies, and new etymologies in turn contribute to a fuller understanding of phonological developments. This relationship has often been overlooked, and etymological studies have been disdained. But the renewed interest in Indo-European culture has stimulated word studies, as is clear from Manfred Mayrhofer's valuable survey of Indo-European word study since the end of the war (39-55), from which phonological studies in turn should profit.

In the study of reconstructed languages, contributions by scholars of various linguistic schools are probably more important than they are in routine descriptive work. For diverse methodologies may illuminate problems which seem insoluble in one linguistic tradition, or which may lead astray scholars of another school. These essays are in the tradition which puts great emphasis on philology; they are distinguished by close attention to sources. Some linguists of another tradition have drawn conclusions about cultural relationships from the supposed borrowing of the Indo-European etymon of Gk. *pélekus* from Akkadian *pilaqu*. But Margit Falkner reviews the Akkadian sources (26) and finds them so restricted that these conclusions are without foundation. Similarly Mayrhofer, in examining the evidence for a supposed IE **sor-* 'woman' (32-9), finds it unconvincing, and suggests for every previously adduced group of words derived from **sor-* a different etymology; whoever the authorities of the past who assumed IE **sor-*, such as E. H. Sturtevant, Mayrhofer's interpretations are much more cogent, as are his suggested etymologies. In general, the Indo-Europeanists represented in this volume are excellent in minutiae,¹ as is Brandenstein in his

¹ As usual, however, there are a number of misprints, most of which will cause no difficulty. The following are worth listing (by page and line): 13.5 from bottom, < not an asterisk; 20.9, supply accent on *kiáutas*; 20.18, -gehen not -geben; 58.4, add *s*; 58.7, *Tenues* not *Mediae*; 58.20, bald not *bals*; 64.22, add *i e a u*. The troublesome reference at 9.5 should be 239 b rather than 230 b. Considerable experimentation is still necessary in the use of photo-offset; but the raised *c* for ' (33.1) is not a happy device. One wonders why *w* and *y* are omitted from the list of resonants, 16.5 from bottom.

discussion of the PIE clusters *ms* and *ns* (5-12). American Indo-Europeanists on the other hand have been much more interested in broad problems, and may at times have dealt perfunctorily with sources, to the distaste of their fellows of the solid European tradition (cf. 57 fn. 5). But in problems involving general linguistic theory or the main outlines of Proto-Indo-European, American views, if bolder, hold more promise for the future of IE studies than does a trust in Brugmann or a limitation to the verifiable data of texts. An attempt will be made below to establish this statement.

For the study of sources, or philology, however necessary, is but a means to the end of these essays, and accordingly no more important than other ancillary disciplines or than the particular approach of schools of linguists. In this volume Trubetzkoyan theory is heavily in evidence. Moreover, Trubetzkoy's suggestion in *Acta linguistica* 1.81 ff., that Proto-Indo-European may never have existed, that the agreement between the various IE dialects may be due to a period of mutual influence among neighboring languages rather than development from some earlier language, is the first problem raised in this book. Although Brandenstein leaves the issue open, we may now have, in glottochronology, a possibility of approving or rejecting the suggestion. In spite of Sauvageot's highly critical comments, *BSL* 47.25-8, and the small number of investigations completed, we may already use this theory with some assurance as a negative check. If the agreement in vocabulary between two present IE dialects fits the formulae developed in glottochronology, as for English and French, we consider this good evidence in favor of Proto-Indo-European, and reject Trubetzkoy's suggestion. But for final rejection we must wait for further glottochronological studies in the IE languages.

Assuming that Proto-Indo-European was a spoken language, we must deal with it as we do with languages spoken today. We therefore apply to it all the findings of recent linguistic work, in phonology, morphology, dialect geography, and other areas. Edgerton's treatment of forms like Skt. *jaganváms-* and *vavanváms-* is therefore more convincing than Harl's (13-4). In his well known study in *Lg.* 19.83-124, Edgerton dealt with the IE resonants structurally, and sought to account for the exceptions to his general formulae, as he did for the unexpected *an* from PIE /n/ before [w] in these words (cf. *Lg.* 19.117 fn. 66). Harl attempts to account for the *an* by assuming in these words a further 'overshort' vowel before the *v* at an earlier period, and before the *y* of similar forms. The flexible structural approach used by Edgerton yields a better solution of the problems in these aberrant forms than does the assumption of further vowels in early Indo-Iranian. On the other hand we concur heartily with Mayrhofer in his application of the findings of dialect geography to the problem of IE palato-velars and the IE word for 'hare' (27-32, 71), and in his conclusions, though we might wish that he had used a method more current in dialect geography than that provided 80 years ago by Schmidt in his *Wellentheorie*.

Linguistic theory also directs the conclusions of what is possibly the most ambitious article, the one by Edeltraut Mayrhofer-Passler on quantitative ablaut in the IE languages (15-22). The method is based on Trubetzkoyan theory dealing with languages characterized by the number of morae, *Grundzüge der*

Phonologie 179 ff. (Prague, 1939). Proto-Indo-European is assumed to be such a language, in which each non-enclitic must have one high pitch accent. The morphophonemic variation between what are commonly designated as normal grade, zero grade, and lengthened grade is ascribed to rhythmic principles (16), and the presence of *e* in the normal grade tentatively to 'the phonetic fact that *e* or *a* can be better articulated at a high pitch than *o* or even *i* and *u*' (17). By these formulae one can probably account for the facts of PIE vowel variation as completely as one can with the theory of their origin in stress variation, if one accepts the remarkable statement about normal-grade *e* and the underlying linguistic theory (though the ascription of 'laryngeals' to phonetic glide phenomena would require considerable explanation when confronted with the Hittite facts; on this point see Crossland's useful essay in *TAPA* 1951.88-130, esp. 103). The mora theory however must deal with further questions to establish its credibility. Rhythmic laws are notoriously vague, almost without any hope of substantiation. Some of the other terminology used here increases one's suspicion that language is viewed as a Schleicherian organism, e.g. 21: 'Die einsilbigen Wurzelwörter haben dieses Prinzip bis zur Viermorigkeit verstärkt, wohl weil sie sonst in ihrer Existenz zu sehr gefährdet gewesen wären.' Besides suspecting such linguistic theory and expression, when we compare with ablaut variation the roughly comparable Germanic umlaut phenomena, for which we know the underlying cause, we do not find this in rhythmic laws, but rather in 'mechanical principles'. The OHG etymon of *Gäste* became differentiated from *Gast* because of a following high vowel; the use of this difference for grammatical purposes, and its spread to words not originally of this pattern, such as *Platz* and *Tanz*, developed subsequently. We make a similar assumption for IE ablaut. Moreover, before it can gain any credence, the mora theory must be subjected to examination for its application to IE morphological as well as phonological data. Lengthened grade is apparently assumed by the author to be 'original', for the standard view of lengthened grade is ascribed (20) to the practice of scholars who 'following the model of the Old Indic grammarians started out from short vowels as the original ones and considered the lengthenings as secondary'. Ultimately the status of lengthened grade will probably be determined more from its role in the grammatical structure of the language than from comparisons with other mora languages or other IE languages. The age of the lengthened-grade forms has unfortunately obscured the conditions under which they arose (or existed before later changes took place); but if, as seems likely, enough of the conditions can be deduced from our data, we prefer an attempt at explanation which is similar to the one that can be established from moderately copious data, as in the Gmc. dialects, to an attempt based on unverifiable rhythmical hypotheses.

Although we agree with H. Kronasser's statement that historical linguistic theory needs refinement, we think that the way to better theory can only be obstructed by statements like the one cited above about the articulation of *e* in high pitch, or (12) like this: 'the language has evaded the phonological difficulties resulting in this way ...' We also find it difficult to accept some minor points of procedure, especially with reference to the laryngeal theory. In his review of

recent work, entitled 'Structural Linguistics' and the Laryngeal Theory (56-71), Kronasser presents various objections to the theory as it has been proposed. (1) He would like more evidence from the Anatolian languages. So would we. But should we wait? Here it is we who exercise philological caution, and ask whether we can rely with assurance on the Anatolian evidence until many philological problems are solved, or more archeological exploration carried out; we agree with Crossland, *TAPA* 1951.90, that the 'material in these languages ... is so scanty and imperfectly understood that it is not profitable to use evidence from them at present'. On the other hand we do not propose to let progress in IE historical linguistics wait for archeology, or on the whims of the government that controls excavations. (2) He complains that the most varied difficulties are explained by the theory in the publications of the laryngealists. Actually only a minute proportion of IE forms is thus explained. Possibly we need a reworking of Brugmann's *Grundriss* to put the laryngeal theory in proper perspective. But the field is too limited for the financial support necessary, and the laryngeal theory is not yet so adequately worked out as to justify the publication of a *Grundriss*. (3) He notes that the laryngeals are shadowy phonemes, presumably on the grounds that they are easily lost. When we survey the development of any language, we find various phonemes 'easily lost': *r* in British English *bark*, *l* in English *talk*, etc. These belong to the categories that seem to disappear readily. But what of stops like *bh dh gh*, as between Proto-Indo-European and Baltic or Slavic? Or *p* in Celtic? We conclude that phonemes lost at one time may not be lost as readily at another. Probably historical linguistic methods will best be refined empirically, as linguists of one school apply their theories to particular difficulties and linguists of another school review them.

Accepting the faith of scientists that their methods will be refined by their successors and their results modified, we ask what should be the present program for the study of Proto-Indo-European, or of other reconstructed languages. Building on the work of our predecessors, we must first accept, or revise, their formal analyses. The forms we reconstruct will have to be symmetrical structurally, as are, for example, Benveniste's PIE roots. This procedure is dangerous, in leading us to over-structuralization, but we have no other check. Positing roots of any shape, as was done in Walde-Pokorny, and is being done still by Pokorny, leads to chaos. From the forms which we reconstruct we derive a phonological description of the proto-language; and from non-symmetrical patterns in our reconstructions, we may be able to reconstruct earlier forms of this language. In phonology, the most readily available material is segmental. Indo-Europeanists need to project a segmental system that best accounts for the facts. The pressing problem of the present is the analysis of those forms which are assumed by some Indo-Europeanists to contain laryngeals; but this problem should soon be solved well enough for further work to be satisfactorily undertaken. This will be on the suprasegmental system. The almost complete absence of such studies is probably responsible for the current status of inactivity in IE syntactic studies. Yet we have available for suprasegmental studies many sources similar to those used in segmental studies: the marking of suprasegmental patterns by so-called sandhi changes; their use in verse; and their reconstruction

from IE languages currently spoken. When the descriptivists provide us with data here, we shall be much better equipped for dealing with Proto-Indo-European. Suprasegmental studies will lead to studies of PIE morphology and syntax, and subsequently of style. Morphological, syntactic, and stylistic developments must be dated by the stages which have been set up in phonological study, as I attempted in the last chapter of my *Proto-Indo-European phonology*. Such dating will avoid misunderstandings of the type noted by Kronasser with regard to a recent article in *Lg.* 28.182-5, Laryngeals and *s* movable. The morphological process suggested by Hoenigswald would have taken place in pre-IE stage C (cf. *PIEP*, Chap. 15). Our information may be too limited, as Kronasser seems to think, for work at such an early stage of IE morphology; but if the stage were clearly indicated, the daring of an article like this would be recognized, and possibly greeted with pleasure rather than dismissed.

While such work is being carried out on the general structure of Proto-Indo-European, scholars of another bent will be re-examining the minute data on which the general theories are founded, and will ultimately provide the basis for a new synthesis.

Le pélasgique: Essai sur une langue indo-européenne préhellénique. By A. J. VAN WINDEKENS. (Université de Louvain, Institut Orientaliste: Bibliothèque du *Muséon*, Vol. 29.) Pp. xii, 179. Louvain: Publications Universitaires, Institut Orientaliste, 1952.

Reviewed by GORDON M. MESSING, *Washington, D. C.*

The term 'Pelagian' was often applied by the ancient Greeks to certain of the so-called autochthonous peoples whom they encountered in their conquest of the territory which they were subsequently to occupy in historic times. Perhaps the name was first attached to a people who lived in the Thessalian plain around Larisa; Homer knows 'Pelagian Argos' (B 681), and Paul Kretschmer (in Gercke-Norden, *Einleitung in die Altertumswissenschaft*) has compared the name with Gk. *πέλαγος* in its original meaning of 'level space' (*Πελασγοί* < **Πελαγσ-κοι*). Had Thucydides seen fit to transcribe a text or two in the Pelagian tongue still spoken in his day on the Athos Peninsula—he terms the speakers 'bilingual barbarians' (4.109)—he might have resolved for us the whole issue here inconclusively raised by Van Windekens' study of pre-Greek vocabulary.

Since the publication in 1896 of Kretschmer's *Einleitung in die Geschichte der griechischen Sprache*, most scholars have assumed that the various Pelasgians, Leleges, and Carians of Greek tradition belonged to a non-Indo-European Aegean substratum once widely distributed in continental Greece, the Greek Islands, and Asia Minor; cf. the excellent summary by Schwyzler, *Gr. Gramm.* 1.59-65. When Van Windekens, in the book under discussion, extends the term 'Pelagian' to mean in general the language of this Aegean substratum, he could have drawn on excellent ancient precedent: Herodotus (1.57) used it in the same sense.

For Van Windekens, however, there is one great difference. He chooses to regard his 'Pelasgians' as predominantly an Indo-European population. Since

his hypothesis can count upon little or no endorsement from his professional colleagues, it is well to define his position more closely. Van Windekens follows two predecessors: Kretschmer, in two articles published in *Glotta* in 1925 (14.84 ff. and 14.300 ff.), launched a theory that the Aegean forerunners of the Greeks had already borrowed some 'proto-Indo-European' elements (this serves to explain, for example, the agreement between Lydian *-k*, Etruscan *-c*, and the derivatives of IE **q^ue* 'and'); while Vladimir Georgiev, a Bulgarian scholar on whom Van Windekens has drawn heavily, went much further. In a series of highly controversial works, Georgiev argued that the carriers of the Minoan-Mycenean culture were Indo-European; hitherto he has found few disciples.

To the present reviewer, Van Windekens' entire hypothesis, together with the specific formulation which he has taken over from Georgiev, is fantastic. Nevertheless, Van Windekens' boundless ingenuity, his impressive fund of knowledge, and his skill in linguistic techniques require that he be taken seriously. I hope to show that he has reasoned from false premises and that his presentation is methodologically unsound.

Let us note first that Van Windekens treats his 'Pelasgian' as essentially a uniform language derived by regular sound laws from Indo-European (though recognizing, it is true, an eastern and a western zone, differentiated by the treatment of IE *-nt-*). Many investigators would sharply question this assumption. Kretschmer posits at least two separate non-Indo-European Aegean languages, the Pelasgian-Tyrrhenian strain with its clear affinities to Etruscan, and that represented by the Leleges and the Carians. We may note in passing that if Van Windekens' 'Pelasgian' were indeed an Indo-European language attested in very early times, it would be reasonable for him to bring his phonetic reconstruction into some relation with the laryngeal hypothesis; he has not done so.

Van Windekens' basic assumption is as follows. Granting that 'Pelasgian' was Indo-European, we are asked to assume that this language was derived from Indo-European by phonetic changes different from those governing the evolution of Greek. This is essentially a circular argument, since the assumption that 'Pelasgian' was Indo-European stands (or more probably falls) solely on the validity of the various etymologies adduced by Van Windekens.

It is daring of Van Windekens to concentrate his heaviest batteries on precisely those elements of Greek vocabulary which for years have figured in the handbooks as both pre-Greek and non-Indo-European: the famous *-νθ-*, *-νδ-*, and *-σσ-* formants; such words as *ἀσάμυνθος* 'bathtub', *ρόδον* 'rose', and *λαβύρινθος* 'labyrinth'. With similar audacity, Georgiev attempted to prove the Indo-European origin of Etruscan by etymologizing its relation-words and numerals, *Die sprachliche Zugehörigkeit der Etrusker* 16 (Sofia, 1943); cf. this reviewer's comments in *AJP* 70.325-7 (1949). Both men assume, and Georgiev specifically states, that only an Indo-European language can be etymologized as Indo-European.

This is specious reasoning, whether applied by Georgiev to Etruscan words, of which the very meaning is often in dispute, or by Van Windekens to a carefully hand-picked group of words out of the Greek vocabulary. The fallacy con-

sists in the introduction of so many phonetic and semantic variables that an ingenious manipulator could supply an Indo-European etymon at will, no matter what the language that he is handling. In Van Windekens' case, as in Georgiev's, a series of far-reaching 'sound changes' must first be assumed; by this means, words which look remarkably unlike Indo-European become more tractable. (Van Windekens' prerequisite is a systematic mutation of consonants as in Germanic or Armenian, shift of IE intervocalic *-y-* to *-b-*, retention of IE *s* in all positions, and treatment of labiovelars and palatals as in a satem language.) The many phonetic peculiarities, from an Indo-European viewpoint, which still persist, are then explained away in a section on the formation of nouns (25-57); here we are informed that 'Pelagian' has replaced the IE suffixes, which were too short, by longer secondary suffixes 'made up of IE materials'.

Let us examine this procedure carefully. We are confronted by Greek words which have proved virtually impervious when considered according to the phonetic formulae by which Greek evolved from Indo-European. It is clearly a help to wipe the slate clean and explain them by a different set of phonetic equations. But this is to ignore the historical origins of the comparative method. The first investigators of Indo-European linguistics initially worked out their systems of pervasive phonetic correspondences by observing sets of words, preferably in two or more IE languages, in which a one-to-one phonetic relationship could be demonstrated in conjunction with an identical meaning (see such a list in Hirt, *Idg. Gramm.* 1.157-9). On these foundations could subsequently be erected an interlocking and consistent system in which phonetic and semantic anomalies in due time find their explanation. But Van Windekens and Georgiev ask us to start with the anomalies; they have no rigorously controlled etymologies as a compelling basis for reliable sound laws. Armenian, as Georgiev argued, may look as little like Indo-European as Etruscan; still, unlike Etruscan, it can show numerous equations at which no skeptic could cavil, such as Arm. *berem* 'I carry': Sk. *bhārāmi*.

Just as the phonetic equations are unsupported by controlled data, so the presumed semantic evolution is tenuous. Van Windekens' 'Pelagian' words are admittedly hard to etymologize. One may remember that, after all, only a fraction of the vocabulary of any IE language can be derived with certainty from Proto-Indo-European. Nevertheless, it is significant that he gives us not a single example (save perhaps Gk. *ἄσος* 'mud': OHG *waso* 'mud') which does not assume extensive semantic change, however plausible. Worse still, Van Windekens is constantly driven back on the dangerous method of derivation from 'roots': since, for example, there is nothing in IE which parallels Gk. *βάρπαξ* 'frog', he attaches it to an IE **bher-*, **bhrā-* 'brown'. (Are 'Pelagian' frogs brown rather than green, one wonders, and if so, are they so conspicuously brown as to make this semantic shift reasonable?) Again, Gk. *θύσις* 'thyrsis', a specialized religious term of obscure origin, isolated in Indo-European, is derived directly from PIE **tres-*, **ters-* 'tremble'. Certainly Walde-Pokorny is an invaluable tool, but it will not supply the lack of a genuine phonetic equation. It is a reversion to an earlier stage of linguistics to enter into what Friedrich Mueller called 'die

Irrpfade der Wurzel-Entwicklung', *Beiträge zur etymologischen Erklärung der griechischen Sprache* 2 (Vienna, 1897).

If Van Windekens' methodology carries no conviction, neither do most of his specific etymologies. Here we can dwell upon a few examples only, and purposely pass over words which are 'Pelasgian' only because their vocalism, from a Greek standpoint, is more or less irregular.

Gk. κόκκαλος 'pine seed'. It is interesting that this formation occurs as a personal name in Pisidia, shows an ending which Chantraine has considered pre-Greek (this is of course unobjectionable from Van Windekens' point of view), and is taken by Vittorio Bertoldi (*Colonizzazioni nell'antico Mediterraneo occidentale* 54-5 [Naples, 1950]) as a non-Indo-European word specifically borrowed from Cilicia.

Gk. κύμινδης 'sort of bird'. Van Windekens sees a derivative of IE **geu-* 'bend', and compares Gk. γύψ 'vulture'. In the *Iliad* (Ξ 290), this is the human name for a bird whom the gods call χαλκίς. Van Windekens might have cited in this connection Kretschmer's article on Nektar, *Anzeiger der phil.-hist. Klasse der Öst. Akad. d. Wiss.* 1947, No. 4, 14-5. Kretschmer makes it likely, amplifying the well-known theory of Hermann Güntert (*Von der Sprache der Götter und Geister* [Halle/Saale, 1921]), that in these Homeric double names, the human appellation is non-Greek. In this case, Kretschmer believes in an Anatolian origin; he compares for the ending the Carian proper names Πίγινδα and Πισίνδηλις and Pisidian Πισινδα. He adds the illuminating remark that while Hipponax felt free to introduce everyday Anatolian terms into his colloquial verse, the epic poet rejected them as vulgar; since many in his audience did not understand the more elegant 'Greek' expression for the same concept, he invented the device of human and divine names.

Gk. λαβύρινθος 'labyrinth'. Van Windekens rejects the usual explanation by which this Minoan term κατ' ἐξοχήν is elucidated as the 'palace of the double ax (λάβρος)'. Instead, he endorses the view of Georgiev that the word is related to Gk. λαύρα 'passage cut in a rock, channel, tunnel', and Alb. *lerë* 'stone, stone pile, landslide', from an IE **lau-rā* 'object in stone'. (This presupposes the convenient 'Pelasgian' shift of IE -*u-* to -*b-*.) A labyrinth would therefore be a 'stone edifice', and λάβρος would itself be a 'stone tool'. Yet semantically it seems unlikely that a tunnel or path in rock would be derived from a root meaning 'stone'. The reviewer is convinced, despite Walde-Hofmann (*Lat. et. Wb.*³ s.v. *lausiae* and *lūra*) that a connection with Gk. λευρός 'open to the view' and Lat. *lūra* 'opening in goatskin' is semantically preferable, despite the irregular vocalism (*a* for expected *e* is a sporadic phenomenon in Greek vocalism which Kretschmer has attributed to an Aegean substratum). As for Alb. *lerë*, is the basic meaning so well assured? Stuart E. Mann, *An historical Albanian-English dictionary*, defines the word '(1) landslide, scree, glacial rubble, pebble bank, (2) dirt, filth, grime'. One must also consider *leth* (*ledh*), which Mann defines as 'mud, clay, dike'; compare also *lehe* 'plot of land marked out by ditches'. There is further the term *ler(ë)* 'puddle', discussed by Norbert Jokl in *Untersuchungen aus dem Bereich des Albanischen* 67 (Berlin and Leipzig, 1923). Possibly the

original meaning was 'mud', whence semantic development in different directions.

Gk. *παρθένος* 'virgin'. Van Windekens substantially accepts the long proposed connection with Skt. *pr̥thuka-* 'child, calf, young of animal', Gk. *πόρις, πόρις* 'young heifer'. This implies for him an IE **phorth-* > **porth-* (Grassmann's law operates in 'Pelagian' too). This puzzling word, which troubled even Brugmann, admits of no completely satisfactory explanation. The most elaborate as yet proposed is H. Moeller's, which ties the word in with Lat. *virgō*, Eng. *girl*, Low German *Göhre*, all with the same meaning. (Kluge-Goetze, s.v. *Göre*, has another explanation of this last word.) Hirt advocated this etymology in his posthumous work, *Die Hauptprobleme der indogermanischen Sprachwissenschaft* (edited by H. Arntz; Halle/Saale, 1939). Gk. *παρθένος* would be for **φαρθενος* < PIE **gʰh₂rgʰen-*; Lat. *virgō* < **gʰerghen-*; the Germanic words < **ghwerghwen-*. Hirt had expounded this etymology earlier (*Idg. Gramm.* 1.145 note 1), observing, 'Jedenfalls sind diese Gleichungen solche, bei denen man das Gefühl, das sie zusammengehören müssen, nicht los wird.'

Gk. *πύργος* 'tower, fortress' and the place name *Πέργαμος*. Boisacq, s.v. *πύργος*, sees a probable Asiatic borrowing, comparing a gloss in Hesychius: *φύρκος* 'wall'. Kretschmer considers that the word was borrowed from Germanic (cf. OHG *burg*, Goth. *baúrgs* 'stronghold, city'). Van Windekens prefers to find a 'Pelagian' cognate of the Germanic base. But since 'Pelagian' is a satem language, he finds himself in difficulties. The Germanic words are usually derived from IE **bhereǵh-* 'high'; a citadel is logically the highest point of vantage for purposes of defense (cf. *ἀκρόπολις*). But he must have recourse to IE **bhergh-* 'hide', commenting that these fortresses were originally sanctuaries, 'des endroits où l'on se cachait'. This is a mere subterfuge and seems very thin.

At most it can be said that there is some utility in the extensive materials here collected on the subject of pre-Greek vocabulary. In the reviewer's opinion, Van Windekens' efforts are largely misdirected. His results are colored by special pleading and sweeping claims. A few are particularly sanguine: 'L'origine de *-θ-* est claire' (43); 'l'origine de *-mn-* [as zero grade of a suffix *-men-*] est fort simple' (50); 'le sens primitif et fondamental de ce mot [*αἶσχος*] n'est-il pas clair?' (61); 'L'archéologie confirme l'interprétation [of *λαβύρινθος*] par la notion de "pierre"' (70; archeology confirms only that stone was used for bathtubs!). Finally, the tone of brisk confidence used by Van Windekens—he is addicted to the perilous word 'proves'—is inappropriate for the murky and treacherous materials with which the student of 'Pelagian' must work. Here if anywhere one must reverse Newton's famous motto to read: 'hypotheses fingo'.

Handbuch der italischen Dialekte: Vol. 1, Texte mit Erklärung, Glossen, Wörterverzeichnis. By EMIL VETTER. (Indogermanische Bibliothek; erste Reihe, Lehr- und Handbücher.) Pp. viii, 447. Heidelberg: Carl Winter, Universitätsverlag, 1953.

Reviewed by JOSHUA WHATMOUGH, *Harvard University*

A pamphlet of additions and corrections to Buck's *Grammar of Oscan and Umbrian* (1904) was printed at Boston in 1928. But the book is all but un-

obtainable. A new edition is in preparation. When it appears it will doubtless be used in courses in the Italic dialects in this country unless the professor prefers, as I do, a plain text in the classroom. Jacobsohn (1910 and 1927) is out of print; Conway (1897) appears occasionally secondhand at a high premium, for the Cambridge University Press got rid of it as a remainder fifty years ago, as did the Harvard Press of *PID*, which booksellers now offer at three times the remainder price; Janssen (1949) is full of misprints, Pisani (1951) those who read my reviews know about as well as I. I suspect that Vetter will be used from now on, until Buck reappears, or (for my classroom) a plain text. It is a good book, and that is high praise. The title (above) indicates the contents and arrangement, and I have only praise for the execution.

Essentially we have a more manageable Conway plus von Planta; we also have, and this is important, account of such advances as have been made since 1897, and also the new inscriptions discovered since then, except the new Umbrian text which I published for the first time in *Harvard studies in classical philology* 50.89-93 (1939). But apparently this is unknown in Italy as well as in Austria, and even in cis-Atlantic regions, where both *HSCP* and *CP* have items of importance to linguists. It was discovered in 1938 at Assisi on the estate of the Countess of Berkeley (a good Boston Lowell), and was communicated to me through the American Academy in Rome.

To come now to Vetter. There is a tick of the clock perhaps once in six months in the field of the Italic Dialects. And there was a strike of the clock, high noon, a few years ago, when it became clear that Venetic is not Illyrian, but belongs closely with Italic. Vetter seems not to have heard those twelve strokes, or he would have included at least a few specimen texts of Venetic, and not been content (362) with an echo of the reverberations; in fact many (not at all 'niemand') would 'verlangen' these, if no other *PID* texts, if only for purposes of comparison. Actually it would now be more than mere 'comparison' to have included Venetic, which (actually again) is at least as pertinent as Sicel or East Italic, which Vetter HAS included (359-62). I miss also the inscriptions of the Val Camonica; see *CP* 35.188-90 (1940).

But let Vetter take heart. I admire his work greatly, not only this volume, but his learned and often convincing articles in *Glotta* and elsewhere. He knows his business, and knows it well; whoever is interested in the Italic dialects should get hold of this book and READ it. I have but few comments to make, for the volume is quite trustworthy. It is a good job, thoroughly well done, with few sins, and those mostly of omission and forgivable, as sins of commission are not, the prayer-book notwithstanding. To one for whom the sun rises and the sun sets with inscriptions, it has given a field day, for which I thank its author.

30: On *fanciam* (not *fanguam*) see *Lg.* 29.193 (1953). 31: On the correct translation see *Lg.* 29.192 (1953). On *etuns* see *Lg.* 29.298-9 (1953). 68: On *iuvila* I am glad to see that Vetter does not feel obliged by my present reputation to take too seriously a paper which I wrote in my salad days; but I still maintain that *Iuppiter* (Jupiter) has no business in this galley (or at Agnone), except by his association with a gang of goddesses; and that the notion of Juno as a female genius is contradicted by all that we know both of the status of women at Rome and of the nature of the genius, at least until Greek philosophy, forgetting all the

experience of the human race, began to regard woman as the equal of man instead of his complement, which is her status in the *R̥gveda* (see Gonda's recent monograph on IE 'one' and 'two', and my review, to appear in *CP*, of that fascinating treatise). 72: *eiduis luisarifs* is indeed 'the Ides of February', but the comparison proposed by Vetter, with Latin *luridus*, is hard to credit, since there is no evidence either for -oi- or for -s- in that word. Far better (cf. Umb. -*lerali-*) was Buecheler's comparison with Latin *lira*, so that *luisarifs* would be the ides of the plowing month (cf. Locrian 'Απάριος and Epidaurian Παπάριος, see *HSCP* 42.171 [1931]), i.e. February. The meaning of *luridus* puts the word out of court; where are names of colors used as names of months? Seasons perhaps, but not months. 77: Conway's interpretation of *prai mamertiais pas set* as the equivalent of Latin *mercedonius*, an intercalary month (between February and March), is confirmed by the copy of the *Fasti* discovered in 1915 (*NdSc.* 1921.122, Plate I, which has *Mer[cedonius]*, as I pointed out in *HSCP* 42.165). 84: On *degetasis* and *deketsiūt* (8) see *Lg.* 3.105-8 (1927), sc. **decentarius*, i.e. 'concerned with tithes'. 106: On *līganakdiket*, an epithet of Ceres (Demeter), sc. θεσμοφόρος, note Vergil *Aen.* 4.58 *legiferae Cereri*, where Schol. Dan., by marrying Ceres to Jupiter, explains the appearance of Jupiter in the *Tabula Agnonensis*, as mentioned above. 224: On *ārputrati* see *TAPA* 57.56-7 (1926). As for **nuclipineum* (also 224), I am 'prepared to read *Constantiopolitanus* where the manuscripts have the monosyllable *O* if the sense demands it.' 226: *fahe* means 'smoked' (specifically beechnut) in contrast with *toco* 'salt' (or fat [bacon]). It is for an older **bhagio-* or **bhagejo-*, Latin *fagus*, *fageus* and—in Gaul *deus Bacon*, *DAG* 181 (with Remark) and *silua Bacenis* ib. 221—OHG *Buohhunna*; in *baco(n)-*, *bh* and *g* have become *b* and *k* respectively, but *ā* has not yet passed into *ō*, see Kluge, *Urg.* 33. 374: Add Sabine σόριον τὸ τῆς γυναῖκος αἰδοῖον (Plut. 2.288 f.), a gloss completely overlooked until now. I have discussed it in my review (forthcoming in *CP*) of Walde-Hofmann, part 20.

Lateinisches etymologisches Wörterbuch. By ALOIS WALDE, 3d ed. by J. B. HOFMANN. (Indogermanische Bibliothek, 2. Reihe: Wörterbücher, No. 1.) Vol. 2, fasc. 17-20, pp. 433-752. Heidelberg: Carl Winter, Universitätsverlag, 1951, 1952, 1953.

Reviewed by GEORGE S. LANE, *University of North Carolina*

The last review of any part of this work (fasc. 16) to appear in *LANGUAGE* is to be found in 27.162-4 (1951). That review, as also the reviews of preceding fascicles, was from the pen of Roland G. Kent. In taking over the task of reviewing the remaining fascicles of this monumental work, I feel that I must be allowed to express to the reader and to the revising editor my feeling of inadequacy as the successor to such a master of Latin linguistics as Kent.

The pages before me impress me, as has the entire work heretofore, with their saneness and mild conservatism. This is more than one can say for many of the etymological works of the earlier part of this past half-century. It is even more than one can say for the previous editions of this same book, and especially it is more than one can say about Walde-Pokorny's *Vergleichendes Wörterbuch der indogermanischen Sprachen*, of which this, in a very definite fashion, is an

heir. Many scholars at the turn of the century and in the first few decades thereafter took the most extreme pains to find an Indo-European etymology for every word of their favorite language, no matter how implausible the phonological (not to mention the semantic) assumptions might be. And not only did they take pains to make sure that an etymology was offered, but they laid great stress on interrelating the vocabularies of the Indo-European languages by means of the least possible number of 'roots'. This attitude toward etymology resulted in the assumption of root extensions and root determinatives galore. By means of these assumptions, words or groups of words could be made at least distantly cognate by simply writing their 'root' with a hyphen before the final consonant, which was then subject to variation without the necessity of explanation. This theory of root variability reached its apogee in Persson's *Beiträge zur indogermanischen Wortforschung* (Uppsala, 1910), which was really the final edition of his earlier *Studien zur Lehre von der Wurzelweiterung und Wurzelvariation* (Uppsala, 1891). Both these works epitomized the trends of their times in etymology and therefore had tremendous influence upon Indo-European etymological studies. Nowhere does that influence, conscious or unconscious, appear to be greater than in the above-mentioned work by Walde and Pokorny.

Now no one will deny the existence of root extensions—that is, that groups like Lat. *vertere*, Skt. *var̥tate*, etc. 'turn' are eventually related to Skt. *vr̥nakti*, *varjati* 'turn, twist', and that they represent different extensions of a simple radical element *wer-*, which may be expressed simply as *wer-t-* beside *wer-g-*. The problem that confronts us in such assumptions is where to stop. For example, if Goth. *wairpan* 'throw' and the like are to be put here as originally 'twist, turn (the arms)' from *wer-b-*, which is very plausible, what of Lat. *verbera* pl. 'rod, switch, punishment' and Lith. *viřbas* 'twig'? There seems to be no criterion of objective control, beyond that of pure phonological possibility; the rest is a matter of personal judgment.

This is the state of Indo-European etymological theory which Hofmann inherited from the earlier editions of Walde and especially from Walde-Pokorny. His has been a task of assembling, of sorting, and especially of judging. It is in the last capacity—in judging what is the most plausible, if any, of the dozens of etymological connections which have been offered for the uncertain part of the Latin vocabulary—that Hofmann has excelled. Actually, then, disagreements with him on this or that etymology can only be a matter of judgment. Some critics will find him either too daring or too conservative; but the middle-of-the-roads, like myself, can have little but praise for the result as a whole.

There is however one less desirable feature which this work shares with its predecessors, namely, the difficulty of style, especially as regards the use of parentheses and of brackets within parentheses. Perhaps the author does not realize how difficult it is for the uninitiated student to find his way unerringly through such mazes of parenthetical remarks and to come out in the end feeling that he really knows what the author thinks about a particular problem.¹

A thorough review of such a work as the one before us would necessitate a careful reading of every item and a minute examination of all citations and

¹ I may remark in passing that Pokorny's new *Indogermanisches etymologisches Wörterbuch* shows a great improvement over Walde-Pokorny in legibility.

references. This, I should think, is obviously impossible for any one reviewer to accomplish either in the time which he may allot to the task or in the space allotted to him by the editor of the journal for which he is writing. It therefore seems more proper for the reviewer to limit his efforts to some particular item on which he may offer constructive corrections or suggestions, rather than to follow an over-all hit-or-miss procedure. In my own instance, there is perhaps no more proper area of concentration than the Tocharian materials here presented, largely for the first time, in what will be a major reference work in Indo-European etymology. It is especially important that these be cited correctly, and that all possible added information be brought to bear on their internal relations and eventual outside connections. At least I shall give them precedence over other detailed comments.

439, s.v. *rōbus*: to Toch. A *orkām* 'darkness' add B *orkmo* adj. and subst. 'dark; darkness'. 445, s.v. *ruber*: the Toch. A form *rātram* (not *rātram!*), even though cited from the dual combination *rātram lymem*, had better be called fem. pl. (cf. SSS 115). Anyway it would seem permissible to cite simply an unattested nom. *rtār*, or at least the stem *rātr-*. To be consistent the B forms should also be cited with *rā-* instead of *ra-*. 454: Toch. B *orotse* is nom. sg. masc., *orocce* is merely obl. (the 'standard' orthography with *c* in place of *č* would seem preferable). 462, line 7: Toch. *ypā* 'machen'. This form is impf. act. sg. 3 of the suppletive verb *ya-*, *ypa-*. It might be best to cite pres. act. sg. 1 *ypam*, since the infinitive *yatsi* is from the other radical. 466, middle: the Toch. B form given by Sieg and Siegling, *Ud. Fr.*² 184 is *salyiye*, var. *sāle*. 468, s.v. *salīō*: one can safely add Toch. A, B *sāl-* 'leap' (A pres. act. pl. 3 *slīñc*, B inf. *šāllatsi*). Cf. van Windekens, *Lex. étym.* 111. 472, s.v. *salvus*: add perhaps A *salu*, B *solme* 'whole, entire'. Cf. op.cit. 114. 478, s.v. *sappīnus*: Toch. A *saku* is defined as 'saft'. The B form is *sekuwe*, which means 'Eiter, pus, suppuration' (*Ud. Fr.* 18a5). This meaning is probable also for A *saku* in the fragmentary passage 191a6 *ysār s[a]kuyo ypic* 'full of blood (and) pus'. This meaning makes Pisani's proposed etymology very doubtful. 509, line 5: for Toch. B *dsc-a-mem* read *āscameṃ* abl. sg. of *āšce* 'head'. The meaning given, 'von dem Anfang', is misleading and affects adversely the plausibility of Lidén's etymology. Likewise van Windekens' derivation (*Lex. étym.*) from **ak-t-* (*ak-* 'point') is to be rejected, since *šc* here is from *st*, cf. obl. sg. *āstām*. (Originally 'bone': Gk. *ὀστέον* etc.?) 519, s.v. *sequor*: add certainly Toch. A *sāk-* 'follow' (pres. mid. pple. *sāk[n]āmām*). 539, middle: the Tocharian forms should be: A *šoma-*, B *somo-* 'one' (in composition), A *sas* (m), *sām* (f.), obl. *šom* m., f.), B *še*, obl. *šeme* (m.), nom. obl. *somo* (f.). 558, s.v. *somnus*: for Toch. A *špām*, B *špāne* read A *špām*, B *špane*. 563, s.v. *soror*: add certainly Toch. A *šar*, B *šer*. 574, s.v. *spīna*: for Toch. A *špin-ae* read *špin-ac*. 586, s.v. *stāmen*: list with A *štām* (I should prefer to write *štām*) also B *stām*, either here or preferably under *stō* (598), with direct comparison (as in the latter place) to OHG *stam*, NHG *Stamm*. 598, s.v. *stō*: the Toch. B forms of the copula to be listed here are sg. 3 *ste*, with enclitic pronoun *star-* (*star-ñ*, *star-me*), once as sg. 2 *star*, pl. 3 *stare* (cf. Krause, *Westtoch.*

² The abbreviation stands for E. Sieg and W. Siegling, *Die Udānālānkāra-Fragmente* (Göttingen, 1949).

Gr. 255, *Ud. Fr.* 188). B *stare* means 'sie sind', not 'sie blieben'. On the listing of A *štām* 'tree' here along with B *stām*, cf. above. 629, s.v. *sum*: add also the Toch. impf. A *šem*, *šet*, *šeš*, etc., B *šeym*, *šait*, *šey*, etc. (cf. SSS 384 f.). 647, s.v. *tangō*: other forms from the Toch. B radical *tek-* 'touch' besides aor. *teksa* are e.g. pres. ind. sg. 3 *cešām*, subj. sg. 1 *teku*, opt. sg. 1 *tašim*, etc. (Krause, op.cit. 249 f.). 667, line 5 from bottom: for Toch. B *stap-* 'lau werden' read *tsāp-* 'zerstossen, zerquetschen'. Line 4 from bottom, for *stāk-* read *tsāk-* and for *stāk-* read *tsāk-*. As for the form in line 5 *tsatsā-pauwā* 'laue Sonne', cited from *MSL* 18.24, I find there rather *misa lykačke kekarčwa tsatsāpuawa* 'des viandes un peu grillées et tiédies' and *lykačke tsatsāpar* 'ayant légèrement tiédi'. 672, line 4 from bottom: for Toch. A *tsu* read *tsru*. 673, lines 20–21: Toch. A, B *triw-* means 'sich vermischen', caus. 'mischen', not 'zermalmen' (the form cited, *tetriwu*, is caus. pret. pple., not a finite form). 689, line 7: Toch. A, B *tāl-* 'lift', sg. 3 B *tāllam* (with *ll* < *ln*, cf. Lat. *tollō*); caus. pres. mid. sg. 3 A *tlāštār*, inf. *tlāssi*, pret. A *cacāl* (so read for *cakāl*, line 8), B *cāla*. 693, s.v. *torqueō*: Toch. A, B, *tsārk-* 'quälen' = *tsārk-* 'burn' (cf. e.g. *Ud. Fr.* 29a6 *emalyesa tssetsarkoš* trans. 'von Hitze gequält', lit. 'von Hitze gebrannt'). With *torqueō* compare rather Toch. *tārk-* 'drehen' (Krause, *Westtoch. Gr.* 248). 701, s.v. *tremō*: for *trāmiñe* read *tārmiñc* (SSS 442). B *tremem* 'das Zittern' is obl. pl.; nom. pl. would be *tremi* (cf. *Ud. Fr.* 131) but defined as 'Zorn, Skt. *krodha*'. 703, line 7: the forms of 'three' are Toch. A *tre m.*, *tri f.*, B *trai*, *trey m.*, *tarya f.* 704, s.v. *trīcae*: probably add Toch. A, B *trik-* 'err, be confused' (cf. van Windekens, op.cit. 140). 712, s.v. *tū*: add, of course, Toch. *tu*, B *tuwe*, *twe*, etc. 794, line 10: for Toch. A *kumnās* read *kumnāš*, and for Toch. B *kāmnaskem* read *kānmaskem* (*mn* > *nm* by regular metathesis).

Aside from these notes on the Tocharian materials, little has come to my notice which calls for correction or serious dissent; but, as I have indicated, serious disagreements with the author's judgment will arise only through use of the book as a work of daily reference.

I have noticed (as Kent did before me) the irritating use of *ae* for *æ* in Old English forms (e.g. 444, below middle). 449, line 18 from bottom: for OCS *ragati se* read *ragati se*. 465, line 11: Hitt. *sakhi* 'I know', *sekweni* 'we know' is certainly to be compared to Goth. *sah/sēhrum* 'saw'. Cf. Sturtevant, *Comp. gramm.* 240, 133. 477, line 2: for Skt. *sabar-(duk)* read *sabar-(dhuk)* or *sabar-(dugh-)*. 497, s.v. *scortum*: for *kvenns kinn* read *kvennskin* (i.e. *kvenn-skin*). 505, below middle: read kymr. *cainc* for *kainc*. 516, s.v. *sentio*: the connection of the Germanic group of Goth. *sinþs* 'time', *sandjan* 'send' with *sentio* has always seemed to me most dubious, in spite of the formal agreement. It is of course accepted by many, but always, it seems to me, as a pis-aller (cf. Feist 424). A semantic defense of the etymology is offered by Buck, *Dict. of selected IE synonyms* 1020 (Chicago, 1949). Incidentally, I fail to find a reference to this book anywhere. It could have been put to good use in just such instances as this.

All these things—misprints, errors, inconsistencies—are but minor blemishes on a work of this magnitude. There could be ten times as many, and in fact there may be, indeed probably are, yet they do not detract from the value of

Hofmann's revision of Walde. Any scholar will be able to correct all these minutiae for himself as they apply to his own field of research. The work will remain for a long time the standard reference work not only for Latin, but, so far as it suffices, for Indo-European also.

Historische Lautlehre des Lateinischen. By MAX NIEDERMANN. (Sprachwissenschaftliche Studienbücher.) 3rd ed. revised, pp. vii, 214. Heidelberg: Carl Winter, Universitätsverlag, 1953.

Review by FRED W. HOUSEHOLDER JR., *Indiana University*

I had intended to treat this book rather briefly, with references to reviews of earlier editions; but I find that it has never been reviewed in *LANGUAGE* at all. Indeed, the only review in English that I was able to locate is Carl D. Buck's review of the first French edition (*Précis de phonétique historique du latin*, 1906) in *The classical journal* for 1907. That version was followed by the first German edition (1907, almost identical), an English translation (*Outlines of Latin phonetics*, 1910), a second German edition considerably revised and enlarged (1925), a second French edition with still further expansion (1945), and now the third German edition. The original version of the book had 95 sections; the latest has 113.

Throughout these editions, the author's general purpose and his stand on various disputed points have remained substantially the same. The book aims to present the historical development of Latin phonology (with some phonemic theory implied but none explicit) from the Latin evidence alone, without comparing Greek or other cognate languages—even Italic languages; the time span is chiefly from Old to Classical Latin, but there is some reference to earlier and to later developments. This aim the book fulfills very well; it is far easier to use and more pleasant to read than Kent's slightly more ambitious work *The sounds of Latin* (1932, 3rd ed. 1945).

Among Niedermann's more debatable positions are the following. He holds the French view that the Classical Latin accent, even in Plautus' time, was a pure tonal accent, without any stress either phonemic or nonphonemic—a view shared by few scholars in this country (14–9, §§8–11). He believes that *ae* and *oe* represented long monophthongs in Classical times (67–72, §§31–3). He holds the view (largely ignored by both Kent and Sturtevant) that the contrast between light or palatalized *l* and dark or velarized *l* was phonemic in Classical times, as in *stella* /stel'a/ 'star' versus *tela* /te'la/ 'weapons' (§68). He believes that /h/ was completely lost by 200 B.C. in all types of Latin, and was artificially reintroduced a century later (with what guide to its pronunciation?) only in elegant upper-class speech (§52). And he holds that *qu* represented a unit phoneme, not a cluster (§43). Though one cannot agree with him in all these opinions, it is interesting to read his arguments and to restudy the evidence for them.

There are of course minor changes from the earlier editions on nearly every page; but the wholly new sections—the ones not in the French edition of 1945—are few: §21, a brief explanation of the special character of final syllables, regard-

less of the position of the stress; §37, a statement of unsolved problems related to the lengthening of original short vowels before *-gn-*, *-rC-*, and *-nC-*; and §§105-9, an extended treatment of dissimilation, metathesis, and similar phenomena. In short, then, if you own the French edition of 1945, you may not want to buy this one; but if you have only the second German edition of 1925, the new version is a great improvement. In its rather special field, it is THE book.

L'aspect et le temps dans le verbe néo-grec. By HANSJAKOB SEILER. (Collection de l'Institut d'Études Byzantines et Néo-helléniques de l'Université de Paris, No. 14.) Pp. 171. Paris: Société d'Édition 'Les Belles Lettres', 1952.

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Seiler's book is welcome for two reasons. First, it deals with a language that constitutes, as few other Indo-European languages do, a mine of the most intriguing problems, almost completely unworked by the methods of structural linguistics. There is an excellent grammar, published in the twenties but completed in manuscript as early as 1911, which may be considered an ingenious precursor of modern methods in any language: L. Roussel, *Grammaire descriptive du roméique littéraire* (Paris, 1922); there are, furthermore, several valuable studies by André Mirambel, most of which have appeared in the *Bulletin de la Société de linguistique de Paris*; and that is about all. Second, the topic of Seiler's investigation is well chosen from the standpoint of general linguistics: the relation between aspect and tense. Both are subdivisions of the larger field of verbal categories; the complicated relation between the two is a vital chapter in the analysis of the Indo-European verb.

After an Introduction (15-25) and a morphemic interpretation of the verbal forms (27-41)—where, apparently for the sake of completeness, dialectal and less common colloquial forms are unfortunately grouped together with the standard forms—there is a discussion of the three binary oppositions that contrast as to aspect (but not as to tense): the two futures, the two subjunctives, and the two imperatives, each of which has one form to express imperfectivity and one to express perfectivity (42-54). Then comes the main part of the study: an interpretation of the aorist indicative (55-91) and of the forms containing the present stem: the present (92-112) and the imperfect (113-47). At the end there is a discussion of a few less common patterns, such as the perfect or compounds of the type *κάθεται καὶ τρώγει* 'he keeps eating', lit. 'he sits and eats' (148-63), followed by the Conclusion (164-66).

Seiler's study is a valuable contribution to the discussion of Modern Greek and of the verbal categories: it is written by a professional, who knows the language he is dealing with and the methods he is applying. In several respects he goes beyond his predecessors and tries, often with good results, to present more subtle solutions to the problems under discussion. Essentially, the book consists of a large number of examples individually interpreted. This material corroborates the traditional assumption that in Greek, in the hierarchy of categories, aspect ranks higher than tense: aspect is always indicated, tense often is not. The second significant finding of the study is that of morphemic

multivalence: the meaning of a form may vary and can only be established through its context. Thus the aorist indicative in certain environments has the meaning 'past', in others it has not. Seiler devotes much of his book to just this problem, with gratifying success. The principle of morphemic multivalence is established for the whole verb system.

The book of course has features that rouse objections—some to the whole, some to individual points. Among the latter are various instances where Seiler's interpretation of a given Greek expression is unconvincing. A few typical examples follow. Seiler says: 'Au lieu de *ἄρρωστώ* "je suis malade", on pourrait, en effet, imaginer *εἶμαι ἄρρωστος* sans changement sensible de sens. De même *εἶμαι ἄργιος* au lieu de *ἀργιέομαι* "je suis furieux"' (109). These patterns, however, are not equivalent: *εἶμαι ἄρρωστος* and *εἶμαι ἄργιος* mean respectively 'I am ill' and 'I am angry', whereas *ἄρρωστώ* and *ἀργιέομαι* mean, with clear aspectual distinction, 'I fall ill' and 'I become angry'. — Seiler analyzes the common Greek pattern of the repeated imperfects, such as *πῆγαινε πῆγαινε* 'she was walking and walking', as 'une seule action qui est décomposée en un certain nombre d'étapes' (130). The concept of 'étape' is not clear to us: perhaps Seiler's interpretation refers to the genesis of the pattern; descriptively, the repetition of verb forms is equivalent to the adverb *ἀδιάκοπα* 'without interruption'; it does not have the meaning 'accumulation' but the meaning 'continuity'. — The form *ἀκούς*, around which Seiler builds certain theories (134–5), can hardly be considered a verb form any longer in the context given; historically, it is of course the second singular of the present of *ἀκούω* 'hear'; in usage, however, it has become an element of emphasis, without verbal categories. — Seiler discusses the expression *ἔπεσε νὰ κοιμηθῇ* under the heading of 'expressions composées' and translates it by 'elle s'endormit' (161); neither heading nor translation is justified: this is not a compound expression, but the sequence of a superordinate and a subordinate verb with the meaning 'she lay down in order to sleep'.

Of the more general features, the selection of the text investigated deserves comment. There is very little material derived from informants; some examples are excerpted from literary sources, but most are taken from folk tales and folk songs. The latter genres with their somewhat stereotyped style were perhaps not the happiest choice; and contrasting examples within corresponding frames, as elicited from informants, would have illustrated Seiler's points in a clearer, less sophisticated way. A marked weakness of the book is the lack of a systematic summary of the results, listing the various functions of the various forms, as well as the various patterns of oppositions. Furthermore, to judge from the bibliographical indications, the massive findings of Danish and American scholars in the field of structural linguistics have been completely ignored. The methods worked out in the last twenty years or so by Jakobson, Hjelmslev, and a number of American linguists would have greatly enhanced the value and validity of Seiler's study. Some of his definitions lack the necessary precision and clarity; for instance, we have not been able to grasp clearly the role of the relation between verb and actor in Seiler's system. Sometimes it seems to us that this relationship is determined by non-linguistic considerations; thus, 'seul le

sujet parlant peut introduire la notion de temps qui relève du moment actuel où il parle' (164). The definition of 'aspect' which Seiler presents (166) is equally non-linguistic:

La catégorie des aspects qu'on voit ainsi dominer tout l'ensemble du verbe est une catégorie concrète. Elle met en évidence le caractère que possède une action aux yeux du sujet. Elle sert à la "*Veranschaulichung*" de l'action.

'Tense', on the other hand, is defined thus (166):

Les relations temporelles par contre ne se soucient pas du caractère de l'action; l'action est alors conçue en soi, comme quelque chose d'abstrait.

These contrasts: 'concrete' vs. 'abstract', and 'related to the action' vs. 'conceived in itself', are (like several others) not easy to work with. We should like, therefore, to suggest a simpler and purely linguistic approach to the problem. In an interpretation of the verbal categories on which we have been working, and of which the first two tentative applications have been published,¹ the attempt is made to establish the grammatical morphemes of verb forms as the elements which, through their relation to the other parts of the sentence, bind the whole sentence together; tense and aspect in this system are then the elements that express, in the verb, the meaning of the non-required modifier, i.e. the adverb. There are, however, two adverb patterns, one with the meaning 'date', the other with the meaning 'perfectivity' (including of course 'imperfectivity'); the hierarchic relation between these adverbs varies from system to system: in Portuguese, date, more frequently present, is the primary, and perfectivity, less frequently present, is the secondary adverb. In Modern Greek the relation is the opposite: perfectivity is always present, date not always. Why this should be is difficult to say: non-linguistic phenomena may have influenced the pattern of the language, or language patterns may have shaped the non-linguistic interpretation of the stimuli.

In the following we should like to present a few comments on individual points of general linguistic interest, and reinterpretations thereof, mostly by applying the approach just outlined: that aspect and tense express the relation between adverb and verb, and not, as Seiler suggests, the relation between subject and verb.

A tentative definition of aspect. In a discussion of the temporal use of the aorist (82-8), Seiler says: 'Souvent ce caractère temporel de passé a besoin d'être renforcé par des adverbes de temps' (83). This brings up the interesting question of what determines what. To Seiler the adverb is redundant; to us, the verbal ending. This is more than a subjective preference. Since it can be shown that in all other instances the verbal endings are only the reflex of—or are determined by—some other element of the sentence, it may be assumed that the tense morphemes are no exception: they reflect the meaning of the (expressed or unexpressed) adverb. The semantic interpretation of adverbs is, of course, a thorny undertaking. What needs to be done (and has not been done, so far

¹ Kahane and Hutter, The verbal categories of colloquial Brazilian Portuguese, *Word* 9.16-44 (1953); Kahane and Saporta, The verbal categories of Judeo-Spanish, *Hispanic review* 21.193-214, 322-36 (1953).

as we can see, for any language) is an inventory of the various aspectual and temporal meanings of the adverbs; it would contribute considerably to an understanding of the respective verbal system. One Greek example will illustrate what we mean: 'Εναν καιρό γήμουν πουλί και πήγαίνα πετώντας τώρα με κόψαν τὰ φτερά ... 'Autrefois, j'étais un oiseau et j'allais en volant, maintenant ils m'ont coupé les ailes ...' (88, No. 8). Seiler comments: 'L'aoriste peut exprimer des faits du présent actuel; on peut y ajouter τώρα ["now"]'. But it is precisely the adverb (whether expressed or implied) that determines the verb form and not, we think, the verb form that determines the adverb. The range of meanings of this (as of any) adverb is wide, to be sure, and often difficult to describe. An utterance such as *μὴ φορὰ μ' ἀγαποῦσε ὁ Νίκος* 'once Nikos used to be in love with me' may be in (semantic) opposition either to *τώρα με ξυλίζει* 'now he keeps beating me up' or to *τώρα με ξύλισε* 'just now he beat me up'. In the first instance, *τώρα* means 'at the present time'; in the second instance, 'recently'. (A methodological difficulty here is of course that in analyzing written material we often need the verb form in order to establish the exact meaning of the adverb which has determined that verb form; speech act and speech analysis proceed differently.) Of the numerous instances in Seiler's book that suggest a reinterpretation in terms of the meaning of the adverb, we should like to mention just two more. Seiler contrasts (43, No. 4) two futures in apparently identical environments: the imperfective *θὰ σᾶς ἀκούω ὅταν θέλετε* 'I'll always listen to you, whenever you want' and the perfective *θὰ σᾶς ἀκούσω ὅταν θέλετε* 'I'll listen to you; just say when'. The two *ὅταν* expressions are adverbs determining the two contrasting forms *ἀκούω* (imperfective) and *ἀκούσω* (perfective), but they are not identical; the two *ὅταν* forms differ in intonation and in meaning. The verb form *θέλετε* in the second (perfective) example may be replaced by the variant *θελήσετε*, whereas the *θέλετε* form of the first (imperfective) example cannot be replaced without a change in meaning. In other words, the two apparently identical but really different adverbs, contrasting in 'aspect', determine the two aspectually contrasting verb forms *ἀκούω* and *ἀκούσω*. The last adverbial pattern on which we should like to comment is intended to point up the well-known difficulties of semantic interpretation; the specific problem under discussion is the term 'often'. Seiler interprets the following text: *Στὴν Ἀθήνα σὰν πᾶμε χίλιες φορές θὰ τ'ἀκούσης: ὁ τόπος πρόδεψε θαυμάσια ...* 'Quand nous irons à Athènes, tu entendras (dire) mille fois: Le pays a fait des progrès formidables ...' (45, No. 6). Seiler sees that the perfective form *ἀκούσης* 'you will hear' depends on *στὴν Ἀθήνα σὰν πᾶμε* 'when we go to Athens', not on *χίλιες φορές* 'a thousand times'; and he suggests the interpretation of *χίλιες φορές* as a 'trait secondaire' (45). We wonder whether this element 'a thousand times' may not have simply the perfective meaning 'then'; the following contrast may support our interpretation: *Αὐτὸ τὸ τραγούδι τὸ ἀκουσα* [perfective] *πολλὲς φορές* 'I have heard this song many times' vs. *Κάθε φορά πού περνοῦσα ἀπὸ τὸ σπίτι της, τὴν ἀκουα* [imperfective] *νὰ τραγουδάη* 'Every time I went past her house I heard her singing'. The adverb *κάθε φορά πού περνοῦσα* 'every time I went past' in the second example has the meaning 'repetition': it forces the imperfective form of the verb.

But the adverb *πολλές φορές* 'many times' in the first example means 'already', and determines the perfective form of the verb.

Finally, in this general discussion of aspect, we should like to mention an interesting linguistic function of Greek aspect to which Seiler draws our attention (117-8): aspect may be a signal indicating the grammatical subject. This phenomenon is connected with the verb *βρίσκω* 'find': *βρήκε τὴν κόρη κι ἔκλαιε*, where both verbs are perfective, means 'he found the girl and wept'; but *βρήκε τὴν κόρη κι ἔκλαιγε*, with the first verb perfective and the second verb imperfective, means 'he found the girl and she was weeping'. The first pattern, with both verbs perfective, indicates identical subject; the second pattern, with the second verb imperfective, indicates that the second action was going on at the time when the first action occurred: the difference in aspect may be the only indication of the difference in subject.

Aspectual contrast between aorist and present. In his discussion of the aorist indicative Seiler reaches the conclusion—mostly by means of two not entirely firm criteria: juncture [*le narrateur marque une pause dans la série des actions alignées*] and the relation of speaker to speech [*l'association qui existe entre l'agent et son action de faire l'énoncé*]²—that the opposition between aorist and present is often that between 'irrelevancy of aspect' (he uses the term 'fait pur') and 'expression of the aspect of imperfectivity' (60). His examples are not sufficiently numerous and clear-cut to prove his point: in some, the two forms are (to us, at least) in free variation (e.g. 60, No. 4); in others (e.g. 61, No. 5) the aorist is strongly aspectual, the present weakly so. We do not, of course, want to argue about the aspectual interpretation of individual examples; the reason for bringing up this point is methodological: it is relatively easy to note an aspectual difference between two variants but much more difficult to state the exact difference; the native speaker's *Sprachgefühl* may be as unhelpful in structural analysis as a dogmatic *Systemzwang*. Yet the selection between potential variants is up to the native speaker: in the aorist examples *σὰν τοῖς βρε* 'after he found' (59, No. 1) and *ἀφ' οὗ τοῦδ' ὤσε* 'after he had given him' (59, No. 2) the verbs are used in the aorist because the function words *σὰν* and *ἀφ' οὗ* 'after', with the grammatical meaning 'perfectivity', do not admit the present. And in the example ... *τοὺς ἔφερα καὶ κρασί* /-/ '... Je leur apportai aussi du vin' (61, No. 5), the form of perfectivity, i.e. the aorist, is required, in the context given, by the terminal intonation meaning 'sentence end': 'And then, FINALLY, I also brought them wine'. The conclusion: Seiler's hypothesis that the aorist expresses 'fait pur' needs a good deal of further investigation.

Multivalence of the morpheme 'imperfect'. In his detailed interpretation of the imperfect (113-38) Seiler stresses the dichotomy of this form: it expresses both tense and aspect. As tense it is relative, i.e. a tense which always presupposes another one: 'he was doing something' requires a statement, expressed or implied, about some other occurrence at the same time. In Greek, the 'imperfect' seems to be the point where aspect and tense are most closely interlocked. What we miss in Seiler's book is only a clear-cut opposition between relative and non-relative (absolute) tenses—in other words, a discussion of

the Greek tense (not aspect) system as a whole. Does it constitute an interwoven pattern of time (present vs. past vs. future) and relativity (relative vs. absolute)?

The discussion of the imperfect comprises the so-called conditions contrary to fact. In Greek (to make a long story short) these contain $\alpha\upsilon$ plus the imperfect in the protasis and $\theta\acute{\alpha}$ plus the imperfect in the apodosis. Seiler uses a somewhat complicated system to explain the structure under discussion, and concludes that it is, above all, a phenomenon of mood. In this we agree with him; but we wonder whether the explanation could not be considerably simplified. These imperfects are clearly not in relation with any adverb with the meaning 'tense' or 'aspect'. This excludes the interpretation of the imperfect as tense or aspect. We find in Greek, then, the same structure of conditional clauses as in the other western languages: the verb forms in this structure are variants of the morpheme 'imperfect', independent of the same verb forms appearing in other structures in which they are in relation with adverbs. The form patterns in protasis and apodosis may change from language to language; but the grammatical relation between the two verbs (the mood) remains the same.

Praesens historicum and praesens pro futuro. Seiler coordinates (97 ff.) two interesting patterns of transposition typical of Modern Greek. It is possible (and quite common) to substitute the imperfective present for the perfective past (the so-called 'historical present') and for the perfective future. Seiler concludes, quite correctly, that these patterns of substitution constitute temporal neutralizations. We should like to go one step further: both the praesens historicum and the praesens pro futuro show neutralization (or at least repression) of the category 'tense' in favor of the category 'aspect'. The praesens historicum remains a past tense, to be sure; in a language such as substandard German we can even keep the adverb with the meaning 'past' in a praesens historicum structure: *Gestern gehe ich den Kurfürstendamm entlang* 'Yesterday I am walking along K. Avenue', though in Greek the adverb 'past' cannot be so used. The essential thing is that a semantic category which usually appears in a perfective form is expressed by a non-perfective form. Two phenomena support this assumption of aspectual transposition. The first is the Greek form of the praesens pro futuro. Greek has two future stems, the perfective and the imperfective. Now, only the perfective future may be replaced by the present, which belongs to the imperfective forms: the perfective future $\theta\acute{\alpha}$ *πολεμήσουμεν* 'we'll fight [this once]' is in free variation with the imperfective present *πολεμούμεν* (99, No. 5). (Incidentally, the imperfect with its strong temporal meaning can not replace the aorist.) This means that with both past and future a repression of tense is connected with a stressing, through shifting, of aspect. The second corroboration of our hypothesis is found in a reverse phenomenon: sometimes the present and—probably through this—the perfective future are replaced, with neutralization or at least repression of the category 'tense', by the perfective aorist: e.g. the common exclamation *ἔπρασα!* lit. 'I have arrived', used by waiters to indicate their intention of approaching the table of the customer (67), has just the one meaning 'perfectivity', used for some psychological reason instead of the corresponding and more frequent imperfective patterns. The same re-

placement of imperfective by perfective is also found in Romance: cf. Judeo-Spanish *Agora se vido ke Sabetdi es un alenari* 'Now we see that Sabetai is a gangster', where the perfective preterit *vido* replaces a more common present; *Si no fut el martes, no va tener más ora* 'If I don't go Tuesday, I won't have any other time', where the perfective preterit *fut* replaces a more common future (Kahane-Saporta 2.122).

Now, it is interesting to see that in certain instances the (perfective) aorist cannot be replaced by the (imperfective) present—for instance when the conversion from perfectivity to imperfectivity would change the meaning of the utterance in some essential feature. We do not believe, as Seiler suggests (77–80), that it is a problem of 'actualisation' or of the meaning of the verbs or of the role of the subject: 'ce sont toujours des expressions qui se rapportent à la sphère de l'âme, du sentiment, de l'intellect' (78–9); 'ce sont des expressions se rapportant à l'état d'âme, à la pensée du sujet ou qui intéressent autrement le sujet de l'action' (80). We think that it is the strong perfective character of the adverb or some adverb-like element that determines the form of the verb. A few examples: *ἡ βασιλοπούλα ποὺ κόμα δὲν κοιμήθηκε μὲ τὰ σωστά της* ... 'la princesse ne s'étant pas encore endormie ...' (79, No. 10): the aorist *κοιμήθηκε* depends on the adverb *ἀκόμα* ... *μὲ τὰ σωστά της* '(not) yet ... definitively', verb and adverb together having the meaning of 'fell asleep', whereas the present *κοιμάται* (without the adverb) would mean 'sleeps'; it would be structurally possible but would convey a different meaning. ... *ρίχνει μιὰ φωνάρα ποὺ τράνταξαν τὰ γυαλιά* '... elle lance un cri tel que les vitres tremblèrent' (79, No. 8): *τράνταξαν* 'trembled' is perfective, determined by the adverb-like element *ρίχνει μιὰ φωνάρα ποὺ* 'she uttered such a shout that' and expressing inchoativeness, 'the windowpanes began to tremble', a meaning that would be lost by the use of the present *τραντάζουν*. In *Πέφτει ἀπὸ τὸ χέρι τοῦ παιδιοῦ καὶ τὸ δαχτυλίδι ἐχάθηκε* 'La bague tombe de la main de l'enfant et disparut' (80, No. 11), the aorist *ἐχάθηκε* 'disappeared' carries the meaning of the (unexpressed) adverb *γιά καλὰ* 'for good', a connotation that the present, structurally possible here, would not carry. Finally, the example *Πηγαίνει στὸ βασιλιά καὶ τοῦ τὸ λέει. Χάρηκεν* [perfective] *ὁ βασιλιάς νάκούση ἓνα καλὸ λόγο* ... 'Il va chez le roi et le lui dit. Le roi était [?] content d'entendre une bonne nouvelle ...' (77–8, No. 2). The meaning of the aorist *χάρηκεν* 'became happy' is, again, determined by the environment: the perfective *νάκούση* 'that he heard (this once)' forces the perfective form of *χάρηκεν*. That the lexical meaning of the verb *χαίρωμαι* 'am happy' has no influence on the selection of its aspectual form, but that this form is determined by the aspectual environment, is shown by the two following contrasting pairs: *χάρηκα* [perfective] *ποῦ σὰς γνώρισα* [perfective] 'happy [I became happy] to have met you' vs. *χαίρωμαι* [imperfective] *ποῦ σὲ βλέπω* [imperfective] 'happy [I am happy] to see you.' To summarize: in colloquial Greek, with a shift of the aspectual and a repression of the temporal categories, the perfective aorist may be replaced by the imperfective present, provided the text does not contain some strongly perfective connotation.

Expressions of 'probability'. Seiler contrasts the two futures, the imperfective and the perfective (43). The perfective pattern *θὰ χτίση* 'he will build' refers to

a future event; the imperfective pattern *θα χτίζει* 'he is probably building' refers to an event taking place, according to the speaker's belief, in the present. To this expression of the so-called 'future of probability', which is widespread in the Indo-European languages, Greek adds an interesting ingredient: aspect. In most western languages the future of probability has the structure: adverb with present meaning plus verb in the future (e.g. Germ. *Wie ich ihn kenne, wird er jetzt bei Horcher sitzen* 'Knowing him, I am sure that he is sitting right now in the Horcher restaurant'); Greek, however, uses only the imperfective future, correlated of course with an adverb meaning 'present'. In expressing probability in the past (called in Spanish grammar 'conditional of probability'), Greek has its own patterns, presented perhaps a little too sketchily in Seiler's study (147): it uses the future element *θα* with the indicative of either form of the past, the imperfect or the aorist. The two forms, *θα* plus the imperfect and *θα* plus the aorist, contrast with *θα* plus the present as to time; they contrast with each other as to aspect, the imperfect of course expressing imperfectivity and the aorist perfectivity. Example: *Ἐκείνα τὰ χρόνια οἱ ἄρκοντες θάτρωγαν* [imperfective] *ἀκόμα χαβιάρι* 'In those days, the upper class probably still ate caviar' vs. *Ψὲς στὸ δείπνο θὰ φάγανε* [perfective] *δεκαπέντε κοτόπουλα* 'Yesterday at supper they must have eaten fifteen chickens'.

The contrast between the two subjunctives. Seiler contrasts (48) two different forms, the aorist subjunctive and the present subjunctive, within identical environments: *κανένας δὲ βρέθηκε ποὺ νὰ τὴν ἀγαπήσῃ* [aorist] vs. *κανένας δὲ βρέθηκε ποὺ νὰ τὴν ἀγαπᾷ* [present]. The first he translates, 'Il ne s'est trouvé personne qui ait pu l'aimer'; the second, 'Il ne s'est trouvé personne qui l'ait aimée et qui l'aime encore'. The first is to him 'constation de pur fait; aucune perspective sur l'action'; of the second he says: 'La perspective, dans ce dernier cas, paraît s'étendre cette fois-ci, temporellement parlant, en arrière.' In other words, in this contrast the present subjunctive expresses, in Seiler's interpretation, aspect; the aorist subjunctive does not. Without denying the possible validity of Seiler's analysis of the text (or his informant's), we see the two members of the opposition as expressing 'perfectivity' vs. 'imperfectivity'. The first example means, 'Nobody was found who fell in love with her', with the aorist subjunctive expressing not 'pur fait' (i.e. irrelevancy of aspect) but 'inchoativeness', a subclass of perfectivity. The conclusion to be drawn is essentially methodological: we need to get more material from informants, in clear-cut contrasting patterns.

The imperative, expression of a vocative subject. Seiler discusses (52-3) the interesting fact that the basic principle of the Greek verb structure, the opposition of the two stems (aorist and present) contrasting in aspect, seems not to be consistently in force in the imperative system. There is a group of verbs that appear in the singular imperative with the present stem only (e.g. *πῆγαινε* 'go!'; *τράβα* 'pull!'), and others that show a similar preference for the aorist stem (e.g. *ἔλα* 'come!'; *ἀνέβα* 'go up!'). The reason for this phenomenon is to be found, according to Seiler, 'derrière la nature même de l'imp. 2^e p. sg.: c'est la forme pure du verbe, absolue, non-syntaxique (c'est-à-dire indifférente aux emplois syntaxiques)'. We do not believe that the imperative is non-syntactical;

we should rather suggest an interpretation of the imperative as a verbal category different from the others. Whereas the other categories express the relationships between the verb and the adverb, the object, another verb, and the actor, the imperative is the linguistic means of expressing the relationship between the verb and the actor when the latter is a vocative. This primary relationship with the actor seems to lessen the significance, in the instances mentioned by Seiler, of the secondary relationship with the adverb.

In this context, we should like to mention a pattern for which we have a different explanation from Seiler's. He quotes a passage from Thrace containing a description of magic customs; in this passage there are several perfective futures: *θα πιάσ'ν 'πὸ τὴν τρύπα* 'they will pass by the hole'; *θα κάμουν ἀγιασμό* 'they will celebrate the blessing'; *θα διαβάσ' τοῦ Μοδέστου τὴ φυλλάδα* 'he will read in the book of Modestus'; *θα εἶνι οὖλους οὐ κόσμους ἱκεῖ μαξιμένους* 'everybody will be gathered there' (104-5, No. 2). Seiler comments: 'L'emploi du temps futur pour rendre des habitudes est assez surprenant, sans doute' (105). These perfective futures do not, in our opinion, express 'habit' but 'imperative'.² In Modern Greek this pattern is quite common, particularly in a series: *θα πᾶς στὸ Νίκο, θα τοῦ πῆς ...* 'You must go to Niko and tell him ...' is more or less equivalent to *τὴ γαῖαν στὸ Νίκο, πῆς του ...!* 'Go to Niko, tell him ...!'

The gerund, expression of a coordinated imperfective. Seiler draws our attention to the fact that the typical contrast between the perfective (aorist) stem and the imperfective (present) stem (which leads to two futures, two subjunctives, two imperatives) does not appear in the active participle: there is only one, the imperfective (37-8). This seems an interesting observation: it fits very well into the general system of the Western languages, where the gerund (whatever its name) is equivalent to a coordinate verb with the aspect of imperfectivity. The equivalence can be established through transposition. Example: *τρώγοντας καρύδια τσάκισα ἓνα δόντι* 'eating nuts I broke a tooth' is equivalent to *ἐνῶ ἔτρωγα καρύδια τσάκισα ἓνα δόντι* 'while I was eating nuts I broke a tooth' or, still simpler, to *ἔτρωγα καρύδια καὶ τσάκισα ἓνα δόντι* 'I was eating nuts and broke a tooth'. Here, the structures with *ἐνῶ* 'while' and with *-οντας* '-ing' are in (potential) free variation with, and therefore equivalent to, the coordinate structure with *καὶ* 'and'. The *-οντας* structure, however, is not used as the equivalent of a coordinated perfective. For this, Greek has another pattern at its disposal: the passive or neutral form in *-μένος* of the perfective stem: *ἔφυγε φαγωμένος* 'il partit ayant mangé', which is equivalent to *ἀφοῦ ἔφαγε, ἔφυγε* 'after he had eaten, he left' or, still simpler, *ἔφαγε κ'ἔφυγε* 'he ate and left'. The two nonfinite forms (imperfective stem plus *-οντας* and perfective stem plus *-μένος*) are then in aspectual contrast: the former expresses 'imperfectivity', the latter 'perfectivity': both express 'coordination'.

These notes have grown out of Seiler's challenging study. His careful and professional investigation will, we feel sure, evoke further discussion of this complex and significant topic, the relation between tense and aspect. It constitutes a real step toward the structural analysis of Modern Greek.

² Cf. J. Wackernagel, *Vorlesungen über Syntax* 1.195 (Basel, 1920).

A grammar of the Macedonian literary language. By HORACE G. LUNT. Pp. xv, 287, with folding map. Skopje, [Macedonia], 1952.

Reviewed by GORDON H. FAIRBANKS, *Cornell University*

This is the first extensive grammar of Macedonian written in English. R. G. de Bray, in his *Guide to the Slavonic languages* 243-313 (London, 1950), has a short description of Macedonian, but it is oriented towards the comparative point of view. Lunt's grammar is written from a 'strictly linguistic point of view' (viii), although it is also intended for the non-linguist who is interested in studying Macedonian. For the student who is interested in the language from the practical point of view the text should be welcome; it is much more inclusive than de Bray's, and the reading selections and vocabulary should be very helpful. For the linguist it is the best description we have so far had of Macedonian, although it is to be accepted with a number of reservations—which do not, however, affect the general validity of the work.

The book is divided into three parts: I. A grammar of the Macedonian literary language (9-104), II. Reading selections (105-85), and III. Vocabulary (187-287). Part I is subdivided into three chapters: 1. Phonology and orthography, 2. Morphology, and 3. The verb. Chapter 2 really treats the morphology and syntax of the substantive and of uninflected forms, while Chapter 3 treats the morphology and syntax of the verb.

The phonology is for the most part stated clearly and adequately; and the orthography is stated in terms of the phonology. Most forms in this section are given in phonetic or phonemic form. The segmental phonemes are classified as follows (9): 'five vowels (i, u, e, o, a), four semi-vowels (r, j, l, ɫ), three nasal consonants (m, n, ŋ), nine pairs of consonants with the opposition of voicing including four pairs of stops (p/b, t/d, k'/g', k/g), three pairs of fricatives (f/v, s/z, š/ž) and two pairs of affricates (c/ɟ, č/ǰ) and a non-paired fricative (h).' Since /r/ functions syllabically and nonsyllabically (11), it might be better not to group it with /j, l, ɫ/, which do not function syllabically.

There is some problem about the phonemic status of [j], although this problem is not stated very clearly (14). Lunt presents the following data: the combinations [ja, jo, ju] contrast respectively with [a, o, u] in initial position, [ja, ji] with [a, i] in postconsonantal position, [ij] with [i] in interconsonantal position; the combinations [je, ji] are in free alternation with [e, i] after another vowel, [ij] in free alternation with [i] before another vowel. Further, other vowels occur in geminate clusters, e.g. [ee] and [ea]; but where we might morphologically expect to find a cluster of three vowels, we find only combinations with [j] between the second and third vowel: [eeja] occurs, but [eea] does not. Lunt points out that if the contrast between [ij] and [i] interconsonantly does not actually occur in normal speech (he is not sure whether it does or not), then [i] and [j] could be considered allophones of the same phoneme by introducing a phonemic feature of syllabicity (14), although he does not mention what advantage would be gained thereby. He does not know, apparently, what to do with the phonologically conditioned [j]; for he says at one point, 'when /e/ and /i/ follow vowels, however, it is not clear whether the phonetic [j] which frequently is

heard is phonemic, for there is no possible contrast' (14); and at another point, speaking of the [j] that occurs in what would otherwise be clusters of three vowels, he says that 'this [j], being automatic, may also be termed non-phonemic' (14). The main problem here may be stated as follows: when a phone in some positions is in contrast with other phones and with zero and must be considered a phoneme, but in certain other positions is either automatically determined or in free variation with zero, how are we to analyze it in the latter set of positions? It is possible to find extreme cases of this situation in different languages. American English presents one extreme. Excluding those dialects which have [srɪmp] 'shrimp', the combination [str] does not contrast with [sr] after pause; in this position the [t] is automatic, and it would be unambiguous to phonemicize *stream* either as /strɪjm/ or as /srɪjm/.¹ East Armenian presents the other extreme. The phone [ə] contrasts with other phones and with zero in only two positions, after a consonant and before plus juncture ([C+] contrasts with [Cə+]), and between consonants before plus juncture ([CC+] contrasts with [CəC+]). An example of the contrast in the first position is [barek'ám] 'friend' and [barek'ámə] 'the friend', which may be phonemicized as /barek'ám+/, barek'ámə+/. The phone [ə] occurs in many other positions, but always either in free alternation with zero, as in [harcənél] or [harcnél] 'to ask', or phonologically conditioned, as in [xəmél] 'to drink' (the combination [xm] never occurring after plus juncture). Again it would be unambiguous to phonemicize either as /harcənél, xəmél/ or as /harcnél, xmél/. We have a strictly rigorous analysis if we consider the phone phonemic only in those positions where it presents a contrast, and also if we consider it phonemic wherever it occurs. In analyzing a particular language there may be some statistical, phonological, or morphological reason for adopting a particular analysis that is consistent in itself, but is different from either of the two suggested here. If Lunt had stated which analysis he preferred and had given his reasons, he would have made the reading of the rest of the grammar easier and would have avoided inconsistent statements.

The Macedonian stress is said to be 'non-phonemic, and for the most part automatically determined' (21), falling on the antepenult where possible. Lunt cites a number of possible exceptions. There are four minimal contrasts, e.g. the adverbial form *godindva* 'this year' and the noun form *godínava* 'this year'. This is said to be a special case, since the stress in the adverbial form is often accompanied by a long vowel. The adverb *odvaj* and certain qualitative and quantitative pronouns have alternative stress positions. The verbal adverb is normally stressed on the penult, although many speakers prefer to stress the antepenult. Many recently borrowed forms stress the penult, such as *literatúra* 'literature', but these also may occur with stress on the antepenult. At this point, if we wish to consider all the forms with stress not on the antepenult as foreign to the literary language, we can agree with Lunt that stress is non-phonemic for this

¹ It is true that the [t] in *stream* contrasts with [p] and [k], as in *scream* and *spreed*; but the [j] of Macedonian also contrasts in the pertinent positions with [l]. If, however, some one phone is to be regarded as automatic and without phonemic status, then for phonetic reasons it will be the [t] in English (not the [p] or the [k]) and the [j] in Macedonian (not the [l]).

particular variety of the language, (provided, of course, that word boundaries are marked by some other phonemic material). However, Lunt goes on to cite forms which show quite clearly that stress is phonemic, although he thinks it is not. He discusses proclitics and enclitics, which are grouped with another word in an accentual whole, and uses a horizontal link (here replaced for convenience by a short dash) to indicate 'that words which are written separately are pronounced as one accentual unit' (22). Proclitics used with a verb are unstressed: *Sáka da-ce-vénča* 'He wants to get married' (23). When the proclitics include a negative particle, the stress is on the antepenult of the accentual unit, unless the verb is monosyllabic: *Ne-ke-cé-venča* 'He won't get married', *Táa ne-mu-gó-dala* 'She didn't give it to him', *Tój ne-mu-gó-dal* 'He didn't give it to him' (23). A contrast is also cited between *ód-drvo* 'from a tree', where *od* has a spatial meaning, and *od-dřvo* 'of wood', where *od* has a non-spatial meaning (24). A noun may form an accentual unit with a preceding adjective, but the stress never occurs earlier than the definite article on the adjective, thus *novatá-kučka* 'the new house' with antepenult stress, but *beliót-zid* 'the white wall' with penult stress. It seems reasonable to assume that Lunt's accentual unit involves close juncture and might well be termed a phonological word. The phonological word includes one stress and may equate with a written word or may include more than one written word. Lunt gives no phonological criteria for setting up the written word as a phonological unit, or for distinguishing between the written word and the accentual unit. He does set up the accentual unit, and must base his analysis of stress on this unit. On this basis it is necessary to say that stress is phonemic for the accentual unit or phonological word. This is in accord with the variations in position of stress found in the written words discussed above. Because of the high frequency of antepenult stress, stress carries a low functional load, but it must nevertheless be considered phonemic. Contrasts in stress between items larger than the phonological word are not discussed at all. And no mention is made of intonation or junctures.

There is a short section entitled Vocalic and Consonantal Alternations (14-8). It might have been helpful to subdivide this section into phonologically determined morphophonemic alternations and others, and so obviate the necessity of repeating the automatic alternations in the morphology.

In the section on morphology, forms are first divided into inflected and uninflected, and the former into verbs and nouns. Nouns are divided into substantives, adjectives, and pronouns; the last are further subdivided into personal, demonstrative, interrogative, relative, and indefinite. The uninflected class is divided into adverbs, conjunctions, prepositions, and particles. It is not always clear whether the categories are determined grammatically or semantically; thus (66), 'The Macedonian verb is set off from the other parts of speech both in form and meaning. The noun presents something as an entity (or ens), while the verb presents something as an event.' The substantive is inflected for nominative singular, vocative singular, dependent singular, and plural, although only a limited number of substantives distinguish the vocative and the dependent form from the nominative. Adjectives are inflected for masculine singular, feminine singular, neuter singular, and plural; the demonstrative pronoun is

inflected for the same categories. The first- and second-person pronouns are inflected for two cases, dependent or accusative and indirect or dative; the other pronouns are inflected for both gender and case. We are told (26) that 'pronouns are distinguished from adjectives in that they may not be modified by an adverb'. In short, verbs and nouns are distinguished by morphology and semantics, nouns are subdivided into substantives and adjective-pronouns by morphology, and then adjectives are distinguished from pronouns by syntax. This creates a couple of difficulties. First Lunt is forced to set up what he calls pronominal adjectives, treated under pronouns, and about which he says (36) that they 'function in every way like adjectives (except that they cannot be modified by an adverb)'; yet we have been given this criterion as the only distinguishing mark of a pronoun. Further, referring to interrogative pronouns, we read (44) that 'the forms *koj koja koe koi* "which one(s)?" may function as adjectives "which?"', where the term 'adjective' seems to be based on some new criterion, probably attribution, and in any case does not mean what it did in the quotation from page 26. Since the criterion of modification by an adverb is not satisfactory and since it is fairly easy to use a morphological criterion, it seems preferable to analyze as follows: forms which are inflected only for gender and for plural (Lunt's adjective, pronominal adjective, and demonstrative pronoun) are called adjectives, whereas forms inflected for case, whether inflected for gender or not (Lunt's other pronoun forms), are called pronouns. In this way all forms except subdivisions of the indeclinables are defined by morphological criteria.

The verb is quite complex from the point of view of establishing the morphemes involved. It includes a present, an imperfect, an aorist, and an imperative, besides verbal adjectives and adverbs. These are treated as composed of stem plus suffix. At the beginning of the discussion of the verb, the stem is segmented into stem plus stem vowel (68), but in the subsequent discussion stem plus stem vowel is treated as a unit. This analysis results in the 3 sg. pres., the 3 sg. aorist, and the 2 sg. aorist all being formed by the addition of a zero morpheme.² The necessity or usefulness of a zero morpheme is not made clear. The morphology of verb aspect is discussed rather briefly (69-72) but quite clearly, with examples of the different types of relationships between the two aspects.

In the section on morphology, all examples are cited in the conventional spelling of Macedonian. Most of the time this causes the reader no difficulty, but sometimes it is not clear whether Lunt's remarks refer to the phonemic forms or to the writing system. In one place (30) we read 'note that *-j-* before the suffix is lost in the plural if preceded by a vowel', with the example *odaja* 'room' *odai*. This statement could refer to the phonemic forms /*odaja*, *odai*/ or to the spellings. In another place (33) we read, 'nouns in *-o* or *-i* add *-a*', with the example *Gorgi*, *Gorgija* 'George'. This cannot refer to the spelling, but can be a phonemic statement if we assume the phonemics /*g'org'i*, *g'org'ia*/. Further (73) we read, 'if *-e* follows *i*, *o* or *u*, a *-j-* is inserted'; the examples are

² George L. Trager, Russian declensional morphemes, *Lg.* 29.326-38 (1953), presents a very highly segmented analysis of a system that is even more complex than the Macedonian verb; but although he uses many zero allomorphs, he uses no zero morphemes.

pi-j-at 'they drink', *bro-j-at* 'they count'. (In the first place this should read 'if -a follows'.) This cannot be a phonemic statement, for if *Gorǵija* is /g'org'ia/, then *pi-j-at* is /piat/ and the -j- is not phonemic, although the -j- in *bro-j-at* is phonemic. These inconsistencies are caused by the fact noted above, that Lunt did not decide on any definite phonemicization for [j]. Probably the necessary morphological statements can be stated most simply if we adopt the second phonemicization suggested above, i.e. if we consider [j] a phoneme wherever it occurs, even if there is no contrast.

There is a fairly lengthy section on the syntax and semantics of the preposition (52-65). The conditions under which the preposition is stressed or not stressed are discussed in great detail. Many of these examples show a non-antepenult stress pattern, e.g. *vo-Skópje* 'in Skopje' (55). The various uses of the prepositions are illustrated with many examples.

The syntax and semantics of verbs and verb phrases are also discussed in great detail (81-104). The present, aorist, and imperfect are called direct forms, as opposed to phrases with *l*-participles, which are called distanced forms. The distanced forms indicate 'an action viewed as distanced in time or reality' (91). Lunt cites many examples of the contrast between direct and distanced forms, e.g. *Toj beše vo Skopje—odnosno bil, ne go vidov* (93), where the speaker says 'He was in Skopje' and then corrects the direct form *beše* to a distanced *bil*, in order to disclaim responsibility for the accuracy of the statement, adding 'I didn't see him'.

The reading selections in Part II are largely contemporary; but some earlier writings have been edited to bring them into agreement with the modern language. There is enough material here to give the student ample practice with the written language.

The vocabulary in Part III is made up essentially of words in the reading selections and those discussed in the grammar, but includes also some words collected for other purposes, which should increase its general usefulness. There are a few omissions from the section on verb syntax, e.g. *šeguba* (83) and *igla* (85). The form *odnosno* 'that is' (93) is not glossed; it might be difficult for a student to guess at its meaning from *odnos* 'attitude, relation' or from *odnese* 'take out'. The form *glas* is used meaning 'news' (95) but is glossed only as 'voice'. A brief check of the reading selections indicates that they are well glossed.

La variedad dialectal del Alto Aller. By LORENZO RODRÍGUEZ-CASTELLANO. Pp. 352, with 3 plates. Oviedo: Instituto de Estudios Asturianos, 1952.

Reviewed by YAKOV MALKIEL, *University of California*

Rodríguez-Castellano's new book, dedicated to his teachers Menéndez Pidal and Navarro and preceded by a substantial Prologue by the former, represents the first large-scale study in south-central Asturian. The three localities under study, Felechosa, Casomera, and Villar, are but a short distance from the border of León, a province with whose population their inhabitants, in the past, used to maintain close contacts. The Upper Aller Valley (to which Casomera and

Villar belong) and the near-by Felechosa Valley are situated east of Pola de Lena, whose dialect Menéndez Pidal explored in his youth, only thirteen years after the publication of Å. W. Munthe's pioneering book; their speech shows a few deviations, especially as regards metaphony, from the norm observed around the turn of the century. It clashes even more sharply with the dialects spoken farther east (Sobrescobio, Campo de Caso) with respect to the products of Lat. L- and -LL-. Further contrasts exist to the usage of Oviedo (north-central Asturian, commonly accepted as the standard in local dialect literature),¹ whereas to the south the equally dissimilar Leonese proper is found. The area selected for this dialect study is thus neatly delimited in terms of linguistic peculiarities. In addition to a felicitous choice of the territory, Rodríguez-Castellano has brought to his task, as special assets, a twenty-year experience of sustained research in the field, first as a member of Navarro's *Atlas* team, more recently on his own;² freedom from pressures frequently attendant upon thesis work, which in Spain normally embodies the results of serious dialect study; and steady residence in Oviedo, at a distance of 55 km. from the chosen area, a circumstance which has presumably allowed him to revisit, in leisurely fashion, a few of the localities surveyed.

After an informative Introduction (7-31), a large part of the book is taken up by a historical phonology (43-116) and morphology (117-87), the assumptions, techniques, materials, and conclusions of which we shall examine in detail. This first nucleus of the study is preceded by a bibliography, considerably less extensive than Casado Lobato's and not free from minor inaccuracies (36-40); it is followed by useful, if scarce, samples of folksongs and folktales (189-96), arranged in two parallel columns, so as to contrast standard Asturian spelling with a narrow phonetic transcription. There follows the second nucleus, a long section devoted to the lexicon, broken up into a series of chapters (197-310) corresponding to areas of human existence (kinship, sickness), human endeavor (slaughtering, kneading), and environment (weather phenomena, animals and plants). A surprising number of entries have been segregated as semantically unclassifiable (311-6), which seems to show that the method employed was not elaborate enough. Toponyms have, with greater justification, been accorded separate space (317-22). Numerous skillfully executed and effectively reproduced sketches of tools and containers (but, unfortunately, not of plants) accompany the Vocabulary, much as in the parallel study of María Josefa Canellada and in earlier German and Swiss monographs of the Wörter-

¹ The author occasionally operates with the terms 'general Asturian' (51) and 'common Asturian' (54, 57, 59, 89, and 303), e.g. in contrasting *reb(u)elgos* 'tickling' with Fel. *roelgos*, Vill. *regüelbos*, from [*]REUELLICARE. An explicit definition of these two terms might have been in order.

² Rodríguez-Castellano is the co-author of important articles on the frontiers of Andalusian (1933) and on the aspiration of *h* in southern and western peninsular dialects (1936). After the Civil War, most of his research, including an unpublished doctoral dissertation, was centered about Asturias; in 1948 he reverted temporarily to Andalusian in his notes on the speech of Cabra. A by-product of his research in south-central Asturian (El sonido *š* < L-, -LL- del dialecto asturiano) is his contribution to the *Estudios dedicados a Menéndez Pidal* 4.201-38 (1953).

und-Sachen school. A few plates have been reserved for photographs, which, in conjunction with the maps and indexes, make for quicker and safer orientation.

1. Background. In his Prologue (1-4) Menéndez Pidal singles out the rhotacism of syllable-final *s*, the metaphony of stressed vowels, and the loss of *-b-* as the three most conspicuous features of the dialect. He insists on the late date of the spread, from the focus around Oviedo, of *-es* < *-as*; on the coincidence of the modern borderline between *f-* and *h-* with the Asturo-Cantabrian frontier,³ at the dawn of history; and on the prehistoric Illyrian strain, as shown by topon. *Carabanzo*.

The Introduction contains miscellaneous bits of information. The author characterizes his book as the first in a series of Asturian monographs (a promise which may justify the length of this review); briefly assesses his predecessors' researches, few and far between, down to the *Atlas* work interrupted by the Civil War; reports on the original scope of his inquiry (all of south-central Asturias); defends his decision, at a more advanced stage, to reduce it to a subdialect spoken in three contiguous, geographically sheltered, hence culturally conservative localities; and describes Felechosa and Casomera as two conspicuously large, economically self-sufficient rural communities in a province otherwise studded with tiny hamlets. There follows what seems a meticulous description of the geographic and economic background (extensive cattle raising, severely limited agriculture, manufacture of wooden shoes; in the past also forestry), with side-glances at parochial and municipal centers (11-3)—one misses a short reference to schools—and at communication facilities, including the long-trodden mountain paths, two recently constructed highways, and a railroad, 5 km. from the area surveyed, which links Asturias to León. The nearest towns are Cabañaquinta (15 km.), which attracts farmers by its weekly livestock markets and incipient mining industry; Moreda (30 km.), with the nearest library; and Oviedo, approximately twice as far away, the ancient and linguistically still quite influential capital of Asturias. Two facts contradict widespread popular beliefs: the Cantabrian Cordillera separating Asturias from León has at no time caused or accelerated dialect cleavage, on account of the similar living habits and resulting contacts of the mountaineers and shepherds on both sides of it (note W. D. Elcock's parallel discovery in regard to the Pyrenees, 1938), whereas the railroad construction has diminished rather than increased the range of these contacts, since the operation of freight trains makes it less imperative for farmers to attend distant fairs with any regularity.

Rodríguez-Castellano succinctly describes the day-to-day life of the three communities. Speaking of housing, he finds Felechosa least advanced and Casomera hybrid in its free combination of indigenous and imported elements. He observes (20) the disappearance of traditional garments, and points out the astounding degree of literacy and active reading habits, especially among women—in contrast to the representative West Leonese communities portrayed by M. C. Casado Lobato (see *Lg.* 25.291-307). The population is characterized as 90% bilingual (21, 30). In view of these unequivocal statements it is in-

³ Near the Tarna Pass, to the east of the Upper Aller, as assumed by Sánchez Albornoz in contrast to Bosch Gimpera.

explicable that so little attention has been paid, in the technical sections of the book, to the interpenetration of the local dialect and (substandard) Castilian, a mixture which is but another manifestation of the same all-pervasive cultural pattern.

After a meager historical chapter (22-8), with perhaps too much speculation on the remote past and inadequate data on periods accessible to the observer, the author, by way of anticipation, proffers a tentative classification of the chosen subdialect. In three respects it is said to resemble western Asturian: *L-*, *-LL-* > [tʃ]; *-as* is preserved in verbs and nouns; word-final *-r* is followed by a faintly audible [e]. In three traits the subdialect follows the lead of north-central Asturias: in the absence of descending diphthongs, in metaphony, and in the diphthongization of *e* and *o* in excess of the Castilian norm. Still other criteria (*CL-*, *FL-*, *PL-* > [j]; high pitch accompanying the pronunciation of lengthened stressed vowels) prompt the author to speak of an autonomous dialect islet (29). Not all of these arguments carry equal weight. In the perspective of evolution the contrast between *PL-* > [λ] and *PL-* > [j] is slight and fluid. Among the features that the subdialect shares with the western fringe of Asturias, at least two out of three are archaisms, whereas those that it has in common with the environs of Oviedo are, relatively, innovations. In redefining the position of Upper Aller speech in terms of diffusion, we may argue that, as a result of its distance (by local standards) from Oviedo, the focal point of most innovations, it has maintained some older traits (not nearly so many as the Brañas territory, still more distant and favored by its Galician hinterland), yet has eventually been reached by a few waves radiating from the north, and has independently developed an occasional minor feature of its own.

Rodríguez-Castellano gives a satisfactory account (31-5) of the methods employed in field work, and individually characterizes each of his informants, but some questions remain unanswered. What is the author's own colloquial skill in the subdialect? Since he started long ago on his project, what is the date of most entries? (Note that the years 1935 and 1952, say, correspond to different political and economic and consequently also linguistic situations in Asturias, as in the rest of Spain.) Individual utterances, as a rule, are localized with considerable care, but rarely credited to specific informants, although there are as many as 9 from Felechosa alone; could not pertinent symbols have been used (e.g. Fel.⁷, Fel.⁸)? What about the matrimonial ties of the informants and of the population in general?—are we to understand that the inhabitants of the three localities intermarry? If not, what is the background of newcomers to this area?

2. Record. Rodríguez-Castellano's book is written in a terse style and remains singularly free from those lyrico-didactic overtones which mar so much of present-day research in Spain.⁴ On a single occasion, his tone rises to a higher pitch: when he complains that the poor equipment of the local printing shop has all but destroyed the original accuracy of his elaborate phonetic transcription (35). No reader will withhold his sympathy from a fellow-scholar striving for increased precision and handicapped by adverse circumstances at the last decisive moment. Nor will he seriously mind such sporadic deficiencies as the

⁴ The remarkably candid comment is by E. Alarcos Llorach.

absence of short horizontal bars from [b], [d], [g] where these represent spirants, the confusion of accent marks,⁵ or the esthetically unsatisfactory use, in the same words, of types of different size and design. It is not these or similar flaws that make the author's transcription subject to criticism. The real difficulty lies in the unwarranted use of three and even four rival modes of recording the same utterances and, concurrently, in the failure to distinguish between relevant and irrelevant variants—an attitude which goes hand in hand with the disregard of the structural approach.

Throughout the bulky Vocabulary, the author uses boldface to record the entries in easily legible standard Asturian spelling, reminiscent of Portuguese and, especially, of Galician orthography, with *x* for [ʃ] and *ts* for the corresponding retroflex cluster representing the major departures from Castilian practice. In parentheses, he lists the same words in narrow phonetic transcription, with diacritic marks such as subscript dots (single or double), hooks, and horizontal or vertical semicircles, besides medial, superscript, and diagonal bars, with additional devices like inverted and raised vowels and the use of two auxiliary letters: [ø] and [ŋ]. Such a heavy apparatus is assumed to convey the most accurate idea of nuances, regardless of their relevancy. This intricate system is quite properly based almost in its entirety on T. Navarro's manual of Spanish phonetics (1918, with many subsequent revisions); it was launched at a moment when narrow transcription was riding the crest of its vogue, with Finland probably leading other nations in the degree of sophistication. To Hispanists concerned with dialectology, it is familiar from A. M. Espinosa Jr.'s Madrid dissertation (1935); significantly, it was abandoned as not really essential by Navarro himself in his monograph on the Puerto Rico dialect (1948), and fails to reappear in L. Flórez's recent inquiry into the speech of Bogotá, inspired by Navarro's new trend of teaching. We are dealing here with the infrequent and regrettable case of a director of research outgrowing his earlier thinking, to which a loyal student, long separated from him, unaccountably continues to adhere.

In the long section on historical grammar, Rodríguez-Castellano has recourse to a third notation, the usefulness and inner logic of which it is difficult to appreciate. He arrived at this compromise between the two other transcriptions by omitting [ø] and [ŋ] in favor of traditional *c*, *z*, and *n* (he also owes *ñ* to the standard spelling), while retaining *j* and *w*, also *š* [ʃ] and *š* [ʃs]; he makes no distinction between stops and corresponding spirants, and inconsistently replaces *gue-* by *ge-* and *-que-* by *-ke-* while wavering between *kw*, *cw*, and *cu*: *kwárkwa* (62), *piskwizu* (69), *cwertera* and *cwerre* (54) beside *cuidar* (57); and also between *ci* and *zi* (115). Accent marks are here used unpredictably, cf. *amanez*, *meresz*, *parez* beside *conóz* (68). In some chapters, the attempt is made to bring out the shades of vowels rather sharply, and then, one by one, the elements of the complicated (second) transcription are readmitted, culminating in the use of [ø] with a small superscript [u] and comparable typographic monstrosities; no commensurate efforts are made to refine the notation of consonants. Finally, the appended

⁵ One is led to believe that the grave and circumflex accents which appear intermittently on vowels are outright misprints (96, 222, 244).

index of words seemingly reverts to standard Asturian spelling (the first transcription, except for the use of lightface roman), but occasionally, I suppose for the sake of emphasis, retains a lone diacritic mark or adds a brief phonetic comment in parentheses. Small wonder, given this wealth of rival transcriptions, that numerous words appear in three and not a few in four different garbs.⁶ Only the first two notations, the standard (near-phonemic and easy on the eye) and the elaborately phonetic, deserve to be adopted in future volumes of the planned series.

The confusion is aggravated by the author's decision (no doubt dictated by an excess of scrupulousness) to record faithfully the minutest details of acoustic impressions, as is quite appropriately done on the maps of a linguistic atlas (where each utterance is credited either to an identifiable or to an anonymous but neatly characterized speaker), but surely not on the same scale in historical dialect monographs. Entirely unretouched transcripts of the speech of individual informants may conveniently be collected in a supplement as a handy frame of reference and a check on the analyst's arrangements. But a certain amount of sorting is advisable in grammar and vocabulary, to discard the immaterial variants. Where the worker recognizes a pattern of distribution or suspects a pattern of change, greater caution is necessary and the recorded variants may be retained and at the same time made significant by properly crediting each to the respective speaker as the representative of a specific generation, locality, level of education, or the like. In such instances, our author, though faithful in his reports and generous in his documentation, is not sufficiently explicit and discriminating in his labeling. Take the final *-u* which, in hundreds of cases,

* Examples of words recorded in three different fashions (the spelling here chosen is the standard Asturian, familiar from dialect literature): *abeya* < APICULA (48, 277), *atsindiar* 'pastorear', from LÍMITARE (112, 285), *ceya*, from CILIUM (104, 217), *cetsero* < CELLARIU (98, 246), *circu* 'halo de la luna' < CIRCU (57, 198, 329 a), *cuchetsu* < CULTELLU (101, 250), *cueyu* < COĀGULU (105, 294), *dial* < DIGITALE (77, 240), *enrozar* 'calentar el horno', from RUSSEU (100, 233), *enzame* < EXĀMEN (103, 278), *escuchar* < A(U)SCULTARE (101, 309), *esquenu* < SCAMNU (101, 250), *exe* < AXE (103, 266), *grenu* < GRĀNU (56, 257), *mataor* 'matarife' (71, 231, 334 b), *muncho* < MULTU (101, 112, 315 a), *muriu* < MŪRU (70, 247), *peezu* < PIT(T)ACIU (56, 229), *poyal*, from PODIUM (105, 248), *puzar* < PULSARE (106, 267), *riegu* 'surco', from RIGARE (57, 257, 338 b), *sestiar* 'sestear (el ganado)', from SEXTA (HORA) (71, 287), *suañar* < SOMNIARE, *tezer* < TEXERE (103, 244), *tsabrar* < LABŌRARE (86, 258), *tsugar* < LOCĀLE (110-1, 310), *uña* < UNGULA (65, 285, 341 c), *yena* < PLĒNA (48-9, 96, 203). Examples of four rival spellings: *enziva* < GINGĪUA (84, 217, 331 b), *ergabetsar* 'separar la mazorca de la caña', from *GABELLA (84, 94, 256), *gatsina* < GALLĪNA (65, 98, 288, 333 b), *isi* < IPSE (58, 135, 314 b), *isti* < ISTE (58, 135, 314, 334 a, aside from the authentic variant *esti*), *tezeor*, from TEXERE (91, 103, 244), *tseche* < LAC(TE) (58, 101, 297, 340 c). Further instances could be quoted if one were to disregard the contrast *-o* ~ *-u* in positions in which these vowels seem to alternate freely, e.g. *silgueru* < *SĒRICĀRIU (86, 206, 339 a). The confusion has been increased by a use of *b* and *v* which I fail to understand. Thus, *UESPA* 'wasp' appears as *abiespre* in the grammar and as *aviespre* in the index (112, 327 b); I find the same contradiction in the case of *valir* < UALĒRE (68, 140, 341 a) and of *venir* < VENĪRE (68, 341 b), where one may think of different contexts, an excuse which hardly applies to the product of GLOBELLU (110, 330 c). The Latin etymon or the Castilian cognate may have determined the choice of the letter in the following cases: *barbechos* (101, where Spanish orthography has not been readjusted to the spelling of UERUĀCTA), *Bernaldo* (113), *verdā* < UERITĀTE (109), *virta* < UIRILIA (104), *vida* < UIDUA (91).

capriciously alternates with *-o*: *meyu*, *meyo*, *méyu* (104, 261) 'mazo de madera' < MALLEU; *freno*, *frenu*, *frénu* (103, 211) 'fresno' < FRAXINU, to say nothing of the recurrence of [q] with superscript *u*. In the not too distant subdialect of Cabranes, it was possible, on the basis of M. J. Canellada's material (*Lg.* 23.63 [1947]),⁷ to distinguish clearly between *-o* < Lat. *-ō* (*cuero* < CURRŌ, *como* < QUŌ MODŌ, *cuando* < QUANDŌ; similarly *pero* < PER HOC) and *-u* < Lat. *-ū* (*ibiernu* < HĪBERNU, *escuru* < OBSCURU, *puestu* < POSITU). In the Alto Aller zone, one is frequently tempted to posit a similar archaic pattern, if not for the present, at least for the recent past: witness the characteristic verb forms *fago*, *oyo* (69); the adverbs, rooted in the ablative of Latin substantives, *agano* 'hogaño' (56, 100, 311) < HŌC ANNŌ, modified in its initial segment by *agora*, var. *abora* (115, 311) < HĀC HŌRĀ, and *antano* 'el año pasado' (ibid.) < ANT(E)ANNŌ (correct in both instances the author's inaccurately spelled base); and *-os*, which predominates as clearly in the plurals of nouns as does *-u* in the corresponding singulars. Yet the contradictions unaccounted for in Rodríguez-Castellano's record of masculine singulars are so sweeping that one is led to assume the incipient disappearance of this age-old distinction. Few things are more rewarding in dialectology than the opportunity to observe at close range the gradual breaking down of a contrast through leveling forces; yet nowhere is sharp formulation more requisite in the permanent record of the process. The author, steering a middle course between deliberate interpretative selection, leading to simplification, and the less arbitrary listing of all details involved, which would allow others to engage in independent reconstruction, whets the reader's curiosity, but leaves it unsatisfied.⁸

One peculiarity of Rodríguez-Castellano's organization of dialect data is the subdivision of the Vocabulary into a number of semantically delimited chapters, each separately arranged in alphabetical order. This method, in preference to the traditional straight alphabetical word list, has lately been advocated by W. von Wartburg; the theory was put into practice in several prewar dissertations written under his guidance in Leipzig and in Chicago. This plan offers advantages for systematic studies in lexicology by bringing together words which, through associative interference, may actually have altered each other's course. In the present case, the reader's only regret is that the carefully defined entries should not have been illustrated, either with coined model sentences, as in Alcalá Venceslada's vocabulary, or at least, wherever feasible, with excerpts from reliably recorded folkloristic texts. For casual consultation this dispersal

⁷ The analysis was endorsed by W. von Wartburg, *Die Ausgliederung der romanischen Sprachräume* 14 (Bern, 1950).

⁸ The lack of normalization in the spelling of words that end in a back vowel is conspicuous in the treatment of *celseru* < CELLĀRIU (98, 246), *cogutsu* 'corazón de la mazorca' < CUCULLU (91, 256), *cozu* < COXU (103, 220), *diu* < DIGITU (90-1, 217, 330 c), *fa(b)ucu* 'fruto del haya', from FĀGUS -I (90, 211), *meligru* < UMBĪLĪCU, *IM(B)ILĪCU (113, 218), and *orru* < HORREU (55, 243, 248). The same type of wavering (fortunately on a much smaller scale) permeates the transcription of words, including substantives and adverbs, that end in a front vowel: *de belde* < DĒ + BĀTIL (58, 312 a), *nuiche* < NOCTE (58, 335 b), *terde* < TARDE (58, 168, 305), *tsueñe* < LONGĒ (52, 316).

of entries carries with it certain drawbacks, sharply accented by the even more radical scattering of dialect words across the extensive phonological and morphological sections. To compensate for the resulting difficulties, Rodríguez-Castellano might have equipped his monograph with an all-inclusive, neatly alphabetized master list of all linguistic forms quoted from cover to cover. Unfortunately, the index of words (325-42) falls short of this goal in several respects, far beyond the author's own apologetic admission (325). A few words are misspelled;⁹ there are traces of confusion between homophones and even near-homophones;¹⁰ some entries are so poorly alphabetized as to be practically hidden away;¹¹ page references are not infrequently inaccurate;¹² several significant variants (and, worse, derivatives) have not been deemed worthy of inclusion;¹³ doubts have been allowed to arise as to the correct infinitive forms, by no means obvious in Asturian.¹⁴

On the other hand, the index contains uncalled-for comments, sometimes

⁹ Change *itsi* (334 a) to *itsi* < ILLIC (78, 92, 166, 314); replace *gáticas* (333 b) by the inseparable group *a gáticas* (70); note the discrepancy between the two forms of the topon. *Cot-salcón* (64) and *Cotsá del Can* (329 c); correct *palanquiera* (336 a) to *palanquiaera* (75, 234), comparable, in point of derivation, to the Castilian words in *-eadera*. The author's own list of misprints (351) is far from exhaustive.

¹⁰ Thus, the Index fails to distinguish between *caná* and *cana* (328 b) and between *fueya* < FOUEA and *fueya* < FOLIA (333 a).

¹¹ For instance, *belisu* follows upon *benu* and *bencu*, in this order (327 c); *farpayar* appears before *fardela* (332 b), possibly because the original draft recorded the rival form *falpayar*; *reonda* precedes *reñer* (338 b); *uveyas* (335 c) is squeezed in, as a separate entry, between *oeyas* and *oeyero*. Examples of subsuming under a single head (on etymological grounds): s.v. *afalar* (326 a) one finds *falar*; conversely, *ajuera* must be sought s.v. *juera* (334 a); no distinction is made between *bonica* and *bunica* (327 c); *orpinar* and *urpinar* 'to drizzle' are listed jointly (335 c); *perdicar*, *pedricar*, and *pidricar*, showing noteworthy variations of PRAEDICĀRE (transmitted everywhere through learned channels, see REW² §6718), have been lumped together (336 b); *rerger* and *rielgar* < RESECĀRE, important for the reconstruction of the history of *resgar*, *rasgar*, *rasgo*, and congeners, are identifiable only through their variant *relgar* (338 a); *robeca* follows upon *rod-* (338 b), being grouped with more advanced *roeca* (but, counter to earlier practice, not subordinated to it); *tsuntru* 'otter', worthy of attention on account of its relative proximity to LUTRA (contaminated with *ἔνυδρος*), can be located only by reference to the less remarkable variant *tsundru* (341 a); the dimin. *xatín* 'young calf' must be looked up under *xetu* (341 c).

¹² *Abeya*: 48, not 47; *fa(b)ucu*: 90, not 91; *arria*: 312, not 310; *char*: 84, not 87; *oeyero*: 287, not 286; *azruque*: 228, not 288.

¹³ *Coorniz* (the most advanced of several products) < COTURNICE (78); *tsuitín*, presumably from LUPUS (69); *güeyu* < OCULU (53; only *güiyu*, which shows metaphony, is listed); *pechu* < PECTUS (58; *pichu*, again with metaphony, is recorded); *curtiyín*, dimin. of *curteyu* (65); *om* < HOMŌ (107).

¹⁴ Remember the pervasive wavering between *-er* and *-ir* in Spanish dialects (*cerner* ~ *cernir* within Castilian; Ast. *reñer* ~ Cast. *reñir*); also, the typically Asturian maintenance of the ascending diphthong in pretonic syllable, throughout the verbal paradigm. Some doubt remains as to the infinitives and analogously accented finite forms correlated to *enseña* 'enseña' (48), *priende* 'prende' (54), *ditinto* 'detenido' (63), *escuelgo* 'descuelgo' (84), *cuérdome* 'me acuerdo' (113), *tice* 'atice', from *ATTITIĀRE (ibid.), *albierto* 'advuerto' (115), *apiertan* 'aprietan', from Late Lat. APPECTORĀRE (111), *arretsuz* 'reluce' (112), *frundo* 'hundo' (113).

too briefly phrased to be intelligible;¹⁵ but the most serious single defect is the large proportion of omissions: repeatedly, one or two page references are lacking,¹⁶ and over one hundred words discussed in the grammar have been completely skipped. Some of the most radical omissions seem haphazard and due to inadvertence,¹⁷ but others show a definite policy and hierarchy of values which your reviewer must challenge. The overwhelming majority of the words thus made inaccessible are (a) outright Castilianisms recognizable at once as intruders: *mejor*^e, *mejor*^e < MELIÖRE (63, 71);¹⁸ (b) words (which may or may not be borrowings) identical with their counterparts in standard Castilian: *botella* < BUTTICULA (94), *ermita* < ERĒMĪTA (75), *fuelle* < FONTE (80), *oso* < URSU (69), *vaca* < UACCA (67), *yema* < GEMMA (94),¹⁹ or at least in substandard Castilian:

¹⁵ Thus, the Index lists *zurnal* (*xornal*), while the passage referred to mentions *zurnal* alone (97, 342 a). What is the meaning (and the function, in a word index) of the formula *e > ie* attached to *cortadietso* (329 b)?

¹⁶ These are too numerous to be listed fully. Here is a selection, with the page references in parentheses to be added to the Index: *abuya* < *ACŪCULA (115) and *abuyero* (ibid.), *a(g)ora* < HĀC HÖRĀ (50), *aqueo* < ECCU(M) ILLU (99), *atsindiar*^e, from LĪMITĀRE (70), *baitsar* 'bailar' < ? (76), *ca(y)er* < CADERE (74), *ceya* < CILĪUM, pl. CILIA (104), *cincar* < *FIGICĀRE X FINGERE (115), *cogorniza* < CŌTURNICE, with hypercharacterization of gender (115), *culiebra* < COLUBRA, *-ŌBRA (80), *char*, from IACTĀRE (113), *degurar*^e < DĒPŪRĀRE (115), *demer* < DĒMERE (48), *ensuchu* < EXSŪCTU (101), *ergayá-se* 'desgajarse' < ? (84), *firver*^e < FERŪERE, -ERE (63), *güiyu* < OCULU (79), *Guseptin*, from IOSEPHUS (64), *itsi* < ILLIC (78), *mataor*, from *matar* < ? (71), *meligru* < *IM(B)ILICU, from UMBILICUS (113), *muil* < MŪGILE (91), *munchu* < MULTU (84), *muquero*, from *MUCCUS (64), *pa* < PER (PRŌ) AD (92), *partsar*^e < PARABOLĀRE, -ĀRI (71), *peezu* < PIT(T)ACIU (78), *puer* < POSSE, *POTĒRE (78), *reu* < RĀPU (61), *reya* < RĒGULA (51), *robeca* 'rebeco' < ? (90), *ser* < SEDĒRE (71), *sestiar*^e, from *siesta* < SEXTA (HÖRA) (71), *tener*^e < TENĒRE (57), *toa* < TŌTA (78), topon. *Toleo* < TOLĒTU (109), *tsene* (probably misprint for *tseñe*, cf. 65-6, 254, 340 c) < LIGNA (48, 104), *uñir*^e < IUNGERE (71), *vieyu* < UETULU (56-7, 208), *xenru* < GENU (80), *yena* 'crecida del río' < PLĒNA (49). The reader thus loses a guide to some of the passages in which the given words are examined from a special angle. It is regrettable also that the words discussed in the Introduction should have been excluded from the Index.

¹⁷ Among these, the following adversely affect etymological research: *artu*, *ertu* 'zarza' < AR(C)TU [?] (87-8); *banastra* < Celt. BENN- X *κάναστρον* (113); *birayas* 'sienes' < UITĀLIA [CAPITIS] (115); *Cotsá* (in toponyms), based on a derivative in -ĀTA from COLLUM -I (64); *embizcar* 'azuzar' (also, I have been told by a native, used in this sense at Valladolid), from *bizco* < *UERSICU [?] (64); *erguilar* 'resbalar' < ? (94); *frundir* 'hundir', presumably from FUNDUS -I, deflected from its course by an undetermined association (113); *penera* < PINNĀRIA (100, 235), often quoted as a relic typical of Asturo-Leonese; topon. *Pelsunu* (= *Pelúgano*) < ? (76); *quei* < QUID HABET HIC (76); *a soc-*, *sog-ustrinas* 'a hombros', conceivably related to COSTA or to SUCCUTERE or representing their blend (116); *tsagartón*, from LACERTU, *-ARTU (113); *xaña*, from SABŪCUS -I (89).

¹⁸ Other examples: *castillo* < CASTELLU (99), *lazo* < LAQUEU (86), *leer* beside autochthonous *tser* (an example interesting on cultural grounds) < LEGERE (111), *liebre* < LĒPORE (86), *silla* < SELLA (96, 99), as against the topon. *Sela* (Oviedo), *Selas* (Lugo), see J. M. Piel, *RF* 64.252 (1952).

¹⁹ This category is particularly large: *abandonar* (90), *abrir*^e (58), *acá* (76), *amanecer*^e (68), *arreglar*^e (64), *barbecho* (101), *berza* (48-9), *caballo* (96), *caldera* (48), *can* (64), *castaña* (102), *castellano* (99), *catorce* (50), *cavar* (90), *cazar*^e (106), *cereza* (48), *contento* (114), *cosa* (50), *coto* (54), *cuesta* (102), *diez* (51), *Dios* (76), *domingo* (94), *entrar* (67), *espina* (75), *matar* (90), *meter* (61), *molineru* (57), *oficio* (78), *once* (50), *oy* (53, 84), *parecer*^e (68), *patata* (88), *pelo* (59), *peor*^e (71), *pera* (77), *poca* (50), *pulga* (65), *quesu* (57), *roer*^e (78), *sudar*^e (57),

Antón < ANTŌNIU (50), *estrozar* 'destrozar', of disputed origin (84), *pidir** < PETERE (63), *rial* < Med. Lat. REALE (77),²⁰ *tar* < STĀRE (113-4), *verdā* < UĒRITĀTE (109);²¹ (c) words differentiated from their Castilian equivalents by a single major phonological or derivational trait, so that mutual intelligibility may be expected to prevail, e.g. (on the one hand) *frauta* 'flauta' < ? (93, 301), *tisoro* 'tesoro' < Gr.-Lat. THĒSAURU,²² and (on the other hand) *praino* 'pradito', from PRĀTUM -Ī (75), *sobrin* 'sobrino' < SOBRĪNU (69).²³

3. Dialect and standard. The implication is, clearly, that Asturian words identical with those found in Castilian, or suspiciously similar to them, are of subordinate interest to the linguist. This hierarchy of values, which pervades the entire book (and much of the current best output of Romance linguists), is arbitrary if the professed aim of the dialect geographer, as opposed to the dialect paleontologist, is to furnish a faithful description of present-day speech forms within a definite territorial framework. This is the avowed goal of Rodríguez-Castellano and of the majority of his fellow-workers; but the firmly established diachronic slant of Romance studies since Diez and Ascoli, and the fascination exerted by the many archaisms which give dialect speech in Europe its special flavor,²⁴ added to the traditional prestige of history in a conservative country, have again and again prompted the author to concentrate on the distinctive, picturesque features, at the expense of those traits (not necessarily

tiempo (78), *tornero* (96), *torre* (50), *trucha* (113, 119), *vega* (48), *ver* (71), *verano* (56), *viajar* (72), and *zanca* (70), all from the chapter on phonology.

²⁰ Or < RĒGĀLE? (No meaning of the word is supplied.) There are many more words that remain semantically undefined. Is *fichu* (101) used as a substantive (cf. Sp. *el hecho*) or as a participle alone? Does *tuntu*, fem. *tonia* (55) coincide in meaning with its counterpart in Spanish ('stupid') or in Portuguese ('bewildered, overwhelmed')? Does *puirtu* (52) exclusively refer to the 'mountain pass'? What tools, in addition to 'comb', does *peñe* (107) designate? Does the primary or the secondary meaning prevail in the case of *cornú* (91)? Other words semantically unidentified: *altafarra* (92), *banar* (100), *bogaero* (75), *comer** (71), *curleyu* (65), *duce* (69), *ferrero* (81), *miolsa* (73), *oso* (55), *parir*ⁱ (71), *paya* (104), *peñar** (107), *poza* (50), *señor** (71), *sotiello* (96), *tienru* (107), *xurnal* (107), *yama* (97).

²¹ *Abistar*, from UĪSU, *UĪSTU (77), *agorrecer* < ABHORRĒSCERE (115), *Estremaúra*, from *EXTREMĀRE (91), *güelo* 'flight', from *volar* < UOLĀRE (114), *salú* < SALŪTE (109), *sega-ores*, from SECĀRE, a reshaping of SECTŌRES (91), *vicino* < UIC-, *UĒC-ĪNU (110).

²² Add *ayegar* < APPLICĀRE (112), *builu*, *buelo* < AU-OLU, *-IOLU, with the apheresis probably spreading from the feminine (113), *ditinto* 'detenido', from DETINĒRE, presumably recomposed (63), *enrosqueu* 'enroscado', from *rosca* < ? (61), *ensiñe* < INSIGNAT (48), *escoa* < SCŌPA (89), *fichu* < FACTU (101), *guadña* 'gana' (verb), from Frk. *WAIDANJAN (75), *mapola* 'amapola', indirectly from PAPĀUER (113), *nun* < NŌN (64), *parria* < PER (PRŌ) AD RĪPA(M) (76), *pelu* < PĀLU (59), *pemiento* < PIGMENTU (110), *portiella*, from PORTULA (96), *preu* < PRĀTU (61), *puistu* < POS(I)TU (56), *roseriu* < ROSĀRIU (56), *sembra* < SĒMINA (93), *sotiello*, from *soto* < SALTU (96), *tuntu* < ONOM. [?] (55), *tuvia* < TŌTA UIA (64), topon. *La Tsonga* < ILLA LONGA (86), *wweya* < OVICULA (64), *vanceyu* 'vencejo' < ? (110), *xornal*, indirectly from DIURNUM (64; cf. my fn. 15), *xueves* < (DIE) IOUIS (85).

²³ Add *arresponder*, from RESPONDĒRE (112), topon. *Campa*, from CAMPU (54), *entremecer** < INTERMISCĒRE (102), *escolgar* < EX + COLLOCĀRE (84), *espinera*, from SPĪNA + ĀRIA (48), *inresistible*, from RESISTERE (112), *sombrerín* 'sombbrero', from s- (or SUB-) and UMBRA -AE (64), *tizar* < *ATTĪTIĀRE (113).

²⁴ On the different connotations of *dialect* in typical European and New World countries, see A. Martinet's forthcoming article *Dialect* in *RPh.*, Vol. 8.

trivial) which the local dialect happens to share with the standard.²⁵ Many of these coincidences may be due to diffusion; the closer the resemblance, the higher the probability of borrowing in the near past. The conspicuously important fact, insufficiently stressed, is that at this mid-century point two new phonemes are being introduced into south-central Asturian from Castilian, [χ] and [λ]. That the carriers must be bilinguals follows from the fact that so far the new phonemes have not yet spread to words exclusively Asturian. Given

²⁵ Rodríguez-Castellano acknowledges the Castilian provenience of words unmistakably stamped as intruders by phonological traits: *caballo* < CABALLU (96), *ja-jerandilla* 'a certain dance' (110, 301), *Leonor* (71, 86), *mejor**, *mijor** (63, 71-2), *ojaliá*, a petrified desiderative phrase in Arabic (70, 172), also *castillo*, *lazo*, *leer*, *liebre*, *silla* documented in my fn. 18; the preter. *líst* 'I read' is attractively explained as a blend of the native and the imported form. Occasionally he recognizes Castilianisms locally disguised: *candileza*, indirectly from CANDÉLA (105), *corteza* 'pig-sty' (105, 280) as against Sp. *cortijo* 'farmhouse, grange', also *quezá* 'quijada', from CAPSA (103; cf. *Lg.* 21.142-83). The author concedes the influence of neighboring dialects (Mieres, Pola de Lena) in the isolated cases of *tsantaina*, *-eina* < PLANTĀGINE (96-7, 215) and *chinu* < PLĒNU (57, 96-7, 312 b; the latter clashes with indigenous *yena* < PLĒNA), a situation which calls to mind well-known instances of dialectal overlapping in the treatment of the PL-cluster (cf. Sp. *choza*, *chubasco*). He even gratuitously credits to Castilian infiltration *tseñe* < LIGNA (65-6, 104, 254) and *puñu* < PUGNU (104), to account for their contrast (in reflecting -GN-) to the descendants of the MALIGNUS family (103-4), apparently unaware of the semi-learned transmission of the latter (REW³ §5266). As for *verano* < (TEMPUS) *UERĀNU, dubbed 'learned' (56), why not regard it as patterned on *aguano*, *antano*?

Yet on all too many occasions he seems to prefer almost any other explanation to the simpler assumption of borrowing. Thus, he speaks of consonant dissimilation in the case of *enziva* 'gum' < GINGIVA (84), without weighing the possibility of a cross of Sp. *encta* and Ast. **zenxi(v)a*; refuses to see diffusion in the departure of (e)char* < IAC-, *IEC-TĀRE from *zelar* < GELĀRE (ibid.); leaves unexplained *oi* < HODIĒ (53, 84), the only alternative to diffusion of a central form being the spread of the local monophthong from formulaic sequences like *hoy día*, *hoy en día*, provided their first syllable rates as unaccented; is evasive in labeling *lanzar* 'danzar' instead of speaking bluntly of a misunderstanding Castilianism; resorts to acoustic equivalence in analyzing *julenu* 'fulano', *julena* 'fulana' (ibid.), although the erratic spread of stressed *e* < *a* to the feminine, normally free from the effects of metaphony, might have cautioned him not to reckon with spontaneous native development: significantly, OGal.-Ptg. *foão*, *fuão* (E. K. Neuvonen, *BF* 12.344 [1951]) < Ar. FULĀN also yielded ground to *fulão* under Castilian pressure. Further points of disagreement along the same line: Why call *franco* 'postal stamp' a learned word (56) without so much as a hint at its channel of infiltration? Why defend, against García de Diego, the native status of *partsar** 'parlar' < PARABOLĀRE (71, 98), in reality a distinctly early Gallicism, if FĀBULĀRI is known to have replaced LOQUI in the Latinity of Spain and of Africa (cf. the 3d-century *Passio Sanctae Perpetuae*)? Wherever vacillation between rival forms cannot cogently be accounted for in terms of divergent local sound development, a plausible explanation is likely to be dialect mixture (including contamination of the receding dialect by the advancing standard). This may apply, in the Upper Aller area, to *alantre* ~ *an(i)antes* 'adelante' (112, 167); *entrambos* ~ *entramos* (100-1), expressly assigned to different generations; *mermo* ~ *mirmo* 'mismo' (94, 315 a; but Spanish for centuries tolerated *mesmo* beside *mismo*: both forms are considered early Gallicisms); *otono* and *toná* 'otoñada' alongside *toñá* 'id.' (101, 113, 287, 305). The tentatively reconstructed route of *nadie* (*HR* 15.204-30 [1945]) gives no assurance that *naide* (52) is native to Asturias. We possess as yet little information on intradialectal and interdialectal word migration in Ibero-Romance; it is from monographs of the scope of Rodríguez-Castellano's that we expect enlightenment in the immediate future.

the continuance of present trends, the chances are that the imported [x] may eventually dislodge [s], the last redoubt of which may easily become some emotionally colored, obscene, or onomatopoeic words, without counterparts in Castilian. As regards [λ], it is safer to refrain from predictions, since its status is less secure in substandard Castilian.

The earlier borrowings from the dialects of the central meseta arrived in smaller numbers and, in all likelihood, at sufficiently long intervals to be thoroughly assimilated to local speech habits, which makes their detection difficult. Rodríguez-Castellano makes no attempt to stratify the Castilianisms and gives no dates, however tentative, for the earliest traces of infiltration, but some of his material speaks for itself. The suffix gives away *candilexa* and *cortexa* (105) as intruders from the south, as they cannot possibly belong to the same layer as *ojalid* (70, 172). There remains an alternative: either [s] reflects the same phoneme in Castilian, which would push back the date of the borrowing to the early 17th century, or it represents a local adaptation of the characteristically Castilian [x], a function it still has in Galician-Portuguese,²⁶ in which case the words may have wormed their way into Asturian at any time between (say) 1650 and 1900. Where phonological criteria in isolation are inadequate, one may have recourse to indirect evidence. It is highly improbable that *patata* (88), imported from the New World, and *brusa* 'blouse' (93), recorded for the first time in French as late as 1798, should not have passed through standard Spanish on their way to Asturias; the same applies to the Hellenism [a.ima] < ASTHMA (94). Semilearned words, often of ecclesiastic flavor, pose related problems: it is unlikely that they should all have developed in mouth-to-mouth transmission down from late antiquity. These words might well have been set apart and studied against the background of church history (bishopsrics, monasteries, pilgrimage routes), with the techniques perfected by J. Jud.²⁷

4. Archaism, innovation, equidistance. In a sharply characterized Romance dialect like Asturian the only legitimate frame of reference, in diachronic analysis,

²⁶ Cf. Ptg. *lagartiza* and the like.

²⁷ Such words (conspicuously learned, if sometimes locally colored on the phonetic side) as *acelarao* 'atolondrado' < ACCELERĀTU (110), *arma* < ASTHMA (94), *contento* < CONTENTU (114), *oficio* < OFFICIU (78), *resarvaos* < RESERVĀTŌS (110) might fittingly have been set off as presumable borrowings from the standard. *Disgracia*, from GRĀTIA, in the sense of χάρισμα (63), and *ermita*, from Gk.-Lat. ERĒMĪTA (75), are formations of old standing unlikely to have descended from Latin in a straight line. *Rutya* 'rodilla' < VLat. ROTELLA (instead of ROTULA) and the corresponding verb *arruiyá-se* 'arrodillarse' (78, 91, 105, 219), vernacular but of Castilian coinage, contrast with native *rodietsa* 'hocico (del cerdo)', 'rodaja de madera' (51, 231, 297); they deserve mention here because their infiltration is probably due to the importance of kneeling in the Church ritual. *Rutya* is likely to have been preceded by a congener of Sp. (*h*)*inojo*, Ptg. *joelho* < *gēolho* 'knee'. One may entertain legitimate doubts on the rectilinear transmission of semilearned words like *cabildru* 'atrio de la iglesia' < CAPITULU (113, 298); *duce* < DULCE (69) as against truly vernacular Ptg. *doce*; *gomitaro* < UOMITĀRE (114), conceivably a term of folk medicine (cross with *garganta*, *gaznate*, cf. the parallel Italian reflexes?); *quilmerna* 'brawl' < CHIMAERA (112), as against the genuinely vernacular zoonyms found outside Spain (REW³ §1873); *treguna* < TRIBŪNA (74), endowed in the mouth-to-mouth transmission with an entirely different meaning (REW³ §8888; the most plausible series of transformations is: **triuna* > **treuna* through false regression > *treguna*); *tsé(g)rima* < LACRUMA, -IMA (59-60, 86).

is Latin, the testimony of which may be supplemented with shreds of evidence from other ancestral languages. Nevertheless, a Spanish explorer at every step feels an irresistible temptation to draw a fleeting comparison with the respective state of affairs in modern Castilian, pointing out the numerous archaic (residual) traits or the original solutions of the chosen subdialect.²⁸ Scientifically the most rigorous procedure would be to avoid such casual comment in the body of the grammar, where it implies a sudden and hazardous change of perspective, but to tabulate and possibly re-analyze, as part of the conclusion, all such features (phonemic, prosodic, inflectional, derivational, syntactic, lexical) as show a slower or quicker rate of development than the corresponding features of Castilian.²⁹ The former would in this context be defined as local archaisms, the latter as local innovations.

Archaic features of the Alto Aller subdialect include the preservation of [S]: *texer* 'tejer' < TEXERE (103, 244), *puxar* 'empujar' < PULSARE (106, 267), and of [f] in any position: *fexe* 'haz pequeño' < FASCE (102, 275), *afogá-se* 'ahogarse' < OFF-, *AFF-ŌCARE (92); the extensive maintenance of -e: *árbol* 'árbol' < ARBORE (69), *tose* 'tos' < TUSSI (68, 222); the persistence of the monophthong where called for by the norm: *sembra* 'siembra' < SĒMINAT (107, 144), of certain secondary consonant clusters: *tienru* 'tierno' < TENERU (107), of once widespread patterns of consonant dissimilation: *Bernaldo* 'Bernardo' (110), *celebro* 'cerebro' (ibid.), of traditional accentual schemes: topon. *La Fuente-l-Cuêlebre* < FONTE (IL)L(AE) COLUBRAE beside *culiebra* (80, 209) < *culuebra < *[colóbra] (REW³ §2060), also *ruciu* 'hierba que se saca al escardar, pasto verde para el ganado' < RŌSCIDU (80, 102, 276, 287), as against Sp. *rocío* 'dew'. A minor conservative trait is the resistance to hypercharacterization of gender in *cuyar* 'cuchara' (105, 250) < COCHLEĀR(E). Morphologically noteworthy are *esparder* (102, 314) < SPARGERE as against Sp. *esparcir*, and *reñer* 'to quarrel' (103, 140) < RING-I, *-ERE 'to growl, snarl' (REW³ §7325) as against Sp. *reñir*, Ptg. *renhir*, Cat. *renyir*. Lexical archaisms are particularly noteworthy and will be dealt with in a separate study; a few examples, by way of anticipation: *Antón* 'Antonio'

²⁸ Only by way of exception does Rodríguez-Castellano confuse the two perspectives: while it is unexceptionable to speak of 'diptongo anormal' in the case of *cuerre* 'corre' < CURRIT (54), because the development [ɔ] > [we] runs counter to the phonological norm, one sees no justification for the tag 'exceso de diptongación' attached to *priende* 'prende' < PRAEHENDIT (ibid.). Here, as in the cases of topon. *Tabierna* < TABERNA (51), of *preba* 'prueba' < Late Lat. PROBA (54), and of *biendo, viendo* < UENDŌ (54, 144), the Asturian variant concomitantly represents the ideal terminal point of Castilian evolution which, for a special reason (analogy, borrowing), has not been reached in the standard.

²⁹ Numerous peculiarities of Asturian, of course, call to mind Portuguese rather than Spanish: *cubiciar* 'codiciar' (90) is closer to OPtg. *cobi(i)ça* than to OSp. *co(b)diça*, from CUPIDITĀS, *-ITIA; *maurecer* 'madurar' (76-7, 211) belongs to the western zone in which MĀTŪRESCERE has persisted (the inchoative also has left traces in the paradigm of Fr. *mûrir*); *miotsa* (73), like the semantically differentiated *miola* and *moela* in Portuguese (REW³ §5463), morphologically comes closer to MEDULLA than does Sp. *meollo*; *tseche* 'leche' (masc. like Ptg. *leite* and most other Romance cognates) is less distant from the gender of LAC(TE), neuter in the standard, occasionally masculine at other levels, than Sp. *leche* (58, 297); *pa* < PATRE and *ma* < MĀTRE (92, 307) adjoin the area of Ptg. *pai, mãi*; the latter, in turn, seems to border on And. *mae*. Note also *mozá* 'almorzada' (113, 227) beside Ptg. *almoçar*.

(50; cf. OSp. *Antón de Montoro*, *fray Antón de Montesinos*), *degurar* 'desnatar' (115) < **deburar* < *DĒPŪRĀRE*; *demer* 'varear' (48) < *DĒMERE*, used exactly in this sense by Varro; *mure* 'ratón' (69, 203) < *MŪRE* (*HR* 19.238-63, 323-40 [1951]); *onde* 'donde' (50, 169) < *UNDE*.³⁰ These and comparable examples of linguistic lag can be subdivided according as the dialect relic at some earlier date was demonstrably current in Castilian (e.g. *celebro*, *cuchar*, *mur*, *onde*) or can only inferentially, with an inevitable margin of risk, be projected into preliterate Ibero-Romance (e.g. **cuélebra*, **deburar*, *demer*, *refier*).

Examples of evolution beyond the stage presently reached by Castilian seem quite as numerous, with metaphony doubtless occupying the first place: *ercu* 'arco iris' < *ARCU* (56, 199), *esquenu* 'escaño (de la cocina)' < *SCAMNU* (101, 250), *meyu* 'mazo de madera' < *MALLEU* (104, 261), *permu* < *PARAMU* (59), *pertu* 'parto' < *PARTU* (56); *güeyu* ~ *güiyu* < *OCULU* (53, 56, 218), *sulu* < *SOLU* (56). The shift of *L-* and *-LL-* to [ʃ] follows as a close second: *tsana* < *LĀNA* (86, 244), *tsingua* < *LINGUA* (86), *gatsina* < *GALLĪNA* (65, 98, 288). The impact of their joint effect is visible in *quetsu* 'callo' < *CALLU* (56), *tsueñe* beside *tsuiñe* 'lueñe, lejos' < *LONGĒ* (52, 316). The erosion of the Latin intervocalic stops has here gone farther than in standard Spanish, but is less striking (especially in regard to the dental series) to those generally familiar with Spanish dialect and careless colloquial speech.³¹ *Estremaúra*, from **EXTREMĀRE* (91), *maúra* < *MĀTERIA* (51, 91), *roera* 'cart track' beside *ruúa* < *ROTA* (91), *salú* < *SALŪTE* (109), *sea* 'seda, crin' < *SAETA* (90, 284), *verdá* < *UĒRITĀTE* (109), *viúa* < *UIDUA* (91), the suffixes *-aor* < *-ĀTĒRE* and *-aera* < *-ĀTĒRIA*, even imported *rutya* 'knee' cause less surprise than *co* 'junto a' < *CAPU* (cf. OSp. *cabo*, *cabe*), *tsu* (pl. *tsos*) 'lobo' < *LUPU*, and especially *cleu* < *CLĀ(U)U* and *reu* < *RĀPU* (89), which illustrate the rise of diphthongs new to the dialect through the combined agency of vowel metaphony and consonant decay. Minor pertinent features include the spread of the diphthong to the pretonic syllable, in verb forms: *suañar* 'soñar' < *SOMNIĀRE* (101, 225); the radical simplification of the medial cluster *-sn-* < *-xn-*: *frenu* < *FRAXINU* (103, 211; conceivably, but not necessarily, *FRĒNUM* has here yielded *frinu*, see 57); the alternative elimination, through metathesis, of the rare cluster *-rl-*: *Calros*, *calrista* (111); sporadic epenthesis of *n* before [ʃ], conceivably in imitation of *ns* ~ *s*, competing since the Roman period: *estrencho* < *STRICTU* (112; equally acceptable if not more so would be the assumption of lexical polarization *ancho* ~ *estrencho*); occasional

³⁰ A small category apart is formed by such deceptive elements of the subdialect as happen to resemble closely their distant prototypes, as a result of secondary regression: *frisco* 'fresco' (57), *nigru* (but fem. *negra*) 'negro' (57); in a sense, also *benu* 'criba para limpiar el trigo' (56, 100, 259). Their evidence should not weight the scales in any appraisal of the conservative vs. the progressive traits.

³¹ Further trivial innovations: reduction to a single syllable, after the loss of the medial consonant, in *dial* 'dedal' < *DIGITĀLE* (77, 240); epenthesis of *r* after a consonant group ending in a dental stop, in *mostro* 'mosto' < *MUSTU* (113); loss of medial *r* and merger of the surrounding vowels, in a word subject to particular wear: *pa* 'para' < *PER* (*PRĒ*) *AD* (92, 170); metathesis of *r*, attracted by an initial stop: *probe* 'pobre' < *PAUPERE* (111). Some of these shifts are as common in Vulgar Castilian as they are in Asturian and ought, therefore, to be left out of the reckoning. Their propagation to the New World may be due to the joint pressure of miscellaneous peninsular dialects.

hypercharacterization of gender: *co(g)orniza* beside *coorniz* < CŌTURNICE (78, 204), and of person: *ye* 'es' < EST (51).

In addition to relative underdevelopment and hypertrophy, one discovers numerous instances in which Asturian and Castilian are equidistant from a common Latin base. Ast. *tsonga* 'luenga' (86) recalls LONGA through its stressed monophthong, but has drifted away from the etymon through the transformation of the initial consonant (via Common Ast. *llonga*); *tsamber* 'lamer' < LAMBERE (86, 100) lends itself to similar analysis. *Tsixiga* 'lejfa' < adj. LIXIUA (74, 103, 241), viewed from Castilian, seems retarded as regards the central consonant pillar and the pretonic vowel (the latter may be attributable to regression); it is progressive in its use of [ɣ], whether antihatic or due to acoustic equivalence, and of [tʃ]. Ast. *sayar* < *sallar (105, 258) and Sp. *sachar* < SARCULARE (from SAR(R)IŌ -IRE) exhibit two distinct resolutions of the unacceptable cluster *-rll-, which temporarily came into existence through syncope around the year 1000. Ast. *esparder* (102, 314), already examined in regard to its conjugation class, and Sp. *esparcir* < OSp. *esparzir*, from SPARGERE or EX-(S)PARGERE, illustrate two independent attempts to avoid unfamiliar [rʒ] which the Portuguese alone eventually learned to pronounce (*espargir*). The mutual relation of the tripartite clusters -mbl- and -mbr- in the respective Asturian and Castilian products of FĒMINA (93, 107, 203, 281; but *fema* also occurs vestigially) and SĒMINAT (93, 107, 144) is quite similar.

The term 'equidistance' here proposed lends itself to further elaboration. It is arguable that OAst.-Leon. *cuyar* (preserved in the Alto Aller zone) and OSp. *cuchar* were equally distant from COCHLEAR(E), but that the balance was later upset by the shift to Sp. *cuchara*. In similar fashion, RĒGULA first produced [rela] in the lowest stratum of Ibero-Romance, then split into OAst.-Leon. *reya* and OSp. [reza], which at that stage were equidistant from the common base; subsequently, the former persevered (105, 268, 271), the latter advanced one step more to [reʃa]. The same analysis applies to *ceya* 'ceja' (104, 217) < CILIA, pl. of CILIUM 'lash, lid', and to *ayenu* 'ajeno' (57) < ALIĒNU. In other cases diverse patterns of differentiation prevail: thus, from FACTU the first steps lead to *feitū*, *fechu*, and only from this point does central Asturian steer toward *fichu* (101) and Castilian toward (*h*)*echo*. These random analyses of the respective distances of dialectal and standard features from their common source thus take into account the successive evolutionary stages and afford a glimpse of intermediate categories between archaism, innovation, and equidistance. If extended to several Asturo-Leonese subdialects, they might provide a much-needed basis for their reclassification.

5. Acoustic equivalence. Perhaps no single aspect of Rodríguez-Castellano's analysis is so controversial as his frequent recourse to acoustic equivalence. This term was introduced into Spanish linguistics 35 years ago with the rapidly increasing need for accurate dialect descriptions.²² Initially, fieldworkers observed that in certain words the spirantized voiced stops, also [f] and [θ], [ð] and [r],

²² A. Castro and T. Navarro, *RFE* 5.197 (1918), with references to earlier stray statements by C. Michaélis, A. Morel-Fatio, J. Jungfer, and A. M. Espinosa(-padre). In 1914 Castro, apropos of [θ] ~ [f], spoke of a spontaneous consonant shift (*RFE* 1.182).

seemed freely to alternate. Simultaneously, laboratory tests proved that, out of context, even trained linguists confused the auditive impressions of these pairs and triads of sounds: hence the label acoustic equivalence. Menéndez Pidal somewhat rashly introduced this category of linguistic wavering into historical analysis without carefully redefining the phenomenon.³³

Operating with acoustic equivalence has a measure of usefulness as long as the term refers to an incipient or tentative linguistic change. Thus, [δ] is an unstable consonant, of which the status in colloquial Spanish is no longer secure: in certain environments, the alternatives for untutored speakers seem to be either to drop it, with compensatory lengthening of the preceding vowel, or to shift it to vibrant [r], also spirant [ɹ], especially where latent associative interferences (desirable word blends, attractive derivational patterns) are apt to offer indirect advantages (Wahlgren). Whether this tendency can ultimately be checked or may in time develop into a full-scale sound shift, remains at this juncture a matter of speculation. Important in analyzing these incipient shifts is the fact that the typical change, though bidirectional, is not nearly so frequent in one direction as in the other: thus, examples of [δ] > [r], [ɹ] are likely to outnumber those of [r] > [δ] ten to one. This degree of disproportion of opposite tendencies is what linguists expect of an incipient sound shift, especially in a complex society: after a group of progressive speakers begins to reshape the existing sound pattern (say, by shifting medial [δ] to [r]) to which conservatives continue to adhere, widespread flux and inconsistency ensue, which almost unavoidably give rise to intermittent false regressions (in this case, [r] > [δ]).

Within these confines of descriptive dialectology and of historical probing centered about the recent past, acoustic equivalence neatly characterizes sound shifts in their stage of incubation. It would be inadvisable to apply the elastic label to other dichotomies of sound development difficult of classification; in particular, to any departure from the phonological norm not yet accounted for in traditional historical grammar, which operates with consummated sound shifts alone. Unfortunately, the term has been overextended by the majority of Spanish linguists,³⁴ and Rodríguez-Castellano has not mustered sufficient skill to avoid the pitfall.

In his chapter on acoustic equivalence (113-6), which to him, at least in practice, seems coterminous with linguistic error, the author adduces evidence of such vacillation between *b* ~ *d* (actually, medial [β] and [δ]);³⁵ *b* ~ *g* (including the special case of initial and medial [bw] ~ [gw], which might have warranted separate listing); *d* ~ *g* (medial [δ] ~ [ɣ]); *d* ~ *l* (syllable-final [δ] ~ [l], and

³³ *Manual de gramática histórica española* 194-201 (Madrid, 1941). The 3d ed. (1918) did not contain a pertinent chapter.

³⁴ For a realistic appraisal of the limits to this phenomenon, see A. Alonso, *Problemas de dialectología hispanoamericana*, *BDHA* 1.440-69 (1930), especially 454-5. Alonso doubts that the error originates at the auditive rather than the articulatory stage of communication; does not recognize sporadic manifestation (in contrast to the assumed regularity of other sound correspondences) as a salient trait of the phenomenon; and insists that specific chronological and geographic limits must here be taken into account as strictly as in the study of any other process of language history.

³⁵ The indispensable qualifications given in parentheses are invariably the reviewer's.

word-initial [d] ~ [l]); $d \sim n$ (medial [ð] ~ [n]); $d \sim r$ (medial [ð] ~ [r], rarely [ɹ], see §50); [d] ~ [θ] (syllable-initial); [f], [φ] ~ [χ] (syllable-initial); $g \sim b$ ([ɣ] ~ [β]; unnecessary repetition); $k \sim g$ (word-initial; also [k] ~ [ɣ] medial syllable-initial); [χ] ~ [g] (word-initial); $l \sim d$ (word-initial; also [l] ~ [ð] medial syllable-initial); $p \sim k$ (initial and medial); $m \sim n$ (medial).

It is a hazardous procedure to establish some such categories on the basis of a single example, yet the author repeatedly took this risk. He was safe in positing [β] ~ [ɣ], especially before a back-vowel: *abuya* (116) ~ *aguya* (239) 'needle' < *ACUCULA, for ACUC(U)LA, from ACUS.³⁶ The equivalence [ð] ~ [n] is a shade less certain (*trébenes* ~ *trébedes* 'tripod' [115, 252], both at Felechosa): the instability of -D- (TRIPEDĒS, see REW³ §8912.1a) makes its sporadic disappearance, entailing the insertion of doubtless analogical and at the same time antihiatic -n- a strong possibility. Has a test confirmed the inability of local inhabitants to distinguish between [n] and [ð]? The case of [d] ~ [θ] rests entirely on the assumption that *baranza* 'side pole with sockets for the stakes of a truck' is a phonological variant of *baranda* 'railing, banister', from UĀRA -AE 'trestle for spreading nets, forked pole' (REW³ §9150); even if the two words were akin (and the author admits his doubts), would the phenomenon involved not be suffix change? For illustration of the use of -ANTIA in Asturo-Leonese tool names, cf. *preganza*, -ancia beside *bregancia*, from a verb (PLICĀRE, see REW³ §6601), but apt to be associated with the local substantives for 'nail'. For [φ] ~ [χ] the lone example is *julenu* 'fulano'; since [χ] has, until quite recently, been alien to the local phonemic system, we are dealing here with an imperfectly adjusted intruder, perhaps from eastern Asturias.³⁷ To show [l] ~ [ð], the author drags in *mostadietsa* (var. *mostaldietsa*) 'weasel', from MUSTĒLA (assumed starting point: OAst. *mosteliella). He vaguely admits that consonant assimilation may as well be involved; your reviewer prefers to reckon with the dissimilation of almost homorganic [l] and [λ] prior to the local shift [λ] > [tʃ]. Either way, acoustic equivalence may safely be eliminated not only as the major cause, but also as a contributing factor.

Other categories have each been inferred from two examples. An acceptable equivalence is [β] ~ [ð], if redefined as a tendential shift of [β] to [ð] before stressed *i*, with ever-present possibilities of further extension and occasional false regression: *cadiya* < c(L)ĀUICULA 'small key, tendril, bar' (as against Sp. *clavija* 'peg') and *tudiyu* < TUBELLU 'small hump, protuberance' (as against Sp. *tobillo* 'ankle'). An abortive attempt is made to demonstrate the exchangeability of

³⁶ This example appears elsewhere (115) in the company of *abora* vs. OSp. *agora* < HĀC HŌRĀ and of the oppositely distributed Ast. *agorrecer* ~ Sp. (Ptg.) *aborrecer* < ABHORRĒRE, Low Lat. ABHORRĒSCERE and Ast. *reguzu* ~ Sp. *rebojo* < REPUDIUM. The author might have striven for higher concentration.

³⁷ On the frontier between [f] and [χ], see Rodríguez-Castellano's own earlier monograph, favorably reviewed by S. Gili Gaya, *NRFH* 1.90-1 (1947), and the parallel, almost simultaneously conducted study by A. Galmés de Fuentes and D. Catalán Menéndez-Pidal, *RDTP* 2.196-239 (1946). The latter has just published a study of Asturian metaphony, with maps (*RDTP* 9.405-15 [1953]), which opportunely supplements the corresponding chapter of Rodríguez-Castellano's book.

[p] ~ [k] by *corcusptn* for **porcusptn* 'porcupine'; why not call the change by its correct name: anticipatory consonant assimilation?³⁸ *Acosta* 'purposely', if at all a congener of W.-Ast. *aposta* 'id.', may owe its transformation to associative interference. Few will accept the formula -n- ~ -m- at its face value: Southc.-Ast. *ferrumientu* 'rusty' and W.-Ast. *ferrunientu* (= *ferruñentu*), brought forward as evidence, do not carry conviction because they lack a common starting point. The former is traceable to FERŪMEN 'soldier's glue' (Pliny 37.2.10.28), later interpreted as a derivative of FERRUM -Ī 'iron' (> FERRŪMEN) and endowed, at least in the provincial Latinity of the Castilian meseta, with the meaning 'rust' (Sp. *herrumbre*). The latter directly perpetuates FERRŪGŌ -INIS 'iron-rust', which has persisted in Galician-Portuguese and the adjacent western fringe of Asturias (REW³ §3261), with Sp. *herrín* representing an offshoot of the doubtless early *-IGŌ variant. As for *plumo* 'pruno', the example is ill chosen to begin with, inasmuch as *pl-* ~ *pr-* provides a second, more perceptible contrast, apt to destroy any illusion of acoustic equivalence; then, historically, because the alternation *m* ~ *n*, here demonstrably traceable to antiquity whatever its remote source (REW³ §6798), has no bearing on recent Spanish sound development.³⁹ The assumed alternation [d], [ð] ~ [l] rests on meager evidence: *albierto* 'advierto' points to the intrusion of the quasi-prefix *al-*, a morphological process, whereas the Castilianism *lanzar* 'danzar' is a misinterpreted borrowing. *K* ~ *g* and [k] ~ [ɣ] are supported by copious documentation; yet lexical overlapping best explains *gordón* 'cordón', from *cuerda* 'rope' < CHORDA 'string' (interference of *gordo* 'thick' < GURDU 'dolt, numskull'). Rivalry of suffixes or suffix variants accounts for *platiguera* 'talkative woman' beside Sp. *plática* 'chat' < Gr.-Lat. PRACTICA, flanked by *platicar* 'to converse', see *Lg.* 25.145-54 (1949); and for *talanguera* 'drunkenness', a semantically advanced counterpart of Sp. *talanquera* 'parapet, picket fence' (which Meyer-Lübke, in the wake of Segl, explained as a cross of Gk.-Lat. PALANCA 'lever, bar' and TABLA 'board'), either through direct regional survival of (accus.) φάλαγγα, as in Sicily and Naples, or through late association with the well-known suffixal series (probably of Germanic descent) -ango, -angue, -anco, -anque, with variations of the accented vowel. Part of Rodríguez-Castellano's claim is invalidated by spurious etymologies: topón. *Gumial* recalls Ptg. *gume* 'sharp edge' from apheresized (A)CŪMEN -INIS 'a point to prick or sting with, sharpness, pungency', and must either be separated from CULMEN -INIS 'point, top, summit, ridge' (REW³ §2376), a variant of COLUMEN -INIS, or, especially in view of Ptg. *cumiada* 'mountain ridge', be classed as a blend of (A)CŪMEN and CULMEN. Why follow García de Diego in linking *garbos* 'thin dry twigs (used as touchwood)' to Gk.-Lat. CALAMŌS 'reeds, canes'? *Gazepu* 'bowl for keeping the hone' cannot, on derivational grounds, be related

³⁸ The var. *piscusptn* shows, in addition, the familiar dissimilation of the pretonic and the tonic back vowels, as in Sp. *pescuezo* and *redondo* (*HR* 14.130-7 [1946]), and the replacement of syllable-final *s* by fricative *r*, as a reaction against the reverse tendency, which especially affects the prefixes *des-* and *es-*.

³⁹ Neatness of analysis is here particularly desirable, because a small residue of instances of *m* > *n* in Romance remains unexplained: if MEMORĀRE > OSp. *nembrar* (beside Ptg. *lembrar*) raises no problems, no satisfactory account has yet been given of MESPILU > Fr. *nèfle*; see O. Bloch and W. von Wartburg, *Dict. étym.*² s.v.

to CAPSA 'box'; what forbids us to associate it with *gazapo* 'rabbit' (animization), in a country widely regarded as the original habitat of that rodent? Of the two remaining examples, one is etymologically obscure, and the other, involving the progeny of *κάρυον* 'nut', exhibits the wavering *k* ~ *g* characteristic of Hellenisms transmitted by word of mouth, a feature irrelevant in this context, since at best it illustrates acoustic equivalence of two thousand years ago.

The residue of the free alternations, $[\beta] \sim [\gamma]$, $[\delta] \sim [\gamma]$, $[\delta] \sim [r]$, $[f] \sim [\theta]$, none of them new, seem authentic; but here too, numerous subtractions must be made before one arrives at the sound core. The initial consonant of semi-learned (originally medical?) *gomitar* < UOMITĀRE, the frequentative of UOMĠ -ERE 'to puke, spew', is not exclusively peculiar to Asturias, nor even to Ibero-Romance (for Italian counterparts, see REW³ §9452); on its transmission, cf. fn. 27. Association with GURGES 'whirlpool' (< 'gullet'), GURGULĠŌ 'windpipe, weasand', GURGUSTIUM (in its Late Latin sense), and GUTTUR 'throat, gullet' might plausibly account for the shift; note the possibility of concomitant association, in Spanish territory, with *garganta* 'throat, gullet', *gaznate* 'throttle', and *hacer gárgaras* 'to gargle', all of which, so far as represented in the Upper Aller area, one may also hold responsible for a *gorgozón* 'in bubbling, gushing fashion', as against Sp. *a borbotones*, *a borbollones*. *Pérdiga* 'loss' vs. Sp. *pérdida* (quite isolated; cf. *búsqueda* 'search'), *castaño* 'chestnut grove' vs. Sp. *castaño* (topon. *Castañeda*) < CASTANĒTU, *trajeria* (given away by [x] as a new import) vs. Sp. *tragedia* all show in varying degree the influence of powerful suffixes. On the other hand, we had better count Ast. *ventregu* beside Sp. *ventrada* (better: *ventregada*) 'litter brood' as morphologically independent formations, representing autonomous branches of a single family at its Late Latin stage: UENTER -TRIS 'belly' vs. UENTRICULUS 'stomach', UENTRIC(UL)ŌSUS, and UENTRIGĀRE 'uentrem agere'. These trimmings leave us with a small core of the words assembled by Rodríguez-Castellano as illustrations of acoustic equivalence: hardly more than a dozen formations. Let me repeat that it would be unfair to blame him alone for a sweeping extension of its legitimate scope.

6. Other problems of phonology. The author's general approach to phonology is best described as untouched by structuralism. Such an attitude brings with it a few advantages and numerous drawbacks. It explains why certain points, selected for searching analysis, are treated exceptionally well (e.g. metaphony, spirantization and disappearance of stops, the retroflex cluster [ʃʃ]), while others are barely mentioned, if at all.⁴⁰ Conspicuous, to begin with, is the lack of any

⁴⁰ There is room for disagreement with the author on the labeling of several phonological processes. How can apocope apply to the partial shift *muy* > *mi* (114, 169)? *Paréa* (Villar, Cabranes) beside *paré* 'wall' (109, 247) < PAR(I)ĒTE is termed 'strange'; actually, *parea*, much like *lexa* (with agglutination of *el* < ILLA) beside *exe* 'axle' (103, 266) < AXE (masc. in Classical Latin) and *tsantaina*, -eina beside nearby *tsantén* 'plantain' (96-7, 215) < PLANTĀGINE all illustrate hypercharacterization of gender, whereas *abiespre* 'wasp' < UESPA (65, 112) and a few other words in which -a is yielding to -e show hypocharacterization. The author's formulation of the behavior of SANCTU fits *Santo Antón* (69-70), but neither *San Miguel* 'September' nor *San Xuan* 'June' (305). On the whole, Asturians distribute *San* and *Santo*, -u as the Portuguese do its equivalents, that is, depending on whether the following name begins with a consonant or a vowel; whereas Spanish at present opposes *Santo Domingo* and *Santo Tomás* to both *San Antonio* and *San Miguel* (*Santander* < SANCTU

inventory, even on an impressionistic basis, of the sounds of the present-day dialect. Without such a preliminary tabulation, they could not, of course, have been arranged in phonemes. The author plunges abruptly into historical phonology, leaving to the reader the ungrateful task of piecing together the total sound range from the phonetic transcription of individual examples. The phonology is so slanted as to illustrate the individual deviations from Castilian evolution. Rodríguez-Castellano shows less sensitivity to their complex interplay, which alone explains such noteworthy phenomena as the reappearance of two diphthongs, *au*: *fraugua* '(blacksmith's) workshop' (62) < (ARTE) FABRICA, and *eu*: *aréu* 'plough' (271) < ARĀT(R)U, *réu* 'tail' (89, 271, 284) < RĀPU; the formation of the triphthong *ueu*: *tsueu* ~ *tsuego* 'soon' (91) < LOCŌ; the contiguity of *ue* to a following unstressed vowel: *ruéa* 'wheel' (91) < ROTA; and the rise of consonant clusters unfamiliar from Castilian, such as [θr] in *azruque* 'azúcar' (111, 228). The latter word further exemplifies the avoidance of unaccented checked final syllables, which have spread in Castilian partly through the acceptance of Latinisms, partly under the pressure of Arabic loan-words (RPh. 6.62 [1952-3]); cf. *cadavre* 'cadáver' (111). More might have been said on the tendency of speakers to disengage *r* from medial clusters and to ally it with initial consonants, as in *probe* 'pobre', and on the formation of new clusters through syncope of the pretonic vowel. A case in point would be *brendal* (Cas.), *brandal* (Fel.) 'cajón donde cae la harina' (293), if related to MERENDA, cf. Gasc. *brene*, Cat. *berena* (REW³ §5521)—the semantic shift is self-explanatory.⁴¹ Other problems worth exploring are the abandonment of primary -LS-: PULSĀRE > *puzar* beside *ponxa* 'chaff, winnowings, graindust' (106, 257, 267)⁴² and the subsequent reintroduction of the group in *yelso* 'chalk' < GYPSU (108), reminiscent of Gal. *gelso* as against Cat. *guix*, Ptg. *gesso* (and imported *giz*), Cast. *yeso*;⁴³

EMETERIU has preserved an intermediate form, obsolete outside toponymy). In discussing *armanu* < GERMĀNU (64, 84-5), *acelarau*, *penarar*, *resarvaos*, *Sarafina*, and *Sarapio* (110), the author does not properly emphasize the role of [r] in the shift pretonic *e* > *a*. *Taresa* < THARASIA (ibid.) hardly illustrates dissimilation of vowels; OPtg. *Tareija* faithfully represents the transitional form.

On the other hand, attractive possibilities of penetrating historical sound analysis were passed up in the cases of *tsoar* 'to carry' (90) < LEUĀRE and *dieldu* 'leaven, yeast' (108, 233) < LEUITU (as against It. *lievitare*, Sp. *aleudar*, Ptg. *levedar*), where a relative chronology of events can safely be established. The labialization of the pretonic vowel in *tsoar* must antecede the loss of the medial consonant. If it is true that dissimilation of consonants reshaped *dieldu*, the initial **ll-* must have changed to *d-* after *-bd-* > *-ld-*, yet before the general shift *ll-* > [ʃs]-, a valuable clue for the dating of the retroflex cluster. Rodríguez-Castellano shows greater alertness to such opportunities in his treatment of *duvietsu* 'ovillo' (98, 100, 243) < GLOBELLU.

⁴¹ In other languages such secondary initial clusters due to the suppression of the pretonic vowel are much more common. Concerning OPic. *preche* 'paresse', *frinne* 'farine', *prolle* 'parole', see H. Stimm's comments on Gossen's latest book, RF 64.473 (1952).

⁴² The *n* here is not due to false restoration, but probably to the influence of *puenzo* 'puso', *punxemos* 'pusimos' etc. (159); this paradigm, in turn, shows the influence of other sigmatic preterites. Conceivable semantic point of contact: the settling of a cloud of grain-dust.

⁴³ *Yelso* may have been imported, or been influenced by an imported form; normally, one should expect *ze-*.

and the interchange of *-ld-* and *-nd-* in *ja-*, *jerandilla* 'kind of popular dance' (110, 301) beside *jiraldilla* (Rato).

One serious weakness of the section on phonology is that sound changes due to the modification of the entire system have not been clearly delimited from others that are primarily derivational, inflectional, and lexical, with secondary effects on the configuration of sounds in individual words; the author might here have benefited from the advice of A. Alonso, *RFE* 22.67-72 (1935). *Cengarrá* 'cencerrada' (110-1, 306) involves the suffix variant *-arr-*, and the cleavage must be very old; *estil* 'manga de la guadaña' (63, 260) < *HASTILE* shows the encroachment of the frequently meaningful *es-* < *EX-* upon meaningless *as-*, cf. Sp. *esconder* < OSp. *asconder*, Sp. *escuchar* < OSp. *ascuchar* (*Ph.Q* 28.294-311 [1949]); topon. *Fervencia* vs. W.-Ast. *Fervienza* (51-2) testifies to two distinct channels of transmission of *-ENTIA* (*UCPL* 1.4.64-72, 79-86 [1945]); *oriégano* 'wild marjoram' (54, 215) < *ORIGANU* is not historically explained by the descriptive comment 'exceso de diptongación': it involves the interference of the suffix *-iego*, cf. the equally erratic course taken by *espliego* 'lavender' < OSp. *espligo* < *SPICULU* (*UCPL* 4.3.118-9, 156-7 [1951]). The initial segment of *recimo* 'racimo' (63) < *RACĒMU* is best analyzed by assuming the intrusion of *re-* (cf. *Remiro* and the like; through a reaction, *RESECĀRE* > *resgar* beside *rasgar*, the latter ultimately standardized). Topon. *Trescasa* (63) < *TRĀNS CASA(M)* can be most profitably studied against the background of *tresquilar* ~ *trasquilar* 'to shear' (G. Tilander, *St.N* 9.48-65 [1936-7]). Similarly, there is little point in speaking of lack of diphthongization in *pacencia* (51), of apheresis of vowel and consonant in *falfa* (113, 286), and of epenthesis of the nasal in *inresistible* (112): the changes underlying these variants can be more forcefully expressed in terms of derivation. Analogical leveling of inflection is the simplest explanation for *sabís* 'sabéis', *tenís* 'tenéis' (78), forms echoed by Central Argentine usage where *-ts* extends even to the *-ar* conjugation (B. E. Vidal de Battini, *El habla rural de San Luis* 1.120-1); the reader is somewhat misled by the contradictory statement on *deméis* (145). To account for *cordura* 'costura', *cordurera* 'costurera' (94, 240), the author posits an otherwise unsupported correspondence (102, §62: *-st-* > *-rd-*) which contradicts all our earlier experiences with Ibero-Romance; why not settle on the simpler hypothesis of special development due to confusion with *cuerda*? On the other hand, Menéndez Pidal's view (at least as old as the 1918 ed. of his *Manual*), reiterated by Rodríguez-Castellano (51), that Sp. *escudilla*, Ast. *escudietsa* 'bowl' represents a blend of *SCUTELLA* and *SCŪTUM* 'shield', allows of an alternative: the raising of the vowel in the pretonic syllable before a stressed rising diphthong (**escodiella* > *escudiella*) would, within the confines of Spanish, fully justify the *u*; wavering between the back vowels in this position accounts for such a regression as *TŪBELLU* (from *TŪBER* 'protuberance') > *tobi(e)llo* 'ankle'. True, the joint evidence of cognate languages (REW³ §7756) tips the scales in favor of the traditional explanation, with Italian significantly preserving traces of both *scodella* and *scudella*.

Only a few hints can here be given of the many structural problems raised by the author's data. The subdialect seems to tolerate a few long vowels; the change from the typically Spanish medium length is due to compensatory

lengthening: *sed* 'thirst' becomes [se:] (109), or to compensatory lengthening plus contraction of homogeneous vowels: *xugá* 'portilla de finca de travesaños movibles' (85, 277 = *uxera*), with the suffix -ĀTA > -ada, is said to be pronounced [xuɣá:].⁴⁴ This statement, if it is accurate, implies the existence of three quantitative categories of stressed vowels, explains their provenience, and poses the problem of their phonemic status. cursory perusal of the material has, to the best of my recollection, produced no contrasting pair of words differentiated solely by the quantity of the stressed vowel; but some structuralists, on both sides of the Atlantic, no longer regard such pairs as a prerequisite to the establishment of phonemic units.⁴⁵ As regards vowel quality, we have the author's express testimony (59) that for the natives any harmful homonymy between *pelu* (with half-open *e*) < PĀLU and *pelu* (with half-closed *e*) < PILU is impossible, whereas outsiders tend to confuse the two words in isolation. In another context (48), without cross reference, we learn that there exists a closed variety of *e*, as in the ending of the infinitives *caer**, *facere**, *tener**, and in *abeya*, *cabeza*, *ceya*, *corteya*, *cresta*, *yena*, etc., which goes back to Latin Ē or Ī, both reduced to [e] in most provinces of the late Roman Empire. These words are contrasted with the series *cereza*, *era*, *palanquiaera*, *vega*, characterized by half-open *e* (traceable, the author might have added, to an earlier diphthong *ei* < *ai*). Before a consonant group, we infer from further examples, the quality of *e* is determined not by its source but by the constituents of the cluster: a nasal as the first element closes the preceding vowel, *r* as the first element opens it; contrast *venga* with *berza*. At the end, however, the reader does not know whether the relatively old *e* of *era*, *vega* and the relatively recent *e* of *pelu* < PĀLU (which in different passages have both been labeled half-open) are identical even on the phonetic level. Without such knowledge, any conjectures about phonemic structuring seem hazardous.

For the diachronic phonemicist principally interested in chain reactions,⁴⁶ there arises the question whether the visibly recent tendential shift -a > -e (in a territory separate from the -as > -es zone; see map 2, between 68 and 69) may not have been provoked by the trend -u > -o, which, it will be recalled, was set in motion by morphological leveling. The fact that -u characterizes masculines as much as -a does feminines may have been a factor also.

At the end of the long section on phonology, one misses a concluding chapter with a summary of significant variations observed within the subdialect, followed by a breakdown into those that correspond each to one or two of the three localities investigated, those found in a single community, and those peculiar to a single informant. Of the first category we have a few clear-cut examples: *butlu* (F) ~ *buelo* (C) beside *gūilo* 'abuelo' (52, 58, 113, 307); *cuertera* (F) ~ *cuartera* (C) 'cobertera' (54, 250), cf. COOPERTÖRIU (REW³ §2206); *roelgos* (F) ~

⁴⁴ Phonologically, this word could be interpreted as an offshot either of IUGUM or of IOCUS. The specific reference to the (removable) crossbar suggests imagery best compatible with the 'yoke'. Cf. also *corralá* [koʔalá:] 'plazoleta de la casa' (90, 246).

⁴⁵ See B. Bloch, *Lg.* 26.96 (1950), and M. Cohen, *BSLP* 48: 2.129 (1952), with a reference to D. Jones, *The phoneme*.

⁴⁶ This approach has in recent years been most successfully used by A. Martinet; see *RPh.* 5.133-56 (1951-2) and his contributions to the latest volumes of *Word*.

regüelbos (V) 'cosquillas' (89, 303); *selmana* (F) ~ *sermana* (C) 'semana' (108, 135); *la tie María* ~ *la tié Leonor*^e (C) (66, 80, 308). The word with the largest number of securely distinguishable variants, resulting from metaphony and instability of medial spirants, is *fuebu* (C), *fuibu*, *juibu* (V), *futu*, *jutu* (F, Pelúgano), *jué* (V) 'fire' < *FOCU* (73-4, 81, 253).⁴⁷ The second category is rarer; an example in point is *trébenes* ~ *trébedes* (F; see 115, 252) < *TRIPEDĒS* (for *TRIPĒS* -EDIS). The third category, given Rodríguez-Castellano's recording technique, is unidentifiable. There are stray instances of overlapping of categories, e.g. *estregu* (F, V) ~ *estreu* (F) 'cama del pan' < *STRĀTU* (74, 233). In still other cases, the distribution in space and time is not quite clear: *ayiri* (also prevalent in the Nembra Valley) ~ *ayer*^e (58, 167) < *HERĪ*, conceivably influenced by *Ā NOCTE* (cf. Ptg. *ontem*); *cuergo* ~ *cuirgu* (115, 204) < *CORUU*; *om* ~ *ome* < *HOMŌ* -INIS (107, 307) etc.⁴⁸ What one would like most to assess is the degree to which the distribution of lexical variants represents the distribution of phonological patterns—an old crux of historical linguistics. A morphophonemic tabulation of grammatically controlled stem variants would have been a further desideratum. As an example, notice the alternation /a/ ~ /e/ imposed by such contrasts as gender: *péxaru* 'pájaro' (59, 100, 206) ~ *pázara pinta* 'aguzanieves' (206), number: *téladru* ~ *táladros* (59, 80, 292), derivation: *xetu* 'ternero' ~ dimin. *xatín* (61, 285), and pronominal extension of the verb: *tomar* ~ *tomé-lu* 'tomarlo' (57).

7. Etymology. Historical grammar can be constructed only on a foundation of accurate etymologies. The Latin-Asturian lexical axis is the line of investigation that visibly interests Rodríguez-Castellano least of all: he shows no flair for original solutions (for which there is a crying need), makes a few serious blunders on the Latin side of the equations, is not always guided by trustworthy pioneers, and, even where he follows authoritative manuals, does not sufficiently guard against slipshod formulation. The many attractive problems posed by his material will be dealt with separately; here we confine ourselves to the correction of obvious errors and to brief comments.

Some of the proposed etyma are dubious, as the statement on *rancón* 'rincón' (65, 315 b); others have long ago been discarded, e.g. *CONCAUA* (García de Diego) as the base of Sp. *cárcava*, Ast. *cuarcua* (62); still others, though widely quoted, clamor for revision: thus, the traditional derivation of *guia* 'aguijada'

⁴⁷ Occasionally, the preferences of other Asturian localities are set off against the usage of the Upper Aller or against one another: *blimba* (Alto Aller) < **bimbla* (cf. *fembla*, *sembra*) < *UÍMINA*, pl. of *UÍMEN* ~ *blimal* (Pola del Pino) < *UÍMINĀLE*, preserved as a name in ancient orography (107, 213); *caer* (F) ~ *cayer*^e (Pola del Pino) < *CADERE* (74); *edra* (Alto Aller) ~ *yedra* (Santibáñez de Murias) 'yedra, muérdago' < *HEDERA* (51, 213); *eire* (Llamas) ~ *eigre* (F, V) < *ĀERE* (76, 199); *fema* (Murias) ~ *fembla* (Alto Aller) < *FĒMINA* (93, 107, 281); *pechu* (Nembra) ~ *pichu* (Moreda) < *PECTU*(s) (58); *tsueñe* (Alto Aller) ~ *tsuiñi* (Nembra) < *LONGĒ* (52, 58; add to REW^s §5116: Ast. [tsuentʃi]).

⁴⁸ This applies also to *entonces* ~ *entónceñes* (50, 167, 313 b) < **INTUNC*(E), cf. *EXTUNC* (Vulgate, Venantius Fortunatus); to *penerar* ~ *penarar* 'cerner' (100, 110, 235), from *penera* < *PINNĀRIA*, of which only the first is localized (F); to *picatyo* (F) ~ *picayo* (where?) ~ *piquero* (where?) 'picadillo' (76, 231). A case apart is *faula* 'cuento' < *FĀBULA* (89, 108) ~ *afalar* < *FĀBULĀRE* 'estimular (el ganado)', which may have spread from two different focal points: the substantive calls to mind Castilian, the verb Galician-Portuguese cognates.

(90, 104, 113, 282), *aguijón* 'aguijón' (104, 273) seems worthless without the adjunct that the word family has been deflected from its normal course by *ijada*, from *ĪLIA -IUM*.⁴⁹ A few etyma may be new, but stand no chance of being accepted: thus, *maurientu* 'mohoso' (91) is surely unrelated to *MŪCŌRE*, which suffered an accent shift entailing syncope (> *Sp. mugre*; for a reverse change, cf. *PIGNORE* 'pawn' > *Ptg. penhor*) and may be easily linked to *maurecer* < *MĀTŪRĒS-CERE*.⁵⁰ Many equations suffer merely from inaccurate formulation or are in need of slight adjustment: thus, *inxertar* 'to graft' (95) reflects *INSERTĀRE*, recorded since Vergil, rather than *ĪNSERERE*.⁵¹

In numerous instances an asterisk must be supplied, e.g. *antoxenu* 'antojana' (106, 117, 246) < ANTE [*]ŪSTĪĀNU (better: *ŌSTĪĀNU), *azá* 'azada' (102, 259)

⁴⁹ Two more examples. *Silgueru* 'jilguero' (86, 206) is not directly etymologized, but implicitly classed with *silbar*; although Meyer-Lübke followed Baist in crediting it to Greek (REW³ §7924), intermediate forms like *serguero*, *selguero*, *xerguero*, *zelguero* prove that Menéndez Pidal's base **sĒRICĀRIU* alone is correct. *Arrandar*, *ariandar* 'arrancar las hierbas malas, juntar tierra alrededor del maíz' < **ARRĀRENTĀRE* is one of the oddest fabrications of García de Diego's etymological laboratory (110, 255); REW³ §7042 gives the correct etymon, the variant may be due to a blend with *reondo* < *ROTUNDU*, which would do justice to the second meaning.

⁵⁰ *Recietsa* 'cabeza de ganado menor' (51), *recietsu* 'ganado menudo' (117), with counterparts in other subdialects, can hardly serve as an excuse for the reconstruction of hybrid **RESCELLA*. *Reútsu* 'roble' (89, 98, 213) presupposes **RŌBULLU*, an archaic diminutive of *RŌBUR* (cf. *CULTER* ~ *CULTELLUS*), as the starting point, as is borne out by the more conservative variant *rebutsu*, pl. *rebotos* (C: 55, 212); *ro-* > *re-* is probably less due to the intrusion of the prefix than to the dissimilation of back-vowels (as in *Sp. redondo*, *OSp. secodir*, *Sp.* < *Cat. reloj*), cf. the legal term *OSp. OPTg. revorar* < *RŌBORĀRE*, in which the prefix may have intervened more strongly, echoing *reforçar*.

⁵¹ Thus, *terral* (F), *tarral* (C) 'ternero joven' is neither *TERNERALE* (107) nor **TERNERALE* (110), but at best **TENERĀLE*, from *TENER(U)* -A 'gentle, soft', with a development familiar from *Fernando* < *Fernando*. *Agora*, *abara* cannot be claimed both for *AD HŌRA[M]* (50) and for *HĀC HŌRĀ* (168); this is the correct base, cf. the survival of *HŌC ANNŌ*, much less (a) *viespre* (65, 208) for *UESPA* (112) and *UESPULA* (51); nor is any doubt permissible on the relative merits of Menéndez Pidal's (*DĒPŪRĀRE*, previously posited by A. Thomas for another dialect group) and García de Diego's explanation (*nŪTYRUM*) of *deburar* (90, 296). *Bazo* (100) is an offshoot of *BASSU* presumably influenced by **BASSĪĀRE*, cf. *Sp. agrio*, *amargo*. *Anueche* (53, 168) presupposes *Ā NOCTE*, as does *Ptg. ontem*, cf. Svennung's and Gamillscheg's latest studies on the preposition involved. *Alandrina* 'golondrina' (110, 112-3, 204) has a complicated history, but its nucleus is *HIRUNDŌ -INIS* 'swallow' rather than (*H*)*ARUNDŌ -INIS* 'reed'. Topon. *Yanáu* (96) is traceable not only to the *PLĀN-* family, but specifically to *PLĀNĀTU*. *Apazongar* 'pastar el ganado' (106, 285) continues **PĀSTIŌNICĀRE*; *ayegar* (112), *APPLICĀRE*, preserved from the Atlantic to the Mediterranean; *cueye* (53), *COLLIGIT*; *eniciar* 'tener descendencia el ganado' (64-5, 281), *INITIĀRE*; *ensuchu* 'enjuto' (101, 313 b), *EXSŪCTU*; *seronda* 'tardía' (adj.), 'época de fines de otoño' (54, 276), *SĒRŌTINA*, cf. *ANNŌTINUS -A -UM*; *tsandre* 'bellota' (97, 113, 213), *GLANDE*; *tsimaz* 'limaco', *LIMĀCE* (correct REW³ §5045, which confuses hypercharacterization of gender with suffix change in *Sp. limaza*). Asturian plurals must not gratuitously be equated with Latin singulars (*vicino*: 110), nor masculines with feminines (passim). Delete the question mark (106) after *pación* < *PĀSTIŌNE*, cf. *comezón* (REW³ §2078 b). The casual etymological interpretation of Leon. *bierzu* (102) is untenable; Rohlf analyzes it as a cognate of *Berceo* and traces it to Celtic (*ASNS*, 189.260 [1952-3]), confirming an earlier view held by G. Baist (*Gr. Gr.* 1.704; 2d ed. 1.900) and E. Alarcos Llorach (*Berceo* 5.5-24 [1950]). The entire problem is analyzed anew by J. Hubschmid in an article just off the press, *ZCPH.* 24.204-26 (1953).

< [*]ASCIĀTA, from ASCIA (REW³ §696, §697), *burtsa* 'burla' (98-9, 309) < [*]BŪRULA;⁵² elsewhere it should be dropped: *rosa* (50) < RŌSA.⁵³ Sometimes, the author has been misled by REW³ into omitting the star, as in *cazar*^o (106) < [*]CAPTIĀRE, though in other instances of special concern to the student of Asturian, Meyer-Lübke used it quite unjustifiably.⁵⁴

⁵² *Abeduliu*, -*uriu* 'abedul' (70, 210) < [*]BETULU; *af(l)echar*^o 'machacar, romper terrones' (101, 110, 255) perhaps < [*]FRĀCTĀRE, supported by FRĀCTĀMENTUM (glosses); *asemeyar*^o 'parecerse' (104, 112, 312 a) < [*]SIMILIĀRE, better still *ASSIMILIĀRE (REW³ §730, §7926); *cerná* 'ceniza (mojada) de hacer la colada' (107, 240) < [*]CINERĀTA; *cubiciar*^o 'codiciar' (90), from [*]CUPIDITIA, for -ITĀS; *escaéz* (102), from [*]EXCADĒSCERE (only DĒ-, EX-CIDERE, Late Lat. CADĒSCERE recorded); *esfrece*^o 'enfriarse' (102) < [*]EXFRIGĒSCERE (only DĒ-, IN-, INTER-, PER-, RE- known as preverbs of FRIGĒSCŌ); topon. *Fadietsa* (91) < [*]FĀGĒTELLU (cf. Bret. *faouet* < *FĀGĒTU, Ernout-Meillet); *fozar* (105-6) < [*]FODIĀRE, beside FOD-ERE, var. FOD-ĪRE, unless related to *hocico*, see my forthcoming *Studies in the reconstruction of Hispano-Latin word-families* (UCPL); *migaya* (91, 103, 229) < [*]MĪCĀCULA, unless the (eventually homonymous) suffix -ĀLIA is involved; *mostadietsa* 'comadreja' (110, 116, 203) < [*]MUSTĒLELLA or—the recorded diminutive of MUSTĒL(L)A being MUSTĒL(L)ULA, cf. STĒL(L)IŌ 'starred lizard' from STĒLLA—[*]MUSTĒLELLA, the latter variant still more subject to consonant dissimilation; *neal* 'nido' (90, 206) < [*]NDĀLE; *pare(a)* 'pared' (109) < [*]PARĒTE, for PARIĒTE; *pielgu* 'tetas de la cerda' (108, 289) < [*]PEDICU (only PEDICA recorded); *piscuezu*, -*uizu* (69, 106) < [*]POS(T)-COCCEU; topon. *Pola (del Pino)* (108 fn. 1) < [*]POPULA, as against *pūlu* < POPULU (52, 56, 108, 310; the relation to topon. [El] *Poulo* < PĀBULU 'fodder', var. *Poula*, as reconstructed by Krüger, *Die Gegenstandskultur Sanabrias* 157, and Piel, *RF* 64.255-6 [1952], invites further study); *regüelbos*, *roelgos* 'cosquillas' (89, 303) < [*]REUELICĀRE—only iter. (Ē)UELLICĀRE, from UELLŌ -ERE, is recorded; *riñetsa*, comparable to Sp. *rencilla* and the exact equivalent of OSp. *riñ(i)ella* (103, 310) < [*]RINGELLA, instead of RICTUS -ŪS and RICTUM -I; *roendo* 'rodezo' (111, 294) < [*]ROTCINU, paralleled by *torrendo* 'torrezno' (111, 229) < [*]TURRICINU; *salendar*^o 'respirar' (111-3, 217, 219) beside *ensalendar* < [*]ANHĒLITĀRE, if such a reconstructed stage is needed; *salgueru* 'especie de sauce' (57, 215) < [*]SALICĀRIU; *tarreña* 'vasija de barro' (110, 252) < [*]TERRĀNEA (a variant in *-INEA may have existed already in provincial Vulgar Latin; for Italian reflexes, see REW³ §8671); *tayón* 'banco bajo y alargado del madreño' (104, 292) < [*]TĀLEŌNE, from TĀLEA, -IA 'cut-off piece', which became the head of an important Romance family; *tsamarga* 'terreno húmedo' (108, 111, 202) < (TERRA) *LĀMĀTICA, from LĀMA -AE, cf. LĀMĀTUS 'untidy' (glosses); topon. *Las Tsinariegas* (86) < [*]LĪNĀR-ECA, from LĪNUM -I (there is, of course, no need to project into antiquity the coinage of the derivative, given the increasing productivity of the suffix); *uzera* 'portilla de finca con quicio' (C, V), *fuzera* 'id.' (F; influence of FUGERE—the 'escape-door'?) (106, 277) < *ŪSTIĀRIA (ŌSTIĀRIUS -A -UM alone is recorded); *vanceyu* 'vencejo' (110) < [*]UINCICULU (if correct; the full history of this bird-name remains to be pieced together); *xamescu* 'rama que levanta llama al arder' (97, 254) beside *xamasca* 'rama sin hojas de cualquier árbol' (213), from [*]FLAMMUSCĀRE (again, the formation need not have arisen in antiquity); *yueza* 'clueca' (52, 97, 289) < [*]CLOCCEA (a very doubtful reconstruction); *zubietsa* 'atadero de ramas retorcidas' (110, 115, 268, 272) < [*]FĪBELLA, in preference to FĪBULA. In addition to bases, hypothetical intermediate stages, to which the author rarely commits himself (74), might have been starred.

⁵³ On this least understandable error recurrent in Romance studies, see J. Bruch, *RF* 64.445 (1952), apropos of H. Schmid's use of INSTĀRE and LACTEM.

⁵⁴ The following corrections of REW³ seem pertinent in this context: *embú* 'embudo' (91) continues IMBŪTUM, the normal participle of IMBUERE (the innovation lies in the meaning, not in the form); *ensiñe* 'enseña' (48) similarly goes back to a word with a new meaning: IN-SIGNĀRE *'to teach' (in glosses: 'to engrave' = ἐγχαράσσω); *juerza* 'fuerza' (81) reflects FORTIA 'feats of strength and courage' (Vergil), 'strength' (in this sense, perhaps initially common

The merits and weaknesses of Rodríguez-Castellano's book are symptomatic of the present status of Spanish dialectology as a whole. No one cares to disparage this unassuming, devoted group, laboring under unfavorable circumstances; but a frank appraisal of their performance through the past decade can no longer be delayed. In contrast to the period before 1936, when teams of foreign Romanicists combed the country, the entire responsibility is now in the hands of a domestic staff largely recruited from natives of the provinces, trained in Madrid. The advantages of this situation are obvious: close contact with informants, familiarity with local conditions, avoidance of errors typical of the unadjusted fieldworker, the possibility of preliminary surveys and later spot checks: in short, sharpness of focus, especially on the descriptive level. However, there are also drawbacks: lack of historical perspective, indifference to linguistic methodology (since discussion of methods presupposes a wide range of curiosity), severely limited skill in handling Latin and other dead languages (including Old Spanish), little if any familiarity with research in cognate languages: in short, narrowness of scope and inadequacy of critical insight, especially on the diachronic level. As long as these deficiencies are not remedied, the analysis of the precious data collected by workers like Rodríguez-Castellano will have to be largely carried on by comparatists.

Manuel de langue roumaine: Grammaire, textes d'étude commentés avec index grammatical et glossaire. By ALAIN GUILLERMOU. (Les langues d'Europe orientale, Vol. 5.) Pp. 285. Paris: Librairie C. Klincksieck, 1953.

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A European linguist recently wrote to this reviewer, in an exchange of correspondence about Romanian, that 'toutes les études portant sur cette langue sont entreprises dans l'esprit des grammaires traditionnelles et nous manquons terriblement d'études descriptives conduites suivant les principes de la linguistique structurale moderne.' No exception can be made for this new manual by Alain Guillermou, Professor in the École Nationale des Langues Orientales Vivantes. In his preface, the author acknowledges on the one hand the teachings of Mario Roques and Jean Boutière, both of the École, and on the other hand the counsel of the eminent European linguists Carlo Tagliavini, Sever Pop, and Grigore Nandriș.

Introductory statements (5-6) emphasize the 'originalité' of Romanian among the Romance languages, and place it in a sort of 'communauté linguistique balkanique' by virtue of certain structural patterns shared with Bulgarian

among early Christians; witness the usage of Commodianus, Carmen Apologeticum [late 3d cent.], and of Prudentius); *molîn* 'molino' (69, 293) represents Late Lat. MOLINUS -A -UM (Ernout-Meillet); *palomba* 'paloma' (100) corresponds to PALUMBĒS (-IS) -IS beside PALUMBUS, -A, cf. PĀUŌ -ŌNIS 'turkey' beside PĀUUS -I; *sechuru* 'reja cortante o cuchilla del arado' (101, 271) < SECTŌRIU (Digesta)—aside from its indefensible asterisk, Meyer-Lübke's *SECTŌRIUS 'Werkzeug zum Schneiden' (§7769) unaccountably shows a masculine ending.

and Albanian.¹ The 'difficulté' of this language is discussed (7) in terms of its subtlety, its not always coherent complexity, its lack of fixedness. Then the purpose of the book is announced: it is to be used by students who 'ne veulent pas d'une initiation hâtive et par là même inutile.' More specifically, it is to be a guide which will facilitate the students' approach to any page of good prose and will orient them toward more advanced research of their own.

The main matter of the book is in two sections: I. Exposé grammatical, II. Textes annotés. Part I is termed in the introduction an 'exposé morphologique' but it is, as the author himself says, 'truffé' with much of the syntax which space limitations deprive of full and separate treatment. Subheadings within the exposé, following an Introduction phonétique, are: 1. Le substantif et ses déterminations immédiates; 2. Les pronoms et pronoms-adjectifs; 3. Le verbe; 4. Chapitres annexes. It seems profitable to comment on each section in turn.

The Introduction phonétique is innocent of phonemic analysis or underlying phonemic theory.² The units are the letters of the Romanian alphabet; the pronunciation of each is described in articulatory terms, often accompanied by comparisons with other languages.³ The treatment is as thorough as the author's methods permit; a linguistically oriented student attempting to use the *Manuel* as a corpus in lieu of spoken utterances could, through careful sifting of the data, arrive at a list of the segmental phonemes.⁴ But because Romanian orthography fails to indicate stressed vowels, Guillerrou says (21): 'Il est très difficile de donner des règles sûres en matière d'accentuation roumaine. Le principe traditionnel suivant lequel un mot terminé par consonne est accentué sur la syllabe finale et un mot terminé par voyelle sur l'avant-dernière souffre tant d'exceptions qu'il vaut mieux ne pas en tenir compte', and then in the midst of a two-page token treatment of stress in nouns alone he adds pessimistically (22): 'Enfin, dans de nombreux cas règne une incohérence apparente qu'il serait difficile de réduire sans se livrer à de véritables monographies.' In other words, stress is phonemic in Romanian. In his Glossaire général, which at the end of the

¹ For instance, postposition of the definite article as in *lupul* 'wolf the'; avoidance of the infinitive in favor of the subordinate clause, as in *vreau să plec*, 'I want that I leave'.

² The author might wish to point out that neither the phonetics nor any of the subsequent descriptions are intended solely for students trained in the newer structural linguistics. This is undoubtedly so, but it is now rather widely accepted that statements about a language can be more simply and more clearly organized for the linguistically uninitiated along lines indicated by a scientific pre-analysis of that particular language, than along the lines dictated by traditional grammar as applied to all languages. It is in defense of this view that the material of the *Manuel* is here criticized.

³ For example, after an earlier statement that a breve placed over *a* 'results in' a different sound, we read (12) that 'la voyelle *ă* est une voyelle mi-ouverte légèrement postérieure, non labiale, proche de notre *e* muet et du son de la voyelle *u* dans l'anglais *but*.' This example is singled out here because in this reviewer's experience no phones of the central vowel phoneme (orthographic *ă*) are 'slightly back', while some indeed are slightly fronted.

⁴ The statement that in certain words (e.g. *inimă* 'heart') initial *i* is pronounced *yi*, combined with the information that the diphthongs *ii* and *iu* figure in both the ascending and the descending series, yields the knowledge that /i/ and /j/ are separate phonemes. Regarding the probability of a contrast /u/ : /w/, however, no helpful hints can be extracted from the *Manuel*. The reviewer is not yet certain that a phoneme /w/ must be included in the stock, although at the present stage of his own investigations he assumes it.

book lists all words cited in the grammar and texts, the author resorts to a typographical system of marking stress which, though complicated and presupposing some knowledge of morphology, serves its purpose well enough. With this aid the student-linguist would be able to phonemicize tentatively most of the words in the book which do not contain the letter *i* in final position or next to another vowel, including another *i*; these writings do not accurately reflect the phonemic contrasts /i : j : ij : ji/, and Guillerrou's phonétique simply does not provide the crucial information.

The section devoted to Le substantif covers what the descriptivist would term noun morphology and endocentric noun phrase structure. Although the author echoes the old saw that Romanian 'preserves' five cases—nominative, vocative, genitive, dative, accusative—he quickly rejects this traditional parallel with Latin as reflecting 'une richesse purement théorique' (25) and follows the current practice of assigning to the noun a nominative-accusative, a genitive-dative, and a vocative case. And since the vocative 'reste en marge de tout paradigme', he relegates its description to the miscellany which makes up his Chapitres annexes. In a footnote to his description of the simple or non-articulated forms of the noun he points out (26) that 'les survivances d'une flexion casuelle sont beaucoup plus nettes dans les articles: ce sont eux qui marquent le cas et non pas la terminaison du substantif'. Nevertheless, missing his own cue for a more realistic basis of analysis, he goes on to state that masculine singular nouns are identical in their nom.-acc. and gen.-dat. cases, e.g. *lup* /lúp/ 'wolf', as are also all plural nouns, e.g. *lupi* /lúpj/ 'wolves', *case* /kása/ 'houses', *cărți* /kércj/ 'books'; and that feminine nom.-acc. singulars like *casă* /kásə/ 'house' or *carte* /kárte/ 'book' have a gen.-dat. identical with their one plural form. Only the articulated forms (for 'the wolf', 'the house', etc.) show constant formal contrast, e.g. nom.-acc. /lúpul/, gen.-dat. /lúpuluj/, pl. /lúpij lúpilor/; nom.-acc. /kása/, gen.-dat. /káselj/, pl. /kásel káselor/; nom.-acc. /kártea/, gen.-dat. /kércij/, pl. /kércile kércilor/. Comparison of such forms with pairs of phrases like /un lúp/ 'a wolf' : /unuj lúp/ 'to a wolf'; /ačástə kása/ 'this house' : /ačéstej kása/ 'of this house'; /múlte kércj/ 'many books' : /múltor kércj/ 'of many books', leads the linguist to isolate a genitive-dative or OBLIQUE morpheme (with allomorphs -j and -or in complementation relative to the plural inflection), which may be added to articulated nouns and to certain classes of attributes of non-articulated noun heads; with the note that these heads, if they are of the class of /kása/ or /kárte/, merely carry an added allomorph of agreement with the oblique morpheme.

As to the subclassification of nouns and adjectives into morphological types, Guillerrou accepts the conventional grouping into masculines, feminines, and ambigenes (the latter being masculine in the singular, feminine in the plural). His tabulation by plural types sets up eight classes of masculines, nine of feminines, and twelve of ambigenes (43, Table III). And these do not include the numerous 'alternances phonétiques' suffered by the vowels and/or final consonants of the bases, which are described in Appendix I, at the end of the section (89-98). In the Glossaire général, each noun is tagged with a designation such as F3 or A9 in cross reference to Table III, plus *c.* if it has a consonant

alternation, plus *v.* if it has a vowel alternation, plus an asterisk if by virtue of its unpredictable behavior the item is also listed in the alphabetical *Répertoire grammatical* which precedes the *Glossaire* (233–57); in the *Répertoire*, the entry is cross-referenced to a paragraph of the main text. Seemingly the author could not have presented all his facts in any less cumbersome or diffuse way while adhering to Romanian orthography. In a recent article—Noun morphology in Romanian, *Lg.* 29.134–42 (1953)—the reviewer proposed a method for classifying and describing any Romanian noun in a single entry. Here are some samples of the notation, compared with the more cumbersome and less informative system of Guillerrou:

<i>bastoănu-</i> 'cane'	(baston, A1 <i>v.</i>)
<i>kopiLu-</i> 'child'	(copil*, M1 <i>c.</i>)
<i>kwintu-</i> 'word'	(cuvânt*, A1 <i>v.</i>)
<i>frate-u-</i> 'brother'	(frate*, M7 <i>c.</i>)
<i>parte-</i> 'part'	(parte*, F1 <i>c.v.</i>)
<i>viteăzu-</i> 'brave man'	(viteaz*, M1 <i>c.v.</i>)

The author groups adjectives morphologically with nouns, assigning the 'masculine' forms of such an adjective as *bun* 'good' to his class M1 and its 'feminine' forms to F6, and entering it in the *Glossaire* as *bun**, M1, F6; the differently patterned adjective meaning 'heavy' goes in as *greu**, M3, F10. Although they do overtly match nouns in their terminal shapes, adjectives differ from nouns in their morphemic composition: they consist of a bound base plus an agreement morph, and can be articulated only when phrase-bound to an unarticulated noun head. According to their class, they have one, three, or four agreement morphs. All this information can be conveyed by a morphophonemic notation, writing *bun-* for *bun**, M1, F6; *greu-* for *greu**, M3, F10; and *verde-* 'green' for *verde*, M7 *c.*, F1 *c.*

The section *Les pronoms et pronoms-adjectifs* follows strictly conventional lines (demonstratives, interrogatives, indefinites, possessives, numerals, personals, relatives), and again combines morphology and phrase structure. It is passed over here for the sake of dwelling in sufficient detail on the next section, *Le verbe*.

Under the first subheading, *Les modes et les temps*, the verb is fitted into the framework traditionally used for any Romance language. The modes are the infinitive (with two tenses), the indicative (with seven), the subjunctive (with two), the conditional (with two), the imperative, the present participle, and the past participle. The infinitive category includes the 'long infinitive', which occurs in phrases such as *cântare-aş* ('I would sing'), subsisting 'dans les chants populaires et dans les parlers dialectaux' (109); it should be added that in the standard language this so-called 'long infinitive' is strictly a noun derived from verb stems by means of the suffix *-re*. The past infinitive is a phrase consisting of *fi* 'be' plus the past participle (literally 'to be having sung' etc.). Of the seven listed 'temps' of the indicative, four are paradigms of one-word forms (present, imperfect, simple past, pluperfect), while the remaining three are sets of phrases: the future, consisting of an auxiliary plus the infinitive (*voi cânta* 'I will sing'); the *passé composé*, auxiliary plus past participle (*am cântat* 'I have sung');

and the future anterior, future auxiliary plus *fi* plus past participle (*voiui fi cântat* 'I will have sung').⁵ The present subjunctive is identical to the present indicative in the first and second person, while in the third person (and in all forms of the verb *fi* 'be') the variable shapes are in complementation relative to the presence or absence of the function word *să* (e.g. *cântă* 'he sings' : *să cânte* 'that he sing'). And since the past subjunctive is a phrase in complementation with the perfect phrase relative to *să*, the subjunctive may simply be eliminated as a nonsignificant category. Both 'tenses' of the conditional are phrases, e.g. *aș cânta* 'I would sing', *aș fi cântat* 'I would have sung'. Morphologically, then, the welter of verb forms presented in the *Manuel* may be reduced provisionally to four finite tenses, an infinitive, two participles and an imperative.

Analytical study of the forms involved, however, leads to an even simpler picture. In morphemic terms, the Romanian verb has a stem and the following inflections: a morpheme of past time, a morpheme of perfective aspect, and a set of mutually exclusive person (or actor) morphemes. The stem with the actor morphemes is the 'present tense', e.g. *vorbi.m* 'we speak, we are speaking'. Stem plus past-time morpheme plus actor makes up the 'imperfect', e.g. *vorbi.a.m* 'we spoke, we were speaking'. Stem plus perfective aspect plus actor is the 'simple past', e.g. *vorbi.ră.m* 'we spoke'. Stem, past, perfective, and actor all combine in the 'pluperfect', e.g. *vorbi.se.ră.m* 'we had spoken', in which the past morpheme has the alternant *-se-* as against *-a-* above.⁶ The bare stem has allomorphs which in isolation are the 'infinitive' (phrase-bound only, e.g. *am vorbi* 'we would speak') and the 'present participle' (free, e.g. *vorbind* 'speaking'). Stem plus perfective, without actor, constitutes the 'past participle', which is phrase-bound except as an adjective (e.g. *am vorbit* 'we have spoken', in which the perfective has the alternant *-t-*, or in some verbs *-s-*, in the absence of an actor morpheme). Finally, most verbs have no morphological imperative; commands are expressed with stem-plus-actor forms by means of syntax or intonation or both; the three or four verbs which do have distinct 'imperative' forms may be said to have special alternants occurring in the syntactical or intonational environment characterizing commands.

Guillermou lists verbs in the *Glossaire* in their infinitive form, each followed by the designation VI, V2, V3, or V4 to mark the 'conjugaison', a descriptively valid classification; he stars those which are also included in the *Répertoire grammatical*. The morphophonemics of verbs are complicated; but with the exception of the highly irregular *fi* 'be' and *avea* 'have', it is possible to indicate the total inflectional pattern of any verb—including (as for substantives) all the alternations of stem vowels and consonants treated in Guillermou's nine-page

⁵ Native speakers claim that this phrasing is 'awkward', that they recognize it but avoid its use.

⁶ It is to be noted that the element *-ră-* is missing from the pluperfect forms used by some speakers, who thus say for example *vorbi.se.m*. In such instances the morph *-se-* is evidently a portmanteau of past and perfective. However, most speakers of the standard language do not use the 'simple past', replacing it with the perfect phrase, e.g. *am vorbit* 'we have spoken, we spoke'. Their pluperfect *-seră-* or *-se-* is then a single morpheme of perfective past, contrasting with *-a-* as a morpheme of imperfective past.

Appendix II (138-46)—in a morphophonemic notation of the stem, e.g. *pođiê-* 'be able', *viNi-* 'come'.

The Chapitres annexes deal respectively with irregular nouns and adjectives; comparative and superlative; proper nouns; courtesy titles; vocative; kinship nouns; invariable words (adverbs, prepositions, conjunctions); sequence of tenses; and the use of the infinitive. Several of these oddments could have been safely included in the main sections on nouns and pronouns; the comparative and superlative constitute a purely semantic category in Romanian;⁷ the sequence of tenses and the use of the infinitive find themselves relegated to this section only because there is no proper treatment of clause structure elsewhere.

Part II, Textes annotés, presents fourteen prose passages averaging a little over a page, selected with a view to illustrating a maximum number of syntactical points. These points, the majority of them not included in the earlier formal exposé by reason of their 'importance réduite' (173), are presented in the order of their textual occurrence in over 700 notes (34 pages) keyed numerically to the texts. It is assumed that the learner will translate the passages, either working through text and notes concurrently, or first relying on knowledge gained from the grammatical exposé and later coming to the notes 'dans un but de contrôle'.

The purpose of the book has thus been reaffirmed. In the light of that twice-announced objective—cf. 'fournir au débutant un guide qui lui permet d'aborder n'importe quelle page de bonne prose' (7)—the question arises why the exposé is encumbered on the one hand with linguistic history and on the other hand with proscriptions. If it is assumed that the student comes to Romanian with some general knowledge of the evolution of other Romance languages (mainly French), the historical data qualify at best as incidental intelligence, not without interest to be sure. But how justify the proscriptions? These are against two sorts of thing: constructions which are never used by native speakers and could only occur as produced by a foreigner, e.g. 'L'emploi du pronom fort isolé est une incorrection grave: *n'am văzut pe tine* [instead of *nu te-am văzut pe tine* 'I haven't seen you'] n'est pas roumain' (80), or 'L'omission de ce pronom de rappel est une faute à ne jamais commettre' (88); and expressions which Romanians do use, albeit without the sanction of authoritarians, e.g. 'Un tour comme *clasa treia* [instead of *clasa a treia* 'third class'] est gravement incorrect, bien qu'il se rencontre très souvent dans la langue parlée et le style relâché' (77), or 'Dans la langue courante *ce* isolé se rapporte très souvent à des personnes. Cet usage est à proscrire' (86). In the first type of prohibition the student is told in effect what to avoid saying, and in the second he is taught to disapprove of something he is not likely to encounter anyway in a 'page de bonne prose'. If he is not expected to speak or write Romanian, or to understand the 'langue parlée', such proscriptions are clearly out of place. These aims are not explicitly stated, and

⁷ Somewhat oversimplified, the situation is that adjectives and adverbs have various anterior attributes; *mai bun* 'better' is in a class of phrases along with *prea bun* 'too good', etc. These phrases, like the single adjectives or adverbs, may follow the pronoun *cel* (which Guillermeu calls an 'article adjectival'), e.g., *cel bun* 'the good one', *cel mai bun* 'the best one'.

there are no exercises to implement them; but if by chance they are implicit, then the *Manuel* is subject also to a different order of criticism in the light of recent developments in the designing of textbooks for teaching active control of a foreign language.

Dois problemas da língua portuguesa: O infinito pessoal e o pronome *se*. By THEODORO HENRIQUE MAURER JR. (Universidade de São Paulo, Faculdade de Filosofia, Ciências e Letras, Boletim 128, Filologia românica No. 3.) Pp. 71. São Paulo, 1951.

Reviewed by JOSEPH H. D. ALLEN JR., *University of Illinois*

The author, a Brazilian who studied under Leonard Bloomfield and others at Yale University in 1945-6 and who now holds the chair of Romance Philology at the University of São Paulo, is perhaps best known in this country for two studies which have appeared in these pages.¹ In the present volume he turns his attention to two perplexing problems of Portuguese linguistics, the personal infinitive and the pronoun *se*.

In the rather loosely written essay on the personal infinitive, the author reviews some of the theories previously suggested (8-15), criticizes the theory, heretofore generally accepted,² of Rodrigues and Carolina Michaëlis de Vasconcelos³ (15-32), and offers his own explanation, a revival of the neglected one of Otto, Leite de Vasconcelos, and F. A. Coelho (33-47).⁴

¹ Unity of the Indo-European ablaut system: The dissyllabic roots, *Lg.* 23.1-22; The Romance conjugation in *-ēscō (-iscō) -īre*, *Lg.* 27.136-45.

² This theory is accepted e.g. by E. B. Williams, *From Latin to Portuguese* §158.2 (Philadelphia, 1938), and by J. J. Nunes, *Compêndio de gramática histórica portuguesa*² 314 (Lisbon, 1930; also 3d ed. 316, 1945); but not, apparently, by W. J. Entwistle, *The Spanish language* 59-60 and 290 (London, 1948).

³ José Maria Rodrigues, O imperfeito do conjuntivo e o infinito pessoal no português, *Boletim da segunda classe, Academia das ciências de Lisboa* 8.72-93 (1913-14); Sobre o uso do infinito impessoal e do pessoal em *Os Lusíadas*, *Boletim de filologia* 1.3-7, 177-84, 2.1-2 (1932-3). Carolina Michaëlis de Vasconcelos, O imperfeito do conjuntivo e o infinito pessoal, *Boletim da segunda classe* 12.289-92, 312-31 (1917-18), accepts and expands the theory of Rodrigues; for an older, abandoned view of hers, see *Romanische Forschungen* 7.49-122 (1893).

⁴ Richard Otto, Der portugiesische Infinitiv bei Camões, *Romanische Forschungen* 6.299-398 (1891); José Leite de Vasconcelos, *Estudos de filologia mirandesa* 1.374 (Lisbon, 1900); Francisco Adolfo Coelho, *Círculo camoneano* 2.201-4. Completely ignored by Maurer are the following studies: Franz Sester, *Der Infinitiv im Neuportugiesischen auf Grund der Werke von Eça de Queiroz* (Köln, 1928); Ernst Zellmer, *Über Gebrauch und Ursprung des konjugierten Infinitivs im älteren Gallego-Portugiesischen, and Geschichte des konjugierten Infinitivs im älteren Portugiesisch: II. Os Cancioneiros*, Gil Vicente, Camões, Fernão Lopes (both Pössneck, 1939); Rodrigo de Sá Nogueira, O problema do infinito pessoal em português, *Língua portuguesa* 5.7-15 (1938); Holger Sten, Les particularités de la langue portugaise, *Travaux du Cercle linguistique de Copenhague* 2.5-77 (1944); Hans Flasche, Der persönliche Infinitiv im klassischen Portugiesisch (António Vieira: 1608-1697), *Romanische Forschungen* 60.685-718 (1947); Harri Meier, A génese do infinito flexionado português, *Boletim de filologia* 11.115-32 (Lisbon, 1950). Also to be consulted are three reviews of Zellmer's studies, by W. J. Entwistle, *HR* 7.264; by G. Rohlf, *Archiv St. NSL* 175.141-2; and by F. Lecoy, *Romania* 65.429.

Portuguese differs from the other Romance Languages in having in its verb system a full tense which they lack, called the personal, or inflected, infinitive. The forms are: (*eu*) *amar*, (*tu*) *amares*, (*êle*) *amar*, (*nós*) *amarmos*, (*vós*) *amardes*, (*êles*) *amarem*. Portuguese also possesses, of course, the normal or impersonal infinitive without personal endings, common to the Romance languages. The problem is to discover the historical origin of the former.

Old Portuguese documents, from the beginnings in the 12th century, show a considerable number of forms of the type *amarmos*, *sahirem*, *venderdes*, (*êle*) *ser*. Rodrigues would divide these into two groups, of which one consists of personal infinitives, the other of imperfect subjunctives. According to him, the Latin imperfect subjunctive survived in Portuguese (*amārem* > *amar*, *amāres* > *amares*, *amāret* > *amar*, etc.), and is still used as an imperfect subjunctive as late as the 15th century, as is shown by its use as the verb in one of two or more parallel subordinate clauses, in the other of which the verb is the undisputed Portuguese imperfect subjunctive (from the Lat. pluperfect subjunctive: *amavissem* > *amasse*, *amavisses* > *amasses*, etc.). From this use, by certain syntactical changes, the personal infinitive developed.

According to Otto, Leite, and Coelho, on the other hand, the inflected infinitive evolved by the analogical addition to the normal infinitive of the set of endings: 1 zero, 2 *-es*, 3 zero, 4 *-mos*, 5 *-des*, 6 *-em*. Since these are precisely the endings of the Portuguese future subjunctive, that is the presumptive source of the borrowing, though some of the endings could have been borrowed from other tenses.

If all these examples are to be divided into two groups, obviously the division cannot be made on the basis of form alone, since the examples exhibit no differences of form, even in the strong verbs. Clearly, syntactical or functional criteria must be used. Some examples given by Rodrigues and discussed by Maurer are:

Mandou elRei em seu testamento que lhe tevessem em cada huum ano pra sempre no dito mosteiro seis capellaens, que cantassem [impf. subj.] por el e lhe dissessem [impf. subj.] cada dia huuma missa oficiada e sahirem [impf. subj. or pers. infin ?] sobrel com cruz e augua beemta 'The king ordered in his will that they should have for him in each year forever in the said monastery six chaplains who should sing for him and who should say for him each day an officiated mass and who should go out (and to go out) over him with cross and holy water', and: *ElRei . . . emviou Gomçallo Vaasquez Dazevedo, seu grande privado, que se fosse [impf. subj.] para elles, e seer [impf. subj. or pers. inf. ?] de companhia em aquella obra* 'The king . . . sent G. V. d'Azevedo, his great favorite, who should go to them, and should be (to be) of company in that work'. (Both examples from the chronicler Fernão Lopes, 15th century.)

The argument turns on the identification of the forms *sahirem* and *seer* in these, and similar forms in similar examples. Rodrigues identifies them as imperfect subjunctives, because they are the verbs in subordinate clauses parallel with other subordinate clauses in which the verbs are clearly imperfect subjunctives. If these are indeed survivals of the Latin imperfect subjunctive forms, still being used as imperfect subjunctives, the hypothesis of Rodrigues has much to recom-

mend it. Against this view, however, Maurer advances three telling arguments. (1) That they appear to be coordinate with Portuguese imperfect subjunctives is explainable as a stylistic device similar to anacoluthon; they should rather, he says, be identified as simply so many personal infinitives, and be included in the group of examples for which we seek an explanation.⁵ (2) Such examples are exceedingly rare in Old Portuguese outside the writings of the chroniclers of the 15th and 16th centuries. (3) Finally, these forms are never found in any of the other types of construction in which they might be expected to substitute for the *amasse* forms (e.g. the type *êle queria que ela o amasse* 'he wanted her to love him'), but, on the contrary, are widely and frequently found replacing the normal (impersonal) infinitive.⁶

In order to demonstrate that grammatical coordination is not proof of identity of function (a point which in the reviewer's opinion did not need proving),⁷ Maurer cites several examples of 'coordination of impersonal and personal infinitives', e.g. *devem nos⁸ reteer em seus corações, e pensarem que . . .* 'they must keep us in their hearts, and think that . . .', *Devemos dandar no sseu caminho e nũca nos departirmos do sseu ensinamento . . . e cõ paciência e humildade ssoportar e soffrer os padçimentos e as iniurias* 'We must walk in his way and never depart from his teaching . . . and with patience and humility endure and suffer sufferings and injuries'.⁹ On this point, it is worth while to cite the penetrating suggestion of the late Aubrey F. G. Bell. Writing of Dom Duarte (1391-1438), he says: 'His modern editor, José Ignacio Roquette . . . comments . . . on the passage *he bem de lavar e criarem* [it is well to work and create] as a great grammatical *discordancia* and *erro*, but it is by no means certain that King Duarte did not omit one of the personal infinitives deliberately, for the sake of euphony, as

⁵ Insufficient attention is here paid, it seems to the reviewer, to the extent to which the chroniclers were following, or thought they were following, the usages of Latin syntax. The style of the chroniclers seems to me highly latinized and artificial. One great weakness of the Rodrigues theory is that it begins with the 15th century, when it should begin with the 12th.

⁶ An example which Maurer might have cited here is *e piedossa coussa era alegrarensse com tam grande santo* from a document dated 1417, *Portugaliae monumenta historica: I. Scriptores* 78, col. 1.

⁷ As another example of such coordination, Maurer quotes from João de Barros: *Saltou todo o gentio da cidade com o favor dos mouros e mataram Aires Corrêa* 'All the gentility [i.e. group of gentiles] sallied from the city with the favor of the moors and killed Aires Corrêa', and from Padre Figueiredo's translation of the Bible: *Quando o teu povo sair a guerra . . . , se te fezerem as suas preces* 'When thy people go out to war . . . if they make their prayers to thee'. Instead of 'coordination of discordant elements', the reviewer had rather these were classified as examples of the familiar confusion caused by the ambivalent number relationships of collective nouns like *gentio* and *povo*, and as such removed from this discussion.

⁸ For clarity I have introduced a space between *devem* and *nos*.

⁹ Maurer makes the absence of ending on *reteer* and *andar* in these examples dependent on the near presence of the finite modal auxiliary. It is true that Old Portuguese does not (though modern popular and dialectal Portuguese does) permit the personal infinitive closely following a modal auxiliary, but many examples could have been cited in which this same coordination of the two infinitives occurs with no modal auxiliary to complicate it, e.g. the citation from Dom Duarte (apud Bell) which immediately follows in the text above.

the *-mente* is omitted in the case of two or more adverbs.¹⁰ It is true that Bell's suggestion is made from the literary, stylistic point of view, and is intended, ostensibly, to apply to only the one instance of such coordination; but why should we not generalize it to apply to all such cases? If we do, many forms heretofore classified as impersonal infinitives must now be classified as personal infinitives which have had their endings suppressed. This seems quite consistent with the theory of Otto and Leite, supported by Maurer, that the endings of the personal infinitive are relatively late analogical additions to these forms.¹¹ As such, they would be likely to be less fixed in usage, and more readily omitted or suppressed than other endings; that they are, is borne out by the almost absolute freedom of interchangeability of personal and impersonal infinitive in several types of construction in Old Portuguese (and even, though less freely, in the modern language), or, to put it differently, by the free variation between presence and absence of ending on the infinitive in certain constructions.

The remainder of the essay is weak. Maurer restates the theory of Otto and Leite, with emphasis on the matter of the nominative subject, and with some obiter dicta on the infinitive with nominative subject in Spanish and the other Romance languages, which, while they seem true enough, do not materially strengthen the hypothesis. The effect of the conclusion is also rather spoiled by occasional unfortunate excursions into normative grammar (34, 37). What is needed here is a discussion which would take as its starting point the hypothesis that the personal infinitive arose by the analogical addition of endings to the impersonal infinitive, and which would proceed to document this hypothesis with copious examples tending to show in what types of construction the personal infinitive got its start, how it is used in the oldest surviving documents, and how it has spread, if at all, to other types of construction in more modern times. The reviewer has undertaken such a study, and hopes to publish it in the near future.

The second essay, on the pronoun *se*, appears, if one may read between the lines, to have had its origin in a difference of opinion between the author and Manuel Said Ali Ida, professor in the Escola do Estado Maior in Rio. In such a phrase as *compra-se o palácio*, Said Ali (apud Maurer) would analyze *se* as subject. Reacting vigorously against this view, the author sees this as an impersonal expression which has evolved from a passive expression. To the reviewer, there is no necessary conflict between these views, and both are probably correct. Conflict between them can only be generated by mixing synchronic and diachronic considerations, a notably dangerous practice. However this may be, the author proposes as his view that *se* in such a phrase is a 'morpheme of impersonality' which 'attributes a special sense' to the theme to which it is attached, a function similar to that of a suffix, e.g. *-dor*. His term for this morpheme is 'particle of impersonalization'.

¹⁰ Aubrey F. G. Bell, *Portuguese literature* 91 note 2 (Oxford, 1922).

¹¹ As Carolina Michaëlis de Vasconcelos has pointed out, the personal infinitive forms are in full vigor when the preserved documents begin at the end of the 12th century. If they were formed by analogical addition of endings, the date is pure speculation, say 8th to 11th century. The Latin forms from which Rodrigues derives them were, of course, established a millennium or more earlier.

These considerations are followed by a long digression on 'the common middle-passive of Indo-European' (55-62), and 'the middle-passive voice in -r' (62-70). This digression is studded with citations and arguments from nearly two dozen languages, ranging geographically from Irish to Sanskrit and Hebrew. The reviewer hastens to disclaim any competence to discuss the majority of these. What is clear is that the author feels that in some of these languages a middle (or passive) construction has become impersonal, that is, has taken on impersonal meaning, in a manner similar to that in which the Romance reflexive has become impersonal (e.g. Ptg. *mata-se o homem* leads to *compra-se o palácio*). In respect to this digression, the reviewer can make only one observation. Granted that all these things are so, it is still difficult to see the application of the analogy. It is difficult to discover the organic relationship which the author wishes to establish between these two widely separated sets of events. The author himself seems to sense this when he says (Conclusion, 70): 'We shall not say that Portuguese, or the neo-Latin languages in general, have reproduced the Indo-European process by a species of mysterious atavism. What we have . . . is an example of a normal psychological function of human language, and [one] which, for that very reason [i.e. its normality] frequently occurs, giving rise to similar semantic types in epochs and regions often entirely distinct.' For this reviewer, the analogy is not cogent.

Os animais na linguagem portuguesa. By DELMIRA MAÇÃS. (Publicações do Centro de Estudos Filológicos, No. 2.) Pp. 431. Lisboa: Centro de Estudos Filológicos, 1951.

Reviewed by JOSEPH H. D. ALLEN JR., *University of Illinois*

This book was the author's thesis for the licenciatura (she does not say in which university). One must say immediately that she has collected tremendous numbers of examples and has displayed commendable energy and industry in arranging and presenting them. That these admirable qualities have been largely misdirected is due to several fundamental errors of conception and methodology.

In deciding to define 'animal' to include everything which is not vegetable or mineral, she undertook a task of a scope sufficient to tax the abilities of a much more mature scholar. When she decided to treat not only the literary-critical aspects of her subject (animals appearing as personages in fiction,¹ as symbols in poetry, etc., 337-63) and the folkloric aspects (proverbs, rimes, children's games, riddles, popular medicine and cures, superstitions, hexes and curses, etc., 231-337), but also the linguistic aspects (etymology, historical phonology, transfers of meaning, tabu, borrowings from other languages, derivation, composition, popular etymology, onomatopoeia, child language, slang, etc., 9-229), the author made herself responsible for working in three highly specialized fields of scholarship (not counting zoology), without, apparently, recognizing very clearly the degree of specialization which the reader would

¹ It is too bad that the discussion of animals which have achieved names and personalities of their own omits Japí, the dog in Alencar's *Iracema*, and Quincas Borba, the dog in Machado de Assis' novel of the same name.

feel entitled to expect in each. To undertake a work on so vast a scale makes a summary treatment almost inevitable; the author recognizes this when she says (Preface, vii): 'I have not claimed to exhaust the subject ... and I have collected and used that which seemed to me most significant.' This procedure is not in itself indefensible, and the reader may suppose that the samplings from Portugal are roughly equivalent in their several categories. The decision to include items from Brazil, however, throws this plan gravely out of balance. The reviewer knows of no reliable statistics on the subject, but the total number of items available from that vast territory of exuberant fauna and complex culture must surely exceed those from Portugal by a factor of at least several hundred percent. Yet the Brazilian examples adduced are few and scattered, and do not always attest to intimate knowledge of the country.²

The necessary result of this all-inclusive approach is that the great bulk of the author's material has had to be taken from secondary sources, which, of course, she freely acknowledges in her extensive and valuable bibliography (365-87). To be sure, a few examples are from novels and newspapers, and she reports some material from the Lisbon telephone directory³ and the postal guide. Her principal source, however, seems to have been the dictionary of Cândido de Figueiredo. Her historical linguistic treatments come principally from Meyer-Lübke, Bloch, von Wartburg, Antenor Nascentes, M. L. Wagner, and other scholars. Even her folkloric material is often a reprint of matter already published by Leite de Vasconcelos and other investigators in the *Revista lusitana* and elsewhere.

Setting aside the literary and folkloristic sections, readers of this review will be most interested in the linguistic aspects of her subject. It must be admitted that her treatment of these leaves something to be desired. The basis of subdivision of much of her material is 'logical' rather than linguistic, which is unfortunate from our point of view because it often separates linguistically related items. Thus, we should have been pleased to see the examples *aleonado*, *atigrado* (89), *alagartado*, *alagostado* (and several others, 152), *acabritado*, *ajumentado* (and others, 178), and *agorafado* (205), brought together and discussed as formations of the *amulatado* type.⁴

² Thus, a South American (presumably Brazilian) habitat is ascribed to the buffalo and the bison. *Cachorro quente* 'hot dog' (in Brazil) is obviously a translation of the American English term, not a Portuguese formation. Discussing animals whose names are used to indicate 'perversity, treachery, etc.', the author includes *amigo da onça*, on an equal footing with her other examples. This is to ignore the story, known to every Brazilian, from which the phrase derives: A man, armed to the teeth, goes to hunt the *onça* (jaguar). His friend would dissuade him: the *onça* is dangerous, his rifle may miss, his revolver jam, his knife fall from its sheath. Exasperated, the hunter exclaims: 'Are you a friend of mine or of the *onça*?' It is thus the *amigo*, not the *onça*, who is treacherous. Again, *cabrada* 'police' (in Brazil) is only indirectly, if at all, 'a collection of goats', as the author's treatment implies. The development seems to be from *cabra* 'goat' to 'mulatto ($\frac{3}{4}$ negro, $\frac{1}{4}$ white)' to 'hired hand, field hand' to 'bodyguard' (so used in northeastern Brazil by Jorge Amado and other writers of the cacao wars) to 'policeman'; *cabrada* 'police' is then formed on the last of these.

³ The advisability of including the family name *Canário* from the directory seems arguable. The name almost certainly comes from that of the islands, from which comes also the name of the bird.

⁴ Cf. Yakov Malkiel, The *amulatado* type in Spanish, *Romanic review* 32.278-95 (1941).

Her remarks on gender show a regrettable tendency to confuse distinct questions. Thus she appears to recognize (95-9) that some of her examples illustrate the *femininum augmentativum*,⁵ but there is evidence of considerable confusion among sex, gender, and word class in her remarks on *lebre* 'hare', and in her statements: '*Rato*, masculine in Latin as in Greek ... (is masculine) also in the Romance languages, except French *souris*', and: '*Pato* [duck] is feminine in German'. There are several different problems here. That of the gender of the Portuguese word itself is an historical one. Another is the problem of which of several terms (those for the male, the female, and the young) the speakers of the language use to designate the species as a whole. (So, in English, we raise chickens, ducks, cows, and horses, not hens or cocks, drakes, bulls, or mares.) The choices made among such words by Portuguese speakers are of the greatest interest; that German speakers use totally different words, which also happen to belong to different word classes, seems devoid of significance, unless one wishes to enter the realm of popular psychology. The question of *lebre* is quite distinct; it is related to the fluctuating gender relationships of the Romance descendants of Latin nouns of the third declension, and of words which were or have come to be like them (e.g. *drvore*, *dente*, *febre*, *fonte*, with their cognates in the other Romance languages). One feels that the author is aware that these are separate problems, but their distinctness is gravely blurred in her presentation.

At times, the author appears to forget that the subject matter of linguistics is the spoken language. Thus she makes two lists of compound words (18-21); the items of one part of the first list (e.g. *luze-cu*, *mija-fogo*) are distinguished from those of the corresponding part of the second list, called 'aglutinados' (e.g. *abrecu*, *alçacu*, *caçalume*), uniquely by the circumstance that the former are written with hyphens, the latter without. The basis of this distinction is unacceptable.

Minor slips and errors aside,⁶ it must be admitted that the author has accomplished a vast labor. One can only be sorry that she has been overwhelmed by a mass of material beyond her capacity to manage, quite possibly (if Brazil be included) beyond the capacity of any living scholar. One wonders whether the responsibility for this can properly be laid upon the author alone. Since the work was a thesis, some share of the responsibility may fall upon the one (unidentified in the book) who helped her to choose the topic, and who guided the investigation. It seems clear that a much more fruitful and useful study could have been conducted by limiting the subject in either of two ways. One could have followed the example of Woodard⁷ by limiting it to consideration of a

⁵ Cf. Henry and Renée Kahane, The augmentative feminine in the Romance languages, *Romance philology* 2.135-75 (1948/9).

⁶ For instance: *mosca* 'glass of muscatel brandy' is surely a shortened form of *moscatel*, unconnected with *mosca* 'fly'; *tsé-tsé* 'African fly' is not a 'Portuguese creation of almost pure onomatopoeia', but a borrowing from Sechwana. It is saddening to see *brekekem*, *hoax*, *hoax* (sic) discussed without reference to Aristophanes. Some linguists will be surprised to see *ariano* 'Aryan' used as a technical term, apparently to mean Indo-European or possibly Sanskrit. One hopes that in the phrase *raça canina dos Montes de São Bernardo*, the word *Montes* is a printer's error for *Monges* (i.e. 'monks', not 'mountains').

⁷ Clement Manly Woodard, *Words for horse in French and Provençal: A study in dialectology* (Language dissertation No. 29, 1939).

single animal and making the investigation reasonably complete throughout Portuguese literature, or one could have done field work in a single village or *concelho* of Portugal on animals in general (possibly omitting insects). The author has shown that she has talent, energy, industry, and a love for the subject. It is to be hoped that now, with this experience behind her, she will find it possible to work more intensively in a more restricted field.

Det isländska accenttecknet: En historisk-ortografisk studie. By GUSTAV LINDBLAD. (Lunds Universitets Årsskrift, NF, Avd. 1, Bd. 48, Nr. 1.) Pp. 230. Lund: C. W. K. Gleerup, 1952.

Reviewed by KEMP MALONE, *Johns Hopkins University*

The study under review is solidly based. It rests upon an examination of 60 Icelandic manuscripts of the Middle Ages, beginning with the oldest codices that have come down to us, those of the 12th century, and ending with a codex of the early 16th century. In the fourth chapter of his book the author takes up these MSS in detail. From some he has excerpted all the words that have accent marks; from others, all such words in specified parts of the text. Each MS, and each hand within the MS, is taken up separately, in an effort to determine how the accents are used by the various scribes. The evidence for a given MS or hand is presented and analyzed systematically, and a number of tables are provided, some incorporated in the discussion, the rest put in an appendix at the end. Unhappily nearly all the MSS in the main table, No. 15, are given numbers different from those used in Chapter 4. This feature of the book has put the reviewer to a lot of trouble, and will make trouble for all serious users of the work. But we must be thankful for the feast that the author has laid before us, and not grumble at his way of serving it up.

The first three chapters make an introduction to the investigation proper. In the first (7-13) we are given a very brief account of earlier work on the functions of the accent marks in Old Icelandic; this is followed by a summary sketch of the author's own procedure and conclusions. In Chapter 2 (14-21) the history of the acute accent in classical and early medieval times is sketched, and more is said about the work done by the author's predecessors Holtsmark, Wadstein, and Flom, work touched on in the first chapter. Chapter 3 (22-7) tells us how the Icelandic scribes made the acute, what the mark looks like, and where it may be set (not necessarily just above its letter). The four plates provided at the end of the book help the reader to follow the descriptions given in this chapter.

Chapter 4 is the heart of the book, as we have seen; it takes up 105 pages. Chapter 5 (133-61), entitled Functions of the Acute Accent, gives the author's conclusions. These do not differ much from those reached by earlier students of the subject, but they are more sharply defined and are backed by far more evidence than has ever before been gathered. The length-marking function was the main thing in the earlier period, but toward the end of the Middle Ages a graphic function, that of marking *i* as such, became the main thing. Here the acute was later replaced by the dot, a device still in general use. But the acute was also

used to mark other letters as such, especially *y*. In addition, the acute might be used to mark the main stress in a word group (phrase, clause, or sentence). In some MSS the acute has yet another function, that of marking quality rather than quantity: the letter over which it is set has a value qualitatively different from the value it has when left unmarked. Finally, the acute may mark dieresis.

In Chapter 6 (162-75) the author tries to trace the origins of the functions described in the preceding chapter, taking up Norway and England in particular. Since however no investigation comparable to his own has been made of Norwegian and English MSS, what he says on the subject is suggestive rather than conclusive. He rightly lays great stress on the influence of the so-called First Grammatical Treatise on the development of the Icelandic spelling system. See Einar Haugen, *Language monograph No. 25* (1950).

In Chapter 7 (176-95) the evidence of the accent marks is applied to certain problems in the history of Icelandic pronunciation; in Chapter 8 (196-203) the fortunes of the marks in modern Icelandic are traced. The work is concluded with an English summary (204-13), the English of which might be better. By way of appendix we are given the tables and plates mentioned above, and a seven-page bibliography. The whole makes a distinguished piece of work, on which the author is to be congratulated.

Kölner Beinamen des 12. und 13. Jahrhunderts, I. By STEN HAGSTRÖM. (*Nomina germanica*: Arkiv för germansk namnforskning, Vol. 8.) Pp. xxx, 482. Uppsala: Appelbergs Boktryckeri AB, 1949.

Reviewed by OTTO SPRINGER, *University of Pennsylvania*

The formidable volume under review is only the first installment of a larger work dealing with the bynames current in Cologne during the 12th and 13th centuries. The reason for singling out the medieval city of Cologne for such a minute and comprehensive scrutiny is obvious to any student of onomastics and of surnames in particular. By the end of the 11th century, the old ecclesiastic and political metropolis on the lower Rhine had surpassed its competitor, the city of Mainz, as a commercial center, and retained its position throughout the later Middle Ages down to the present day.

The importance of the city is reflected in the wealth of documentary material preserved from medieval times. Indeed, Cologne seems to have been the first community on German soil to develop an orderly and large-scale system of recording legal transactions of all sorts, especially transfers of real estate, donations, and bequests made by some of its 30,000 inhabitants. These historical records, known as *Schreinsurkunden*, are to a large extent published in reasonably accurate editions and contain thousands of personal names; they form the basis of the present study.

In twelve brief chapters (1-59) the reader is introduced to some of the methodological problems concerning the investigation of bynames, such as their precise definition, their origin, and their four main types: bynames derived from bodily or mental and moral characteristics (*Kurremmurre* 'Grumpy'), occupational bynames (*Lînwâtmengere* 'Linen-monger'), bynames derived from older given

names (*Erenfrid*), and bynames taken from the place of origin or residence (*Basilerus*). Other questions of a general sort are the form in which these names occur in our historical records, the special types applied to men and women respectively, their frequent rendering in Latin, their development to inheritable family names. According to Hagström, the main reasons for the creation of bynames and also for their ultimate establishment as surnames are, in addition to man's everlasting desire to tease and to label his fellowman, the rapidly growing need for differentiation in the crowded urban centers emerging at the end of the Middle Ages and, making the confusion worse, the popularity of a relatively small number of Christian names. Thus we know, though Hagström fails to mention this, that according to the records of the 12th century there were in Cologne at one time as many as 823 persons called *Heinrich*, 639 *Herman*, 497 *Dietrich*, 460 *Gerhart*, and 369 *Conrat*. In fact, Hagström himself (12, note) lists two brothers who were both called *Gerardus* and two others called *Henricus*, an onomastic curiosity not infrequent during the Middle Ages. A special type of byname applied to women was formed in *-se*, which, according to the Cologne records, produced among others such monsters as *Cleidirriszirse* 'clothes-dealer f.' (333), *Neylmengersa* 'nail-monger f.' (355), *Gulstlegerssa* 'gold-beater f.' (315), and *Bessemridersse* 'she who rides on a broom(stick), witch' (70), the last being the earliest recorded evidence in Germany of the popular belief involved. Another factor contributing to the rise of surnames is mentioned by the author only in passing: imitation of foreign customs, especially French and Italian. For some of the more basic linguistic problems, such as common noun vs. proper name (Bloomfield, *Language* 205), he refers to the systematic discussions of onomastic principles by Adolf Bach and others.

After the introductory sections come the first and second main chapters, listing in alphabetical order bynames derived from physical or mental and moral traits or, even more frequently, defects (60-298), and bynames of occupation (299-406). The third and fourth types are reserved for the second volume.

Under each byname the author first presents all recorded occurrences in chronological sequence, and then gives linguistic and, whenever necessary, social explanations, supplemented by onomastic parallels and careful cross references to other parts of the book. For the linguistic discussion not only the standard MHG and MLG reference works have been consulted but also most of the pertinent modern dialect dictionaries. In many instances the Latin rendering of a byname suggests the correct or at any rate the contemporary interpretation, as for *Surbier* (260) : *Amara-cerevisia* (62), *Bligiserre* (305) : *Plumbi-fusor* (361) or *Funditor-* or *Fusor-plumbi* (395), *Vudermengere* (394) : *Pabularius* or *Pabulorum-venditor* (357), *Hûnremengere* (319) : *qui vendit Pullos* (363). The cognomen *Gir* (101) is translated as *Avarus* (64, MHG *gir*), but also as *Vultur* (101, MHG *gir*); the name *Cleinededanc* (141 f.) appears as *Parva-mens*, *Parvus-animus* (201), but also, more flatteringly, as *Subtili-mente* (258), in accordance with the two meanings of MHG *klein*.

While for the linguistic analysis of his onomastic material the author thus leans heavily on the dictionaries at his disposal, he is able, in return, to enrich our lexicographical knowledge of medieval German and, in a measure, of medieval

Latin by a respectable number of formations hitherto totally unknown or attested only for considerably later dates.

As to medieval German, most of the newcomers are compounds and derivatives, such as *Armwortere* (301) 'crossbow-maker', probably a Klammerform for **arm(brust)worhtære*; *Blozmudir* (75); *Geverere* (100, from MHG *geværen* 'to deceive'); *Husigil* (121); *Kerzworther* (329) 'candle-maker'; *Kluppilschit* (146); *Cumpostere* (340, from MHG *kumpost* 'preserves'); *Manichwer* (175); *Morgensweiz* (187 f.); *Ritscalch* (365) 'Reitknecht, groom'. For medieval Latin the following formations are new: *Acetatrix* (300) 'vinegar-vendor f.'; *Arvinarius* (301) 'ointment-dealer'; *Balbutizans* (64) 'stammerer', probably a blend of *balbutire* and *balbuzare*; *Barbitonsor* (302) 'beard-shearer'; *Dormitorius* (309, cf. *qui custodit Dormitorium* *ibid.*); *Galeator* (311) 'helmet-smith', cf. *Helmslegere* (317); *Herbator* (317); *Hostiatrix* (318); *Caldeator*, -*trix* (322); *Candelator* (325, cf. *Kerzworther*); *Caseator* (326 f., cf. *Keseman* 329), *Cindator* (330) 'silk-weaver'; *Pannator* (358) 'cloth-maker'; *Specionarius* (377) 'spicer'; *Vestimentarius* (391) 'clothier'.

Still larger is the number of lexical items which in Hagström's onomastic material occur two or even three centuries earlier than in the highly selective vocabulary of our literary sources. Thus NHG *Bierbauch* of no uncertain connotation can now, thanks to Hagström and contrary to the meager entry in *Dt. Wb. I* (1823), boast of a lineage as ancient as 1169: *Hermannus quidam agnomine Birbuc* (71). Similarly *Gernegroiz* (100) is now attested for 1236 [1333];¹ *gremde* (104) 'annoyance' for 1189 [14th century]; the much-debated MHG *gollicht* 'tallow' for 1259 [about 1300], as byname of one Gerardus, no doubt denoting 'candle-maker', as is evident from a passage referring to the same individual: *domus que quondam fuit Gerardi Kerzwortere* (108); *Kid(d)el* (137) for 1135-80 [13th century]; *Coppotichin* (151), diminutive of *koppūt* 'carp', for 1205-14 [1482]; *Cobolt* (152) for 1135-42 [13th century]; *Kudirwale* (158) for 1247 [1379 *Khawderwalch*] (incidentally Hagström offers additional support for deriving German *Kauderwelsch* from *kaudern* 'to chatter'); *Mostart* (189) for 1200 [1339]; *Moster* (354) 'cider-maker' for 1135-80 [1361]; *Ploc* (207), MHG *pfloc*, for 1187-1200 [1350]; *Rump* (221), MHG *rumpf*, for 1139-52 [13th century]; *Sentscheffe* (371) for 1248 [15th century]; *Spac* (248), MHG *spach* 'dry' for 1170-90 [1477]; *Fazbendere* (388) for 1135-80 [1310]. The byname *Belster* (304) confirms OHG *balastar* (not *balaster* as quoted by Hagström), known only from the glosses (Graff 3.102) and no doubt derived from Lat. *balistarius* 'thrower of projectiles, artilleryman'.

Of special interest are also the many formations in *-menger(e)* (ModE *-monger*) and in *-wor(h)te(re)* (ModE *-wright*), the use of abstracts—*bôsheit*, *blîdeschaf(t)*, *êre*, *herschaf(t)*—as cognomens, and the long list of phrase names, such as *Bintup* 'Tie-up' (69), *Brechscacht* 'Break-shaft' (79), *Brockin* 'Dip-in' (80), *Brüwel* 'Scald-skin' (81), *Haveneit* 'Have-nothing' (113), *Heldecruche* 'Lower-pitcher' (114), *Idelspille* 'Empty-spindle' (122), *Koternasen* 'Dig-in-nose' (151 f.), *Cras-mule* 'Scratch-mouth' (154), *Rovetasge* 'Pick-pocket' (220), *Scerbardo* 'Shear-

¹ In this paragraph, dates in square brackets are those of the earliest occurrences previously recorded.

beard' (233), *Chouchelm* 'Shake-helmet' (240), *Spaltbone* 'Split-bean' (249), *Vilhunt* 'Skin-dog' (269), *Wrenkehals* 'Wrench-neck' (281), *Zwengenal* 'Pinch-nail' (289).

As we would expect of a study coming from the Uppsala school of onomastic research, Hagström's analysis of the names is linguistically sound, cautious, and critical. One of the few exceptions is his tendency to operate with hypothetical nomina agentis of the type **brûche* (80) from *brûchen* with the supposed meaning of 'user, waster (?)' (more probably derived from a topographical designation), **brûse* (81) 'Brauser', **schicke* (236) 'der ordnet, schafft', **smeiche* (245) 'flatterer', *Stroiff* from **strouffe* 'skinner' (379), **waie* (278) 'who comes blowing like a wind', and *Cure* (341) 'chooser, tester'. By the same token, Hagström's analysis of *Scheinth* in *Theod. Scheinth vel Lignum* (232) as a nomen agentis from *schenden*, meaning 'he who puts others to shame, bailiff', is impossible because of the spelling *-th-*, the apocope of final *-e* as early as 1266, and the absence of any historical evidence in Germanic of a formation **skandja* or the like. On the other hand, identification with MHG *schît* 'Scheit', as suggested by the Latin rendering *Lignum*, would presuppose very early diphthongization, unparalleled in native words of the area.

Very improbable is also Hagström's identification of the cognomen *Scindat* (236) with MHG *schintât* 'evidentia facti'. I should rather connect the name with MHG *zindât* (*zindâl*) 'silk', from Latin *cindatum* (*scindatum*), and suspect an indirect name of occupation similar to the direct occupational cognomen of one *Cindator*, *Scindator* mentioned on 330 and living in a house *in qua paratur scindatum*. The obscure name *Sulf* can hardly be derived in form or meaning from MHG *selp*. Nor is there any need for resorting to a 'shifted form' **blutz* (for *blutt*) 'bare' compounded with *hât* 'hide, skin' to explain the byname *Blûtsût* (76); no doubt it corresponds to MHG *bluotsuht* (as mentioned by the author), with the loss of *-h-* before *t* characteristic of the speech of the Cologne area, cf. *-wor(e)te* for *-worhte(re)* and *-kneyt* for *-kneht* in Hagström's own material.

On the other hand, whenever the author is at a complete loss regarding the explanation of a name, he does not hesitate to say so—thus in the case of *Begere* (66), *Berluit* (69), *Bollartz* (77), *Hovekit* (119), *Clucinck* (145), *Moswin* (188), *Profleit* (210), *Slenbo* (243), *Bicolnus* (304), *Sargyfer* (369). In many more instances, however, the difficulty lies in an embarras de richesses, in the multiplicity of competing possible explanations. These are conscientiously registered under the label *Konk(urrenzen)*.

For the sake of confirming the linguistic analysis Hagström frequently goes out of his way to cite onomastic parallels from similar collections, whether German, Scandinavian, or (Old) English (e.g. *Vinnoc* 270 'Fein-genug' : *Good-enough*). Here the American reader is often tempted to add some of his own from the rich treasure of American sobriquets, old and new. So when the author hesitates to accept the many bynames derived from words for 'stump, trunk', such as *Bloch(man)*, *Knorre*, *Stock(elin)*, *Stolle*, *Stempfel*, *Stumpard*, as denoting heavy-built, broad-shouldered, sometimes short individuals, we think immediately of *Stumpy*, *Stubby*, and their confreres in modern parlance. For the Cologne citizen with the somewhat unusual epithet *Hirne* (116) we should like

to remind the author of our *Old Brains* (General Halleck of Civil War fame). His medieval German *Bone* (78) we can counter with *Beanpole Jimmy*, his *Duvel* (93) with *Devil Pete* (John Peter Muhlenberg). Even his 'weather-names', eccentric as they may seem, such as *Hermannus Sconewedir* (239), we can outdo with our *Ain't Gonna Rain No More Man*, the old schoolmaster who always guaranteed fair weather for his boys' picnics.

There is a very detailed linguistic summary at the end of the book (406-82), in which Hagström tabulates the vowels and consonants of the recorded name forms with Middle High German as the frame of reference; he has also added some general observations on declensional forms, word derivation, and compounds. What is missing, in the volume before us, are certain general conclusions on such onomastic problems as the actual rise, decade by decade, of bynames and eventually surnames, the relative frequency of the different types of bynames, and the history and characteristics of bynames in Cologne as compared with other German cities during the corresponding period of time. We are told by others that about 1150 only 18% of the people of Cologne (that is, of those recorded) had bynames, while a century later there were 70-80%. Statements of this sort can no doubt be checked, and many other questions can be for the first time satisfactorily settled on the basis of Hagström's detailed and thoroughly reliable study. In the hope that the author will let us have his answers to some of these general questions in a closing chapter of the work, and that he may also undertake the labor of providing his study with a complete index, so indispensable in books of this kind, we look forward now to the publication of Volume 2.

French influence in English phrasing. By A. A. PRINS. Pp. vii, 320. Leiden: Universitaire Pers Leiden, 1952.

Reviewed by ARCHIBALD A. HILL, *Georgetown University*

Prins has compiled an extensive vocabulary of parallel word-sequences in English and French, gathered by combing such dictionaries as the NED and Littré, and supplemented by industrious reading of documents spread from *Ælfric's Lives of saints* to *The Forsyte saga*. The result is useful and impressive, since even the specialist is apt to be surprised by the closeness of familiar phrases like *after all* and *après tout*, or *in a book* and *en un livre*, where the Modern English is appreciably closer to the French than it is to Old English *on bēc*. Over and over again, Prins shows that there is a one-for-one translatability between the two languages. Most scholars would be prepared to accept such a statement on general terms, but the demonstration of it is none the less valuable.

The soundness of interpretation which Prins has given his materials, and indeed of the presuppositions with which he worked, is another question. There is no definition of 'phrase,' though scanning of his entries makes it clear that 'phrase' here means 'group of entities written as more than one word'. It is surely not too much to point out that in these days, at least, habits of writing are not the best way of distinguishing phrases from words. Prins treats *gentleman* as a phrase, since it was once written *gentle man*. Yet by any phonological test I know how to apply, *gentleman* is as much a single word as *elephant*, though its

etymology may be clearer. Further, I do not know how Prins selected those collocations of words which he decided to include. A very frequent phrase in English is *young man*, which is certainly parallel to French *jeune homme*. I am not arguing that *young man* should have been included, since I am well enough aware that to put it in would have let down the bars to hosts of commonplace collocations which would do no more than prove that English and French are both European languages. But just as when I study the fascinating pages of the NED, I wish I knew how things were left out.

Sometimes also, Prins relies on convenient formulations like French or English 'spirit', rather than on evidence. For instance, he says of the replacement of *make a visit* by *pay a visit*, that this 'substitution perhaps reflects the more commercial outlook of English life' (42). Or in discussing *fair sex* (129): 'The F. example is nine years later than the first E. entry, but this sort of phrase reflecting an attitude which is French rather than English, is no doubt of French origin. The English would hardly have made it up themselves.' If the study is to tell us anything of French and English attitudes as expressed in language, preconceived notions of these attitudes should not control the evidence.

Yet in spite of these faults in approach and method, it is a position taken in the introduction which is the weakest point in the book. Prins takes strong issue with the thesis of Chambers that English prose is continuous, and has this to say (12):

The continuity of the English language ... is a fallacy. Part of the vocabulary of Modern English is no doubt continuous from Anglo-Saxon to this day, but the number of foreign admixtures is so large and they are of such general application that Modern English may almost be called a Romance language as to its vocabulary ... [Foreign words have] completely altered the system of derivation and composition and broken up the connection between the various parts of speech. The adjective *initial* to the verb *begin* and its derivation *beginning* and adjective *verbal* to the noun *word* show what I am alluding to.

This is an astonishing position. A part of it was demolished long ago by writers like Emerson, Jespersen, and Baugh, who pointed out that a mere dictionary count was no way in which to measure the native elements of the lexicon against those of foreign origin. Words like *food*, *water*, *stone*, *man*, *woman*, *foot*, and *head* come up much more often in conversation than do *reason*, *power*, *quality*, or even *duke*, *royal*, and *army*. Another part of Prins' position is that because *initial* and *begin* are now unrelated, the language structure has been somehow changed. Rather, what has happened is that the addition of foreign words has created new word families, and somewhat modified old ones. When Prins says that the system of derivation is completely altered, he forgets sets of derivatives like *shmooish*, *shmooishness*, and *shmooishly*—no one of which I have ever heard, but all of which I would use freely, in strict accord with long established habits of derivation, and all of which would be understood by any American. When, further, he says that foreign vocabulary has altered the relation of the parts of speech, he forgets that English can treat a given morpheme or construction now as one part of speech, now another, with only the addition of the necessary gram-

matical elements, as in *I lit the fire* and *I fired the furnace*. We can treat his example *initial* in the same way: *These are my initials*, *I initialled the document*, *This is my initial effort*. So far from having broken up the relation of the parts of speech, what we have done is to take over foreign words and fit them into the English system.

The underlying attitude which explains Prins' denial of the continuity of English comes out most clearly in a passage found on the same page. Prins argues that adding a foreign word to English is like adding a drop of yellow to a large quantity of red paint. As more and more drops are added, the red is gradually modified, until at some point it is no longer the same. Such an attitude is to identify the English language altogether with its lexicon. Prins is shutting out all of phonemics, all of grammar. His position is as startling as if a student of singing were advised that he could learn a song by mastering the words, with no attention to the tune.

Our storehouse of Missouri place names. By ROBERT L. RAMSAY. (University of Missouri bulletin, Vol. 53, No. 34; Arts and sciences series, No. 7: Missouri handbook, No. 2.) Pp. 160. [Columbia, Mo.]: University of Missouri, 1952.

Reviewed by JAMES B. McMILLAN, *University of Alabama*

Ramsay reports that eighteen graduate theses on Missouri place names, covering all counties of the state, were produced at the University of Missouri between 1928 and 1945; that a card file of 32,324 names was prepared in 1945-47 and placed in the university library (with a duplicate in the office of the Board on Geographic Names in Washington); and that since 1947, additional names have been added to make a file of about 35,000 names. The commentary on each name includes whatever is known about its identification, derivation, and history, supported by dated documentation. Workers in onomastics will be forever indebted to Ramsay and his students for this corpus.

In the book here to be reviewed, Ramsay selects nearly 2,000 'important, typical, and representative names' for classification and discussion in popular style. The names are grouped according to source as borrowed, historical and political, local personal, topographical, and cultural names, and are subdivided by nonlinguistic criteria. For example, historical names are arranged in subgroups labeled Indian, French and Spanish, Presidential, Other National Leaders and Statesmen, Soldiers and Sailors, etc. The names in each subgroup are listed with dates, and the more interesting ones are discussed informally, with emphasis on the reasons for selection, spread, extinction, and survival.

Professional students will be most interested in Ramsay's description (128-43) of the projected dictionary of Missouri place names, including a display of 30 sample entries. It is his plan to reduce the 35,000 names to five or six thousand entries by eliminating most of the names of small or unimportant features (farms, springs, groves, caves) and by treating thousands of names in omnibus entries for such elements as *-burg* and *-ville*. The identification and classification of elements is etymological; thus, *-burg* and *-boro* are discussed together in the

sample entry for *-burg*. Pronunciation is indicated in IPA symbols, as modified by Kenyon and Knott; both phonemic and conspicuous subphonemic variants are included.

Future professional students will probably prefer a complete lexicon and a descriptive rather than an etymological classification. But Ramsay is faced with the practical problem of reducing a tremendous mass of data to publishable dimensions, and future students will always be able to go to the master files in Columbia and Washington for the total corpus, which has evidently been assembled with meticulous care.

Arabisches Wörterbuch für die Schriftsprache der Gegenwart. Unter Mitarbeit mehrerer Fachgenossen bearbeitet und herausgegeben. By HANS WEHR. Two volumes, pp. xi, 986. Leipzig: Otto Harrassowitz, 1952.

Reviewed by CHARLES A. FERGUSON, *Foreign Service Institute School, Beirut*

This is an excellent dictionary. It is much better than other existing dictionaries of so-called Modern Arabic—the form of Classical Arabic used today throughout the Arab world as a written language and as the language of formal speech. In spite of certain omissions and a few points of detail which may be criticized, it will be an invaluable tool for anyone concerned with Modern Arabic materials.

First of all, the background of linguistic sophistication is unparalleled in Arabic lexicography. In three pages of the Vorwort the author sets forth the present situation of the Arabic language and the difficulties of lexicographers in dealing with it. The exposition is remarkably clear and precise, and the techniques chosen by the author to deal with the problems are well devised. His major points are worth repeating here. I have numbered them, and list them, in condensed form and slightly paraphrased, in his order of presentation.

(1) The modern Arabic Schriftsprache, for which the term *neuhocharabisch* is suggested, is used from Iraq to Morocco for written purposes and orally on formal occasions, especially on the radio.

(2) This language differs very slightly in morphology and syntax in the various countries, but the vocabulary tends to show considerable variation, to be expected from the size of the area.

(3) The language is also colored by the local dialects, especially when dealing with everyday or specifically local matters, since even for the educated the normal language of conversation is the local dialect.

(4) A journalistic style has evolved in the language of the press and radio which is highly standardized and has a relatively limited vocabulary. This special, somewhat Europeanized style is almost the only form of Classical Arabic read or heard by the masses of half-educated people. This is the nearest there is to a fixed norm for the language.

(5) In technical terminology there is no norm. In popular works there is anarchy. Each author feels free to invent terms, and little tendency toward standardization has appeared.

(6) The practice of working-in material from the older Classical language is

still widespread. Authors make use of quotations, allusions, and rare words and phrases for esthetic effect even at the cost of intelligibility.

(7) Expressions occur in respected modern authors which are not understood and cannot be explained by highly educated informants. These expressions may be creations of the author, cant terms of a small clique, or simply errors in Classical Arabic grammar.

One feature of the situation which the author hardly mentions at all is the difficulty of determining the correct vowelizing of many modern words, since they do not normally appear in print with vowel signs.

The author picks his way carefully through these pitfalls for the lexicographer and makes sound decisions throughout. For example: technical terms which have appeared widely but have not yet become general are marked with a special symbol; decisions of the Egyptian Academy are ignored; some loanwords of which the pronunciation depends on the education of the reader are given without transliteration, in some cases with the original foreign word cited as a clue; non-Classical vocalizations in common use are included and specially marked.

As a final point in connection with linguistic matters, it must be noted that the necessary grammatical apparatus is given throughout and clearly. Stem vowel of the imperfect, verbal nouns, plurals, appropriate prepositions are all given. This fact alone sets Wehr's dictionary apart from the others.

The second important feature of this dictionary is the nature of the vocabulary itself. The entries are limited rigorously to words in current use. Obsolete words and obsolete meanings of modern words are excluded. As a typical example, *maşna* is glossed as 'factory' or related meanings only, while in Hava's Arabic-English dictionary it is glossed only as 'cistern'. The author apparently went through vast quantities of modern written Arabic. He reports that the work was based on a collection of some 45,000 slips and that he made use of numerous Egyptian newspapers and magazines, almanacs and guide books, and modern literary works. A few non-Egyptian sources were also used. As secondary sources he utilized the glossaries of Bercher, Colin, and Odi-Vassilieva, as well as the second edition of Elias. From the material included it is clear that Bercher was gone through exhaustively; it is a pity that a later edition of Elias was not used just as thoroughly.

The chief objection which may be raised against the vocabulary itself is the exclusive emphasis on terminology used in Egypt and, to a lesser extent, in Tunis and Iraq. Special terms used elsewhere are usually not entered. This means, for example, that we usually cannot find political terms (e.g. for president of the republic, various ministries, electoral and parliamentary terms, frequently cited political factions, problems), terms for weights, measures, and money, and names of food and clothing of countries like Syria, Lebanon, Jordan, and Saudi Arabia. In a subsequent edition the attempt must be made to include many of these terms. Also, terms connected with the UN could readily be added, especially since many already may be found in the fifth and sixth editions of Elias' Arabic-English dictionary.

The vocabulary is very comprehensive; repeated reference to it for elucidation

of newspaper Arabic during a four-month trial period rarely produced a disappointment, and usually proved more helpful than consultation of other dictionaries. Apart from the regional terms already mentioned, there are occasional omissions such as would be discovered by anyone using the dictionary steadily for some time. Examples of omissions that I noted are *niṣāb* 'quorum', *ṣuḥrah* 'surname', *kilāsikī* 'classical', *fizā* or *vīzā* 'visa'.

Proper names are not handled satisfactorily, although probably better than in many other dictionaries. Personal names are not included at all, and place names are haphazard. Thus, Lebanon and its capital Beirut are entered, Jordan is entered but not its capital Amman. The relative adjective is listed for Tunis and Mecca, but not for Beirut and Baghdad, although the latter adjectives have plurals worth noting. In a revised edition it might be better (a) to omit all proper names, (b) to include many on some consistent basis, or (c) to add a separate gazetteer and list of personal names.

The format and typography of the book are superb. Good paper, wide margins and generous spacings between lines and columns, unusually clear Arabic type, simple and consistent abbreviations and devices combine to make the dictionary readable and handsome. For steady reference use it would probably be better to have the two volumes bound in one, with a sturdier binding. One typographical feature deserves to be mentioned particularly: the double spelling in Arabic and roman letters. All items except for a few categories listed in the foreword are listed first in unvowelled Arabic script and immediately afterwards in full transliteration. This avoids the cumbersome and expensive addition of the vowel signs, with attendant problems of misprints and difficulty of reading, and yet provides the reader with the necessary information about the vowels. This feature is a successful innovation and deserves imitation in other works on Arabic in European languages.

The system of transliteration employed is the traditional scheme of Semitists; it is satisfactory except for a very few ambiguities. For example, final *fathah* and *tā'* *marbūḥah* are both transliterated -a, and final *alif tā'* and *alif tā'* *marbūḥah* are both transliterated -āt. Final *kasrah yā'* *shaddah* is inconsistently transliterated -iyy or -ī, sometimes with examples of both on the same page. Also, it seems unnecessarily pedantic to transcribe final *hā'* 'his, him' as -hū, -hī instead of -hu, -hi.

In transliterating certain words, either loanwords from other languages or Egyptian colloquialisms, the author occasionally uses letters representing sounds not included in the traditional inventory of Classical Arabic phonemes: *ē*, *ō*, *g*. This practice is a reasonable compromise in treating words which as a matter of observable fact are regularly pronounced with these sounds, not with the 'correct' Classical Arabic sounds. It is difficult to draw the line in a matter of this sort, but in my opinion the author could have gone further in this direction. By freer use of *ē*, *ō*, *g*, *v*, and *p*, Wehr could have given reasonable transliteration of words like *gālōn* 'gallon', *vīlō* 'veto', *diplōmāsī* 'diplomatic', the problem of which he avoids by omitting the transliteration altogether. A few inconsistencies should be cleared up, such as the occasional omission of macrons in *ē* and *ō*, and the treatment of three-consonant clusters.

The problem of the order of loanwords is handled effectively by including them under roots only when they clearly fit and otherwise listing them in straight alphabetical order by the letters of the word. This solution seems satisfactory, and suggests that it might be possible to arrange a dictionary of Modern Arabic completely by the alphabetic order of the words rather than of roots. In this connection it would be useful to have a few more cross references, especially for forms ambiguous with regard to root and for alternant forms of recent loanwords.

Several details of typography deserve comment. First, in accordance with Egyptian usage, final *yā'* is always written without the dots, and the sound *g* in foreign words is spelled with *jīm*. The more widespread usage of *yā'* with dots when representing *ī* or *y* and without dots when representing *ā* seems preferable. Also it might be better to use an unambiguous symbol for *g*, such as the Persian barred *kāf* widely known in Iraq, or the *kāf* with three dots sometimes used in Morocco. Finally, the character *ī* appears throughout the book from two different type fonts, apparently at random (e.g. both in the same word *arāṭīqī*, p. 10).

In the linguistic sophistication shown, in the actual content of the vocabulary selected for inclusion, and in mechanical matters of format and typography, Wehr's dictionary is superior to any other dictionary of Modern Arabic; he is to be congratulated on the satisfactory completion of a difficult task. Future editions may make revisions and add items, but the groundwork has now been laid. The American student of Modern Arabic can only wish for the appearance of an Arabic-English dictionary of equal quality.

A resurvey of Hebrew tenses. With an appendix: Hebrew influence on Biblical Aramaic. By FRANK R. BLAKE. (*Scripta Pontificii Instituti Biblici*, No. 103.) Pp. xii, 96. Roma: Pontificum Institutum Biblicum, 1951.

Reviewed by CARLETON T. HODGE, *Foreign Service Institute*

Blake's monograph contains two separate articles, a longer one on Hebrew tenses (1-80) and a briefer one on Biblical Aramaic (81-96); only the former is the subject of discussion here. The basic dichotomy in the Hebrew verbal system is between a suffix paradigm, the 'perfect', and an 'imperfect' with prefixes for indicating person and suffixes for gender, number, and 'modes'. The forms of these are adequately described in the traditional grammars. The problem is the description of their occurrence and of their meaning. A separate problem, also treated by Blake, is their origin and development. The classic treatise is that of S. R. Driver, *A treatise on the use of the tenses in Hebrew* (3rd ed., Oxford, 1892; quotations below are from the 2nd edition of 1881, which is that referred to by Blake, page 1). Blake wishes to replace Driver's description of these forms as 'aspects' with what he calls a 'tense' system. He calls his book (vii) 'an attempt, under the stimulus of H. Bauer's monograph on Semitic tenses, to clarify the ill arranged mass of material on the syntax of the verb presented by the traditional Hebrew grammar (best represented by Gesenius), and to substitute for the unrealistic and fanciful explanations of syntactic facts arising out of the usually

accepted "aspect theory" (as in Driver's *Tenses*), a treatment that is more in accord with what we know of the development of language, and with the fundamental principles of linguistic science.'

'The fundamental principles of linguistic science' are nowhere defined by Blake, but it is clear from his presentation that he is primarily concerned with historical linguistics. This is in distinct contrast with Driver. The latter is very sound methodologically. In the work noted above he has examined the Hebrew verb forms as forms, collected their occurrences, and examined them to determine from the context of each how they were used and how they corresponded to English usage. Notes are added on the historical picture, but the method is basically descriptive. This is sound linguistics; one may quarrel with Driver's conclusions, but not with his methods. The soundness of his approach may be seen from the following quotation from the Preface (vi), signed Christmas 1880: 'There are, however, many obstacles to be overcome before the true nature of the tenses can be realized. In the first place there is the influence of our own language. This has been familiar to us from childhood; it constitutes the framework of our thoughts; it has determined for us the forms under which ideas present themselves to our mind; it has impressed upon us its own distinctions and lines of demarcation, at the same time silently ignoring those established by other languages.' In regard to the variety of English meanings that a given form may have, Driver says (55), 'The meanings assumed, however divergent, do not in reality involve any contradiction: a fundamental principle can be discovered which will embrace them all—a higher unity exists in which they meet and are reconciled.' And again (56): 'The *context*, carefully and intelligently studied, constitutes the differentiating factor which *fixes* the signification of the tense.' It is quite unfair to describe Driver's work as Blake does (1): 'The whole treatment presents a picture strongly characterized by complexity, obscurity and artificiality, a system which it is difficult to imagine as developing and existing in the minds of any language group.'

Blake, on the other hand, is operating on two false assumptions: one, that tense (time) distinctions are fundamental to language ('*A priori* it seems unlikely that any language would vary the forms of its verb without regard to the most prominent function of that part of speech ... namely its indication of the time point of an action or state, its tense' [2], though a few lines later he speaks of the 'omnitemporal verb form like the verbs in Chinese or Malay'); two, that the only approach to the problem is the historical one. In the latter assumption he is following Bauer (as quoted above from the Preface; see also page 1).¹ The present volume is an exposition of his theory of the origins of the Hebrew perfect and imperfect, and an explanation of the situation as it exists in Biblical Hebrew in terms of this historical hypothesis.

¹ H. Bauer, *Die Tempora im Semitischen* (BA 8, 1912). The similarity of Bauer's viewpoint and Blake's is seen from the following passage (25): 'Es ist nämlich m. E. ein ganz unmögliches Unternehmen, die Grundbedeutung des Imperfekts vom Hebräischen aus finden zu wollen. Das Wesen einer Sprachform lässt sich eben nicht durch Reflexion über daran abgeleitete Bedeutungen bestimmen ... Was geschichtlich geworden ist, lässt sich nie rein begrifflich, sondern nur auf geschichtlichem Wege verstehen.'

In very broad outline the situation in Biblical Hebrew is as follows. The perfect is used as a general past, occasionally as a present or a future. The imperfect is used as a general present-future, occasionally as a past (usually durative). After waw conversive, the perfect is used as a future, the imperfect as a past. Driver's thesis was that the perfect is a completed aspect and the imperfect an inchoative-imperfective aspect. Blake's thesis (summarized 77-80) is that the imperfect was originally 'an omnitemporal form' and the 'perfect a predicate adjective form with present meaning' (77). The situation as it exists in Hebrew is then derived by specialization of time aspects. The perfect becomes 'the normal expression of past time, but with preservation of ancient present meaning in some constructions, especially in the case of stative verbs' and has 'present future-modal meaning' after waw conversive, 'based perhaps on original present meaning of perfect' (77). The imperfect becomes 'the normal expression of present-progressive past-future-modal ideas, though past meaning is still retained in some constructions' and after waw conversive, 'based on exceptional past meaning of imperfect', it 'becomes regular verb form coordinated with preceding perfects' (77). We are here going from omnitemporality to omnitemporality, and regardless of the validity of the hypothesis we are in just as dire need as before of some such analysis as Driver's. Blake's hypothesis does not explain anything, let alone substitute tense for aspect. If the imperfect was omnitemporal, what did it mean? Was it an aspect? How did it differ from the perfect? The original 'present meaning' of the perfect is an assumption based on the Hebrew stative verbs, the 'prophetic perfect' and 'the use of the perfect in Arabic in wishes and future conditions' (3). This is tenuous indeed. Before generalizing from 'stative' use, one should compare the variant meanings (as far as time goes) of such forms as the Ugaritic perfect, the Akkadian permansive, and the Egyptian old perfective. There is need for a restatement of the Hebrew verb system, but Blake has not given us the answer. Such a restatement must be followed out more along the lines of Driver's work.

The task of describing Biblical Hebrew is a very complex one. The text of the Bible is composed of selections composed over a time span of about a thousand years; but the pronunciation of this material is relatively uniform, as the text reached its final form (complete with vowel points, accents, etc.) about the 6th century A.D., seven to eight hundred years after the composition of its latest parts. To complicate the matter further, scholars are in broad disagreement as to the dates of many sections. This poses an almost insuperable descriptive problem when the text is taken as a whole. A really definitive statement will, when written, concern itself first with descriptions of easily datable selections, to eliminate the possible concurrence, in the data being described, of forms or usages of different dates. This will eliminate a tendency to use the time span as a scapegoat for inconsistencies or as an explanation for unwelcome facts. Blake, faced with the omnitemporal situation in Biblical Hebrew, says (75): 'It is extremely unlikely that such a mixed syntax should exist as the syntax of any one language at any single period, and the fact that the Hebrew Bible ... contains documents covering a period of from three quarters to a whole millenium ... makes practically inescapable the conclusion that Hebrew syntax as we have

it gives a conglomerate picture of the varying syntaxes of different periods of the language.' Such a series of descriptions will also furnish what data there may be for the internal development of forms and their uses. The problems involved in such an undertaking are not to be underestimated.² Blake does not attempt this in any way, but uses the data found in Driver and in Gesenius-Kautsch, *Hebrew grammar* (28th ed. 1910), as the basis for his study. A completely fresh study, based directly upon a limited body of material from the text itself, must some day be undertaken. Until then Driver's work will remain standard.

Modern Hebrew. By ELIEZER RIEGER. Pp. 156. New York: Philosophical Library, 1953.

Reviewed by NOAM CHOMSKY, *Harvard University*

Any study of modern Hebrew must take account of certain realities. Hebrew was revived quite recently and quite artificially as a spoken language and the participants in this revival have been speakers of many different linguistic backgrounds. There are at present great numbers of recent immigrants in Israel who speak Hebrew haltingly or not at all, and many first-generation speakers even of long residence still speak self-consciously and with distinct traces of their native languages. On the other hand, modern Hebrew has by now evolved as a unified language with a strong nucleus of native speakers. This rapid development naturally poses many special problems for study, but it makes prescriptive grammar, whatever its justification might have been a half-century ago, approximately as irrelevant here as in the study of any other language.

That the author is well aware of this is evident from many remarks that he makes. Thus he points out correctly that 'the emergence of a unified Hebrew pronunciation is taking place at a very rapid rate', and he criticizes the attempts to impose preconceived systems on languages, at the same time proposing the project of a descriptive grammar of current usage. Unfortunately, this awareness seems to dissipate when he in fact approaches the linguistic material. Not long after reading that 'the task of the grammarian is not to devise rules but to analyse and describe the structure of the language at a given period', we come upon a list and discussion of the 225 most common 'errors' in spoken Hebrew. The technique by which these 'errors' were collected suggests a method that a linguist might be tempted to use in constructing a linguistic corpus: 'each student [of the Hebrew University School of Education] spent a week listening to the speech around him—on the street, in buses, in restaurants and cafés, etc.—and jotted down all the errors he could detect.' The conclusion is then reached that 'conventional Hebrew grammar does not reflect many important aspects of everyday spoken Hebrew.' But now, in place of the previous suggestion that this gap be closed by the construction of a truly descriptive grammar, it is recommended that this list of 'errors' be used as a guide for a correctional teaching program. 'One can divide this number over a specified number of years and concentrate each year on correcting a given number of errors.'

² For some idea of these problems see the treatment of historical phonology by Z. S. Harris, *Development of the Canaanite dialects* (New Haven, 1939).

A detailed investigation of the list merely adds to the confusion. Many of the errors appearing in the list are common idioms typical of native speech, e.g. *ex kor'im lxa* 'what's your name', *ani oxel hamon* 'I eat a lot', *kmuwan šehalaxti lakontsert* 'naturally I went to the concert'. Many are forms representing regular and typical morphological developments of the modern language, e.g. *ra'ilem* instead of *r'ilem*; *habetsefer* instead of *bet-hasefer*, where a common compound noun *bet-sefer* 'school', literally 'house of a book', is treated as a single inseparable noun, a very prevalent development; *yašan* in the past tense instead of *yašen*, by analogy with all other common verbs. Many are deeply entrenched forms which no native would think of questioning, e.g. *tsorfat* instead of *tsarfat*, as the Biblical vocalization would indicate; *bana't* instead of *bandi*; *amod* 'stop!' instead of *atsor*. Others are real errors which would stamp any speaker as non-native. At the same time the recommended 'correct' forms are often in fact standard usage, but are frequently stilted and pedantic forms which would be considered purely literary, or perhaps even foreign, by native speakers.

The chapter on pronunciation is equally confused. While on the one hand the author takes a stand against those who favor the reintroduction of the gutturals (as if grammarians could effect this return to the 'original' pronunciation), at the same time he asserts that 'comparatively few people in Israel take pains with their pronunciation, and consequently the Hebrew pronunciation one commonly hears leaves much to be desired'; 'modern Hebrew pronunciation tends to be careless and little attention is paid to the esthetic aspect'; 'the most important step [to improve 'the esthetic quality of our speech'] would be to introduce geminated consonants and the shva', etc. The principal objective motivating these suggestions for the 'improvement' of Hebrew pronunciation is 'to achieve a common standard pronunciation in Israel and the Diaspora'. But in view of the fact that modern Hebrew is a living language with a large body of native speakers, it is clear that the only significance of such a goal is that the language of this community be taught wherever modern Hebrew is proposed as a second language. The proposal that immigrants should be taught an artificially constructed language quite different from that of the speech community into which they are to be introduced seems a strange conclusion for a study intended in part to help to 'shorten considerably the period of adaptation required by the masses of immigrants in Israel'.

The second half of the book contains a discussion of methods in language teaching, leading up to the presentation and discussion of the 'Jerusalem method', which, much more than the linguistic sections of the book, is in accord with current linguistic theory. The 'Jerusalem method' is based on the aural-conversational approach, with careful attention to pronunciation, gradually decreasing amounts of translation, inductive presentation of grammar, material based on Israeli life, and a carefully controlled vocabulary. This vocabulary, based mainly on frequency counts carried out by the author and on a suggested 'basic' vocabulary, is presented in detail, along with a discussion of the considerations involved in its construction.

Oneida verb morphology. By FLOYD G. LOUNSBURY. (Yale University publications in anthropology, No. 48.) Pp. 111. New Haven: Yale University Press, 1953.

Reviewed by HARRY HOIJER, *University of California, Los Angeles*

Oneida, today spoken in Wisconsin, is one of seven American Indian languages which make up what is left of the Iroquoian stock; the others are Mohawk, Onondaga, Cayuga, Seneca, and Tuscarora, all spoken in upper New York and the adjacent regions of Canada, and Cherokee, spoken in North Carolina. Very little descriptive material has been published on any language of the group; Lounsbury's bibliography lists only two items, both brief, that concern these languages specifically. The Iroquoian languages have been known to be related since the publication of J. W. Powell's *Indian linguistic families of America north of Mexico* (1891), but no comparative work of any significance has been published since that time.

Lounsbury began his field researches on the Iroquoian languages in 1939, and has now collected data on all or nearly all the languages of the group. *Oneida verb morphology*, based on field materials which were secured in 1939-40, is his first major publication. It was written originally (1949) as part of a doctoral dissertation at Yale University, under the title *Iroquoian morphology*, and is here presented with only minor revisions.

The monograph is divided into three major parts. The first of these (Introduction, 11-26) is a review of 'the method of descriptive morphology' (11-22) and a brief outline of Iroquoian verb structure generally (23-6). In the second part (Oneida, 27-92) we find the bulk of the work, a careful description of Oneida phonology (27-34), pre-pronominal prefixes (35-50), pronominal prefixes (51-71), verb bases (72-84), and inflectional suffixes (85-92). The last part is devoted to the analysis of an Oneida text (93-110).

Lounsbury distinguishes between two methods, that of 'morpheme alternants' and that of 'internal reconstruction' or the morphophonemic method (11 ff.). The former is described as (1) cutting forms into minimal segments that possess meaning ('morphs') and (2) grouping 'phonetically different morphs which have the same meaning ... and which are in complementary distribution' into classes called morphemes (11). The method of internal reconstruction, on the other hand, 'sets up a fictitious agglutinating analog, such that a one-way transformation from the analog to the actual utterances is possible, and it segments that instead. ... Forms of the analog are sometimes called MORPHOPHONEMIC FORMS ... The relation of actual forms to those of the analog is expressed in transformation formulas called MORPHOPHONEMIC RULES' (13).

The method of morpheme alternants, it appears, raises some problems in respect to segmentation; it is often difficult either to find a morpheme which carries a meaning expressed in the form or to assign specific phonemes (or phoneme sequences) to one or another morpheme. Lounsbury outlines four methodological possibilities of accounting for these ambiguous cases: α -morphemics, where 'segmentation is carried as far as possible, and such morphemes (present in meaning) as have not been accounted for in the resulting segments are said

to be represented by zero allomorphs' (12); β -morphemics, which 'allows for the possibility of a phoneme belonging either to an allomorph of only one morpheme or simultaneously to allomorphs of two (or more) different morphemes; i.e., it allows of overlapping morphs' (12)—even portmanteau morphs (which overlap completely) and morphs so related that one is included in the other; γ -morphemics, which disallows overlapping morphs but assigns some phonemes to morphs (called empty morphs) 'which do not belong to any morpheme' (13); and δ -morphemics, 'which allows of all the possibilities' (13), subject of course to orderly rules of procedure.

Lounsbury himself uses the method of morpheme alternants in the analysis of the Oneida verb, and the segmenting technique called δ -morphemics. He adds (17): 'The following rules will govern segmentation. (1) A zero morph is never set up when the meaning to be assigned would be implicated by the selection of a particular allomorph of some one other morpheme. In such cases a portmanteau or included morph is set up instead. (2) An empty morph is set up only when a given phoneme or phoneme sequence would otherwise have to be taken as identical fractions of the allomorphs of two or more different morphemes. (3) Use of the principle of overlapping morphs is restricted to the special cases where the whole of one coincides with the whole or with part of another, i.e., to portmanteau and included morphs.'

While there need be no quarrel with Lounsbury's choice of methods (these might better be referred to as techniques) insofar as a strictly synchronic description is concerned, it is also true that the morphophonemic method provides, in addition to an adequate synchronic description, numerous clues to linguistic change that may be useful in comparative studies. There is no good reason why such material should not be made available when it is possible to do so. Historical considerations may be irrelevant to synchronic studies, but this is no reason to insist that synchronic studies should not so arrange the materials of a language as to implement and sustain comparative work.

Lounsbury's treatment of the Oneida verb is an excellent example of the application of the method of morpheme alternants to a specific body of data. He explores all the potentialities of Oneida verb structure, and while his illustrative material is scanty and difficult to follow, it is adequate for its purpose. The verb structure, it seems, includes four classes of morpheme, arranged in an unvarying order as here indicated:

(1) PRE-PRONOMINAL PREFIXES are elements of widely variant meanings (tenses, locatives, negation, repetition, etc.) which 'nevertheless constitute a formal sub-system because of the complex pattern according to which they combine' (23). Lounsbury finds some difficulty in determining the meanings of his morphemes; in some cases, at least, the morphs included in a single morpheme are not only phonemically diverse but also highly divergent in meaning. Thus, he isolates the morpheme *s-/j-* (it has these two morphs), which is defined as 'iterative'. But 'the term "iterative" ... fails to describe more than the first of the [six] uses and meanings listed ... It is difficult to find a common kernel of meaning in all of these ...' (49). Cases like this, not infrequently encountered, suggest that Lounsbury may be applying his method too literally; it is quite

possible, given a larger sampling of the language, that morphs so divergent in phonemic form and meaning, which now appear to be alternants in complementary distribution, may turn out to belong to different morphemes.

(2) PRONOMINAL PREFIXES include both subject and object pronouns, and define such categories as person, gender, and number. Oneida recognizes a first, second, and third person; an inclusive (first plus second) and exclusive (first plus third); and a zero person, used in the case of zero subject for oblique reference to any person, and in the case of zero object, in intransitive verbs with subject and no object. There are four genders: neuter (inanimate things and abstract notions), feminine-zoic (any adult female person, or general indefinite reference to any animal), masculine (male person or animal), and feminine-indefinite (female person, usually young girls and old ladies, and also indefinite persons or persons in general). Some verb bases 'inflect with all 58 pronominal prefixes, and have both animate ... subjects and animate objects and combinations of these, as well as neuter and zero subjects and objects. Some verbal bases, however, inflect only with the 15 prefixes in which the object is zero' (52).

(3) THE VERB BASE has five positions, each of which may be occupied by one or more morphemes. The sets of morphemes filling each position are mutually exclusive. Only the verb position is essential; the reflexive position, the noun position, case position, and purposive position 'may or may not be filled, depending on the semantic requirements of the situation' (72). In the noun position are found incorporated noun stems, which function in various capacities relative to the verb. Morphemes found in the case position include dative, instrumental, causative, and distributive suffixes; several of these may occur in a single form. The purposive morpheme 'has meanings which correspond exactly to those of *going* and *going to* when used as auxiliary verbs in English ...' (83).

(4) INFLECTIONAL SUFFIXES include three aspectual morphemes (for the perfective, punctual, and serial), a continuative (used with the perfective and serial aspects), a remote past (used with the perfective), a 'former past' (used with the perfective alone or with the perfective plus remote past), and a progressive (added to the perfective aspect stem to form a new bases which then may be further inflected in all aspects and tenses).

The structure of Oneida, it is evident even from this brief summary, is a complex one, even for an American Indian language, and offers a welcome addition to our data on linguistic structures. Lounsbury's description, though overly brief and compact, is an excellent one, carefully presented and methodologically consistent. It represents in fact one of the better monographs from the point of view of modern linguistic method, and is important as well for being the first descriptive account of an Iroquoian language produced in the modern tradition.

There is, however, too great an emphasis on method and technique, and too little space devoted to the linguistic materials themselves. In this too Lounsbury belongs to the modern tradition, at least as represented by Americanists; for much of our work in the past twenty years has emphasized method and technique far more than is necessary. In Lounsbury's monograph, it surely seems unnecessary to take up eleven pages to describe procedures well known to pro-

fessional linguists; the space might have been more usefully devoted to Oneida grammar.

Finally it must be noted that Lounsbury's style is so compact and telegraphic as to make reading and comprehension unnecessarily laborious. Very few examples, so important to the understanding of a structural description, are clearly analyzed by the author; he leaves this task to the reader. An expanded exposition, with many more carefully analyzed illustrations, would have greatly increased the value of the work. As it stands, the monograph is more an exercise in method, illustrated from Oneida materials, than a descriptive study of the language.

Speech development of a bilingual child: A linguist's record. Vol. 3, Grammar and general problems in the first two years; Vol. 4, Diary from age 2. By WERNER F. LEOPOLD. (Northwestern University Studies in the Humanities, Nos. 18 and 19.) Pp. x, 200; ix, 176. Evanston, Illinois: Northwestern University Press, 1949.

Bibliography of child language. By WERNER F. LEOPOLD. (Northwestern University Studies in the Humanities, No. 28.) Pp. v, 115. Evanston, Illinois: Northwestern University Press, 1952.

Reviewed by HARRY HOIJER, *University of California, Los Angeles*

Speech development of a bilingual child is an elaborate four-volume study of one child's acquisition of two languages, English and German. Volume 1, *Vocabulary growth in the first two years*, was published in 1939, and Volume 2, *Sound-learning in the first two years*, in 1947. Both were reviewed by H. V. Velten in *Lg.* 25.215-9 (1949).

The scope of Volume 3 is indicated by its main heads: Syntax of the First Two Years (1-75), Morphology of the First Two Years (76-101), General Linguistic Problems of the First Two Years (102-88). The last named section deals with the learning of meanings (Semantics, 102-53), other aspects of the learning process (154-73), and bilingualism (174-88).

Volume 4 is a diary record of the child's linguistic development from two years onward to 1942, when the child was twelve years old. This material is less detailed than that on which Volumes 1-3 are based, and is presented just as it was written, with no attempt at analysis. As a record of one case of linguistic development it is not without interest. It is a pity that Leopold did not choose to publish the diary for the first two years as well. He excuses this omission on the ground that Volumes 1-3 so thoroughly treat the data of the first two years that there is no need to publish them. It is possible, however, that the material on the first two years, were it available, might be treated quite differently by another linguist.

As Velten pointed out in the review cited, Leopold's work is one of the few studies of child language made by a linguist; most of those previously available have been done by psychologists and educators. Leopold has produced a minutely detailed and painstaking record, which is intended as a case study of a single

child's language-learning experience. He recorded everything the child said, from the earliest age at which she made intelligible sounds until the record was complete. Elicited forms, except in a few instances, were not recorded, and no attempt was made to determine how much the child understood of the speech around her but did not actually use. The record was made phonetically, with as much accuracy and detail as the observer could muster. It goes without saying that data so collected have a considerable importance in an area so little explored linguistically as child language. Detailed records over so long a period are difficult to acquire, and Leopold's material stands as a model effort in this direction, a model which we hope may have many imitators.

In his analysis of the material, Leopold makes a distinction between the learning of sounds and vocabulary on the one hand, and of syntax on the other. Vocabulary and sounds are acquired 'primarily by imitation of objective material heard again and again in substantially unvaried form; analogy and pattern ... play a subordinated part' (3.1). Syntax, however, involves not imitation so much as pattern: 'Phrases and short sentences can be learned by imitation; but the application of syntactic patterns to ever-varying vocabulary material requires well-developed powers of abstraction. It is not surprising that the learning of syntax comes last' (3.1).

Correlated with this conclusion is the fact that the early speech of the child consists very largely of 'one-word sentences' and therefore, according to Leopold, affords 'nothing of a syntactic nature to report' (3.1). It is not until near the end of the second year that the child begins to use sentences of two, three, and more words, and to arrange the words mainly by taxemes of order. Inflections and endings develop still later: 'The accidence [of the child's early speech] was rudimentary. ... Words were placed side by side without formal modifications, and the syntactic relationship between them had to be inferred by the listener' (3.76). It may be noted parenthetically that Leopold defines morphology wholly in terms of accidence; his treatment of derivations and compounds is not in structural terms, but is included rather under semantics. (See especially 110-114.)

A summary of Leopold's view of the development of language in the child's first two years is given in the paragraph that follows (3.77):

Although the child does not use many formal signs of syntactic functions during the first two years, it is probable that, after the first stage of syntactically diffuse one-word utterances, the feeling for syntactic functions and for the discrimination between parts of speech grows clearer and clearer. ... As in the areas of vocabulary and sounds, it is reasonable to assume that the child progresses from a dim, diffuse perception of the material presented by the standard model to an ever clearer grasp of differences, from coarser contrasts to ever finer intermediate shades. The power of expression lags behind the understanding, until the full imitation of the model is eventually achieved. In the field of syntax, the morphological signs are the last stage, which in turn consists of a series of steps progressing from the learning of the most conspicuous and useful formal elements to the less useful ones.

These conclusions imply, it seems to me, that language learning is more or less a conscious process, that the child learns to manipulate language patterns by

gradually becoming conscious of them. In the first two years, according to Leopold, the child's language is essentially 'pre-grammatical' as a result of his 'dim and diffuse perception' of the model language. Thus (3.3-4):

There can hardly be any doubt that the one-word sentence constitutes for the small child a complete expression of thought and feeling. The child is not aware of the fact that his sentence is incomplete in comparison with prevailing adult speech-habits. His attention in the understanding of spoken sentences is concentrated on the most significant word, which also bears the strongest stress. This word is at first reproduced alone, and since the child's environment is well equipped to understand the intention of such utterances with the help of the concrete situation which is at that stage always involved, this primitive mode of expression satisfies the needs of communication for an extended period.

Leopold appears to lose sight of the fact that all language, whether of children or adults, is made up almost entirely of unconscious patterns of speaking. While we are, at this stage of our knowledge, uncertain as to the procedures whereby these patterns are acquired, it is surely an over simplification to say, as Leopold does, that they are taken on by an increasingly more conscious perception of the patterns of the model language.

The stages in language learning proposed by Leopold are also open to criticism. We are led to believe that the child's language is without a grammar—or at least is 'pre-grammatical'—during most of the first two years. If this is so, then surely the child has no language in this period; grammar or structure, while by no means all of language, is certainly indispensable to its existence. Leopold cannot want to convey the notion that the child first takes on vocabulary items and sounds, and only later adds a grammar to these; the material he collected is after all a long list of utterances. Moreover these utterances are not random; they have a formal structure, phonological and morphological, even if in lexical content they consist only of single words. In one place (3.4) Leopold says: 'The child has one formal syntactic device at his disposal from the beginning to distinguish between several types of utterances: intonation.' If this is true, then it would appear that at least two categories of meaningful arrangement are present in the child's language from its inception: modulation and selection.

Leopold unfortunately does not follow up this aspect of the investigation. Indeed his recordings of the child's utterances, otherwise so painstaking and meticulous, indicate intonation only by the imprecise technique of using conventional signs of punctuation. Moreover there is in effect no classification of utterances in structural terms, but instead a long discussion of their functions. For example, much of the section on syntax is taken up with problems like this one (3.5): 'Without losing sight of the fact that emotions, wishes, questions, and statements cannot be completely disentangled during the one-word stage, we shall look at a representative number of examples in which one or the other of these categories prevails.' Without denying the value of these observations, it can be regretted that Leopold did not also give us a careful structural study of the child's language.

A large portion of Volume 3 is given to semantics, the problem of the develop-

ment of meanings in the child's language. Leopold's conclusions are briefly summarized in the following quotations (3.154):

... words [in the child language] were [commonly] used in a variety of applications which the standard did not allow. The process of learning to conform to standard usage then required a later reduction of the scope of applicability. ... The standard usage was the ideal ... which imposed itself relentlessly on the child's speech and was bound to obliterate all deviations eventually.

We saw the reason for extended application in the vagueness of principles of classification, which is natural for an inexperienced small child. ... The principles underlying standard classifications are not always logically superior to those of the child; but the child recognizes the authority of the standard language and yields to it step by step. ... The child learned additional words as soon as she felt the need for more specific terms. At early stages, rough classifications sufficed. With advancing maturity, finer subdivisions were needed and new words were learned to express them.

The process is parallel, in its general outlines, to the learning of sounds, grammatical forms, and syntactic constructions. The material to be learned is taken over in an organization which reproduces at first only the crudest outlines of the standard organization. Little by little, finer classifications of the standard are recognized and reproduced.

As a general criticism of the portion of the book so far reviewed, it may be noted that Leopold lays too great a stress on the imitative process in language learning and ascribes too large a role to the model language. The problem is by no means so simple, as Leopold's own material demonstrates. It is more likely that the model language serves principally as a stimulus to the child's analogical faculties, and that child language, from the earliest period onward, is a patterned arrangement of elements, complete in itself, and deficient, relative to adult language, only in that the child has a much smaller stock of forms in which to operate. As his experience in speaking grows, his language develops a more complex structure. The problem of most interest to the linguist is (1) to describe the structural-semantic patterns of the child's language and (2) to relate these as a whole to the patterns of the adult language. It is in effect a problem of comparing two distinct languages, not, as Leopold sees it, of relating two stages of the same language where one, that of the child, is simply, or in large part, a poor imitation of the other.

On pages 154-73 Leopold discusses other aspects of the learning process that are related to and interwoven with the learning of language. His subject headings include (1) selectiveness of imitation, (2) motivation, (3) gestures, (4) comprehension before speaking, (5) memory, (6) echolalia, and (7) stages of language learning. Some of his conclusions in respect to these topics are summarized below.

(1) Language learning 'proceeds by imitation of the language which is presented to the child by his environment' (3.156). Imitation is not, however, a passive process; 'it requires the most active cooperation of the child and the exercise of all the capacities which develop apace with it' (3.156). Moreover the child selects what to imitate, and often rejects specific items: 'the transmission of a language is not like the filling of an empty barrel. It is a guided re-creation of a complex instrument' (3.156).

It is this factor in language learning, as indicated above, that is most neglected in Leopold's analysis. He tends rather to over-emphasize the 'dominating power of the model' and to treat the learning process almost exclusively as an additive or step-by-step process of imitation. Leopold's data suggest that analogy and patterning begin very early in the child's speech, and play an equal or even a larger role than imitation.

(2) The motivation for the child's learning to speak is 'that the child aims at establishing communication with the environment' (3.157). But there is also 'an element of pure enjoyment in self-expression, and communication with others does not only serve practical ends, but is enjoyed in itself' (3.158). More needs to be known on this very complex subject; communication, though certainly an important motivating factor in language learning and an important function of language as well, may well be overemphasized in both roles. Both children and adults may use language, as many have pointed out, in other and equally important functions.

(3) Since gestures are not a linguistic phenomenon, Leopold gives us only a brief description of the development of gesture in the child. Gestures are said in large part to precede and accompany speech, and both to take the place of words and to be replaced by them.

(4) There is no doubt of the fact that understanding precedes imitative speaking, 'progresses much more rapidly than speaking, and displays much finer discrimination' (3.163). Leopold's data, however, do not support the hypothesis of a state of 'pure comprehension' preceding that of speaking. Nor does he attempt to demonstrate 'the preponderance of words merely understood over those imitated'; 'nothing would be gained by it except a confirmation of what is obvious anyway' (3.163-4).

(5) Many words 'came up from memory without having been used on the same occasion by the standard speakers. At 1;11 [one year, eleven months] ... new words were so numerous that we did not always understand them; this means that no clue for them was found in the present situation or in the immediately preceding presentation, that they arose from memory' (3.165). It would be interesting to compare such forms with those known to be elicited by a particular stimulus. Perhaps some other process than memory is at work, one which involves an analogical extension of the child's learned vocabulary to new situations.

(6) '... a stage of echolalia is sometimes posited for all children, a stage of mechanical imitation preceding meaningful reproduction' (3.167). Leopold found no such stage in his child's speech.

(7) After reviewing other authors' attempts to divide language learning into stages, Leopold recapitulates the stages through which his child's learning passed. Crying, cooing, babbling, and inarticulate cries occupy most of the first nine months. Toward the end of this period, the child understood her own name, her cries acquired meaning and became words, and the first imitative words appeared. These early stages overlapped considerably. Succeeding developments (summarized 172-3) may be omitted here; we have mentioned them earlier.

Leopold completes the third volume with a brief discussion of the effect of

bilingualism on language learning. His child, it will be recalled, was exposed both to German and to English from the beginning, though unequally, for during most of the first two years English was the language most often heard. Leopold finds that this bilingual presentation had little effect on the child's sound system, morphology, and syntax. Furthermore, he finds no unequivocal evidence that the learning of two languages adversely affected the child's speech in either; on the contrary, he suggests that the benefits of learning two languages as a child far outweigh the possible losses.

The *Bibliography* is described as 'a by-product of many years of work in the field of child language' (iii). It is not intended as a complete bibliography, but simply as a record of titles that have come to the author's attention from every branch of child language study. It is 'much more comprehensive than any that has been published heretofore' (iii) and is certainly a useful listing of references in a field that has drawn the attention of so many disparate disciplines.

To sum up this review, there can be no question that Leopold's study, taken as a whole and with all its possible defects, represents none the less a notable and unprecedented contribution to a field that has been too little cultivated by the linguist. It is particularly important as an empirical record that carefully and in detail describes one child's experience in the learning of language. Leopold meticulously avoids the premature and often purely speculative discussion that so often prevails in publications of this sort. As linguists we can learn a great deal from Leopold's material, even though we may quarrel with his analysis and interpretation of the data. If, in spite of this work, the definitive empirical description of child language still remains to be done, we can at least say that Leopold's work has pointed the way to this task and made it easier of accomplishment.

The use of vernacular languages in education. (Unesco: Monographs on fundamental education, Vol. 8.) Pp. 156. Paris, 1953.

African languages and English in education: A report of a meeting of experts on the use in education of African languages in relation to English, where English is the accepted second language, held at Jos, Nigeria, November 1952. (Unesco: Educational studies and documents, No. 2.) Pp. 91. Paris: Education Clearing House, [1953].

Reviewed by WILLIAM E. WELMERS, *Cornell University*

The first of these publications includes and is built around the Report of the Unesco Meeting of Specialists on the Use of Vernacular Languages in Education, held in Paris in 1951. In addition there is a brief Introduction discussing the nature of language and its relation to education, a survey by continents of language distribution and of the use of vernaculars in education, and seven 'case histories' of problems involving languages in education. An appendix includes an outline of a tentative classification of languages spoken in the world today, a general bibliography, and a useful index.

It is a distinct pleasure to note that the general linguistic orientation of the introductory material and of the Report is excellent. There is hardly a trace of the outmoded but still widespread confusion of language and writing, or of popu-

lar folklore on the subject of allegedly 'primitive' languages. There is a general recognition that any language is a perfectly adequate means of communication within the culture of those who speak it, and an oft-repeated assumption is that elementary education should ideally be conducted in the native language of the student.

This publication is, it goes without saying, concerned with problems of applied rather than theoretical linguistics, as well as with problems of educational psychology and methodology, educational economics, and many other related fields. The interrelation of all these aspects of fundamental education in many parts of the world is exceedingly complex. Specialists in such details as language analysis can profit from this excellent survey of the entire field, and can take encouragement from the knowledge that their work can be and is being applied in so many concrete situations. It is encouraging also to note that educationalists are increasingly aware of their dependence upon specialists in other fields such as linguistics.

In the light of this background, the linguistic scientist will not quibble over the use of otherwise frightening phrases like 'the improvement of a language'. What is meant is the acquisition by a language—even under forced draft in some cases—of terminology required by contact with a new and different culture. It is only hoped that the clear-cut principles so well stated in this report will be conscientiously implemented by educational authorities throughout the world.

The case histories, involving problems of using vernacular languages and second languages in education, are at once interesting and instructive. If any of them can be singled out for special mention, it should be that of the Iloilo experiment in the Philippines, from which there appears to be definite evidence that children educated for two years only through the medium of the vernacular did better work **EVEN IN ENGLISH** during their third year of school than did those who were taught in English from the beginning. Although the problems of areas where several languages are spoken, some of them by only a few thousand speakers, are fearful in their complexity and delicacy, there is considerable hope that no man's language will be thought unworthy of the classroom.

Most of the material in this publication is interesting and eminently readable. In the introductory chapters and the body of the Report itself there are a few unfortunate slips of style and fact. Danish is mentioned among the official languages of the American continents (for the sake of Greenland), but Dutch is omitted (for Surinam) (23). A remark (33) about the situation in Laos is quite unintelligible: 'Now the two language families represented have their three main groups of dialects, French has been the favoured European tongue.' Fortunately, however, such slips are rare and hardly detract from the generally encouraging impact of the entire book.

A particularly interesting case of the interlocking of linguistic and other problems is given in the case history called Unification: The Akan dialects of the Gold Coast, by K. J. Dickens. The establishment of two dialects (Fante and Akwapem Twi) as literary media, to the neglect of Asante and others, the rise of local rivalries and tribal-dialectal pride, and the attempts to rectify the situation, add up to what Dickens aptly characterizes as a 'melancholy history'. It is the opinion of the reviewer that an oft-neglected factor in this unfortunate case is the bad

linguistics that was sometimes displayed. The older written forms of the dialects in question recognized nine vowel phonemes, but had the disadvantage of employing diacritics which were often ignored. The reformed spelling, based on recommendations by Westermann, recognizes seven vowel phonemes—or at least uses only seven vowel symbols. The symbols *e* and *o* are made to do service for two phonemes each; for brevity they may be described as /ɪ/ and /e/, and /ʊ/ and /o/, respectively. In almost every case, it is possible to state which phoneme the symbol *e* or *o* represents in terms of other vowel phonemes in the word, according to rules of vowel harmony. But the orthography conceals the fact that /e/ alternates morphophonemically with /ɛ/, and /o/ with /ɔ/, and that the two phonemes represented by each of the letters used belong in different morphophonemic sets. One could hardly expect native speakers of the language to analyze this problem concisely without linguistic training, but a vague feeling of discontent on their part should not be surprising, nor should it be attributed only to blind conservatism. One also wonders why no one, apparently, has presumed to suggest that Fante drop the spellings *ts* and *dz*, which represent allophones of /t/ and /d/ before high front vowels. Twi does not have these allophones, and at this late date the suggestion would probably be met by protests that the distinctiveness of the Fante people is being subverted. This protest was actually made to the reviewer, although he first made the suggestion at a time when he had heard nothing but Fante, and before he even realized that Twi showed a different allophonic pattern.

The second publication to be considered is called, in the subtitle, Report of a meeting of experts on the use in education of African languages in relation to English, where English is the accepted second language. The meeting was held at Jos, Nigeria, in November 1952. In addition to the report of the discussions as such, three papers are included: The place of African languages and English both in and out of school, by P. A. W. Cook; Problems in the use of African languages and dialects in education, by J. Berry; and The teaching of English as the second language in African territories, where English is the accepted second language, by P. Gurrey.

The report of the meeting itself is in outline form, with brief statements under each heading. The form of the outline is difficult to follow, but the coverage is complete and well thought out. Both in the report and in the papers, the same sensible outlook on linguistic problems is to be found. Of the three papers, the first is, in the nature of the case, most fully packed with information and statements of problems. The second is an excellent, readable, and sometimes whimsical statement that is highly recommended as an introduction to what Berry has privately termed 'politico-linguistic' problems.

The availability of the papers as well as the report means, of course, that slightly different viewpoints will be represented. The report, for example, observes in one connection (9) that 'It is of advantage wherever convenient that the orthographies used in a territory should agree with each other and especially with that used for the national or official language'. The reviewer's reaction was to protest that agreement between orthographies is too easily imposed for its own sake, without consideration of the structural problems of the individual language. In the second paper, Berry warms to this subject in a pointed footnote

(46) that demonstrates his able handling of structural problems. A few details can be added. In various African languages, sounds like that at the beginning of English *sheep* must be variously interpreted as /s/ (Kpelle), /sy/ (Jukun), /hy/ or even /h/ (Twi), and a unit phoneme /š/ contrasting with /s/ (Gourma). No amount of talk about uniformity—which both Berry and the reviewer heartily agree is desirable within limits permitted or demanded by the linguistic structures involved—can alter these facts.

A similar danger arises with the easy statement (9) that 'Diacritics are best avoided wherever possible'. It might well be added: but not at the risk of missing phonemic contrasts such as those of tone, nasalization, or stress. It has been assumed far too generally that the African can read his language without tone markings. In a few cases this may be true. In most cases it has been difficult or impossible to test, but there is every reason to believe that incnsstnt mrkng f tn (i)s (a)s bd (a)s omittng vwls n Nglish. It is hardly a compliment to the African's language to make him guess at what is meant—or at best, to force him to read very slowly—just to save a few type faces. The report in consideration here does not express itself specifically on this subject, but unquestionably favors orthographies as nearly phonemic as practicable. It is regrettable, however, that room is allowed for some old misconceptions to be perpetuated through misunderstanding.

Two minor details deserve mention. It is questionable whether a tentative orthography should omit symbols that might possibly be needed rather than use symbols that might later be eliminated (9). The reviewer also looks with considerable suspicion on the repeated assumption that Africans—'as everybody knows'—have unusual abilities for learning other African languages. The reviewer's offhand impression is that Africans speak English abominably, but that in French colonies they speak French beautifully and fluently. Could it be that this is because the reviewer does not speak French well? In point of fact, the Hausa spoken by native speakers of Jukun in Wukari Province, Nigeria, is atrocious: they miss the same glottalized consonants and length distinctions that Europeans tend to neglect, and rarely use plural forms of nouns. Similarly, in Liberia, the reviewer has had occasion to assist native speakers of Loma who were trying to learn Kpelle, although the two languages are very closely related.

In both of these books, it is most unfortunate that missions and missionaries are repeatedly cited as the source or conveyors of certain difficulties—support of conflicting and inaccurate orthographies, insistence on minor dialects, etc.—while no mention is made of the indisputable fact that, had it not been for the devoted work of these very organizations and individuals, scores of languages now used for school textbooks would still be unwritten today, and these experts would probably not even have had occasion to meet to discuss the advances already achieved and the problems involved.

In spite of these faults, none of which invalidates the great piece of work that has been done, both of these publications are valuable contributions to the field of applied linguistics, to the field of education, and to all the related fields with which Unesco must deal. It is to be hoped that they foreshadow a closer co-operation in the future between linguists and educationalists and other specialists.

NOTES

We record with sorrow the death of two honorary members. HOLGER PEDERSEN, Professor Emeritus of Comparative Linguistics in the University of Copenhagen, and an honorary member of the Linguistic Society since 1930, died in Copenhagen on Sunday, 25 October 1953. He will be remembered as one of the direct heirs of the Neogrammarian tradition, by his own achievements one of the giants in the history of linguistics. Readers of this journal recall his trenchant review of Sturtevant's Hittite grammar (29.93-6), one of the last works from his pen.

JULES BLOCH, Professor of Sanskrit in the Collège de France, and an honorary member of the Linguistic Society since 1949, died suddenly in Paris on Sunday, 29 November 1953, of a pulmonary congestion. He retained his faculties unimpaired to the end. His last book, completed only a few days before his death, deals with the application of cartography to Indic linguistics; it is to be published by the Société asiatique. One of his last articles appeared in the Edgerton number of this journal (Prākṛit *cia*, latin *quidem*, Lg. 29.229-30).

THE SOCIAL SCIENCE RESEARCH COUNCIL has established a Committee on Linguistics and Psychology, in order to create a community of understanding among specialists in diverse fields who are concerned with problems of language behavior. During the year 1953-54 the members of the Committee are as follows: Floyd G. Lounsbury of Yale University as chairman, John B. Carroll of Harvard University, Joseph H. Greenberg of Columbia University, James J. Jenkins of the University Minnesota, Charles E. Osgood of the University of Illinois, Thomas A. Sebeok of Indiana University, and Joseph B. Casagrande of the Council as staff representative.

As a first step toward its goal, the Committee planned and sponsored a research seminar in psycholinguistics, held at Indiana University in the summer of 1953 in conjunction with the Linguistic Institute. The seminar examined the three independently evolving theoretical models of language processes—those of linguistics, learning theory, and information theory—and tried to discover how these might be brought into a common conceptual framework. The Committee has also conducted a survey of current and projected research in language behavior, to ascertain the range of problems, the methods used, and the personnel engaged therein. Scholars who are at work in this field, or who know of studies in the field now in progress at their own institutions or at others, are invited to communicate with the chairman of the Committee or with the staff representative.

CHARLES F. HOCKETT, of Cornell University, asks that the following comment be published in order to clarify and reaffirm his position with respect to a fundamental question of linguistics.

It is sometimes convenient, and by no means improper in seeking to make a point clear, to invent a man of straw. But it does not improve the argument to give a straw man the name of an actual scholar. This is what Paul L. Garvin has done in a recent review in *LANGUAGE* (29.473 fnn. 6 and 8, 1953). Since I am the scholar in question, I must object.

Garvin's straw man is one who believes that linguistics is a game rather than a 'cognitive' science. My position is that linguistics is either an empirical science (I prefer 'empirical' to 'cognitive') or a mess of metaphysical pottage. As an empirical science, it may be aided by certain types of temporary game-playing—that is, by certain types of mathematical formalization. The same is true of other empirical sciences, such as physics. (I have stated all this overtly: A note on 'structure', *IJAL* 14.269-71 [1948].) Garvin assigns me to the 'pure game' camp by two quotations. The first (fn. 6) cannot reasonably be so interpreted. The second (fn. 8), here cited without its context, can; but to lift it out of context is to distort it.

Ordinarily a note of protest like this one is not worth writing; those who desire to get the facts straight will succeed in doing so despite distortions. In the present case I make an exception largely because Garvin's discussion sets up my 'point of view' in opposition to that of Jakobson. I do not wish to be in opposition to Jakobson. I wish to agree and disagree with specific statements or opinions of Jakobson's, as normal scholars do in the normal development of a scholarly field. Jakobson may be right about the role of binary oppositions in phonemics; but Garvin does us no service in trying to introduce binary oppositions into our collective scholarly life.

THE COMMITTEE ON HONORARY MEMBERSHIP (Albrecht Goetze, chairman; Joshua Whatmough; W. Freeman Twaddell) invites nominations from the members of the Linguistic Society. Any member who wishes to suggest a distinguished foreign linguist as a candidate for honorary membership should send his suggestion to the chairman of the committee, before 1 September 1954. (Address: Hall of Graduate Studies, Yale University, New Haven, Conn.)

THE SECRETARY-TREASURER of the Linguistic Society calls the attention of all members to his new address, effective at once:

Archibald A. Hill, Secretary-Treasurer
1719 Massachusetts Ave., N. W.
Washington 6, D. C.

PROBLEMS OF ARMENIAN PHONOLOGY I

WERNER WINTER

University of Kansas

THE PHONETIC VALUE OF OLD ARMENIAN *p' t' k'*

1. It is generally taken for granted that the present-day pronunciation of Arm. *p' t' k'* represents the original status: Meillet (*Esquisse*² 23) simply states this view without discussion: 'Il y avait trois séries d'occlusives ... chacune d'elles existant sous forme de sourde non aspirée, de sourde aspirée (c'est-à-dire où l'explosion était suivie d'un souffle) et de sonore'. In spite of the general agreement upon this matter, it seems worth while to take it up again and test the common view of *p' t' k'*, since this view appears to be based only on the present-day situation.

2. There are two ways to investigate the phonetic value of sounds in languages of the past. One is to study the reflexes of these sounds in alien systems of writing as well as in the indigenous orthography, together with the reflexes of sounds in other languages, whose phonetic value is known, in the system of writing to be studied. The other way is to determine the phonemic value of the sounds in question and from this to draw conclusions about their phonetic character.

3. The first way might appear, at first glance, to be especially promising for Armenian. The language contains a vast number of loanwords from Greek, Syriac, and various Iranian idioms, and for most of these we know in some detail how they were pronounced in the languages of their origin. What we do not know—and this seriously impairs the value of such loanwords as sources of information—is how much sound substitution there was in fitting the new words into the Armenian pattern. From other sources we know that in the Greek pronunciation of the first centuries after Christ, *χ* was a spirant; but we cannot infer from this that the *k'* by which the Armenians represented this sound (*K'ristos*) was anything but an aspirated stop, as it is nowadays, and is assumed by the current theory to have been then. We cannot exclude the possibility of sound substitution: if Armenian had no adequate spirant to express the sound of Greek *χ*, it could very well have been replaced by a stop.

4. It seems, therefore, more suitable to look over recordings of Armenian words in Greek script than vice-versa, since we know fairly well what the Greek pronunciation was at any given time. For our particular problem, Greek is of course unable to prove or disprove Meillet's assumption that *k'* was [kh] as long as we draw our materials from sources of a period when Greek itself still had a pronunciation [kh] for *χ*. At that time, an Armenian [kh] would have been represented by Greek [kh], while an Armenian [x] could have been expressed only through sound substitution, e.g. by [kh] or [h]. Thus, *Καρδοῦχοι* (= *Korduk'*) and *Τάοχοι* (= *Tayk'*), recorded by Xenophon, cannot be used for our purposes.

The value of Greek sources is far greater from the period when *χ* was no longer pronounced as an aspirated stop but as a spirant. This change came to an end

by the 3rd century A.D. (cf. Schwyzer, *Griech. Gramm.* 1.205); we may evaluate all materials after this date with great confidence. If a Greek now had to transcribe Armenian [x], he would of course choose χ, while for an aspirated stop [kh] he would be more likely to choose κ. According to Marquart, *Über den Ursprung des arm. Alphabets* 56 n. 1 (Vienna, 1917)—though his further conclusions as to the pronunciation of Greek are untenable—in Greek texts of the 7th century A.D. and later, Armenian *p' t' k'* are actually transcribed by π τ κ, while earlier Greek writers have only φ θ χ. For the second part of Marquart's statement, cf. cases like Τζοφηνή, Τζοφανηή for *Cop'k'* in Justinianus, Χοθαῖται for *Xoyt'* in Georgius Cyprius (around 600 A.D.), and Λιφίννιοι (*Lp'ink'*), Θορδαν (*T'ordan*), Χαρχαρεών (*K'ark'eay*) in the Greek version of the Agathangelus, ed. P. de Lagarde, *Göttinger Gel. Abh.* 35:1 (1887); the corresponding Armenian text belongs to the 5th century, cf. Hübschmann, *IF* 16.197. (Some of the examples mentioned here were collected by Hübschmann, *Arm. Gramm.* 1.403 f.)

Unfortunately, because of the vagueness of Marquart's quotations, I find myself unable to check the evidence for the first part of his statement. If correct, this would entitle us to conclude that around the beginning of the 7th century, Arm. *p' t' k'* became aspirated stops. An argument to support the assumption is that in later loanwords from Greek we find χ no longer represented by *k'* but by š, cf. ἀρχιμανδρίτης > *aršimandrit*, ἀρχιεπίσκοπος > *aršiepiskopos*, μολόχη > *mološ*, χλωρός > *šloros* in texts probably composed in the 12th century (Hübschmann, *Arm. Gramm.* 1.328); but it seems to be impossible to establish the exact date when such a transcription came to be used.

5. Since this material does not provide us with conclusive evidence for the phonetic value of Arm. *p' t' k'*, it is necessary to study also the role which these sounds played in the phonemic pattern of Old Armenian.

6. *kat'n* 'milk' and *kit'* 'act of milking' can be related to one another only in two ways: either *i* < IE *ē*, and *a* < IE *ə*, which leaves the Armenian words without relation to any other IE forms; or *i* was developed from **e* before a nasal that was later lost, as was the nasal of **an* (or **am*) from IE *ṇ* or *ṇ* in *kat'n*. The latter assumption permits us to keep in mind a possible connection of these words with *kut'* 'harvest, vintage', with *u* from **o* before a nasal subsequently lost.

Leaving aside for the moment the origin of Arm. *t'*, we may seriously consider the possibility that Arm. *ka(n)*-/ *ki(n)*-/ *ku(n)*- before a dental represent different ablaut grades of IE *gem-*, which we find in Greek ὕγγεμος συλλαβή, Homeric γέντο 'he took', or in Church Slavic žeti 'press', (cf. Pokorny, *Etym. Wb.* 368). The meaning of *kut'* 'harvest' is closer to the Greek words, that of *kt'em* 'I milk' (: *kit'*) closer to the Slavic.

Other forms show the same lack of a nasal before a consonant pronounced nowadays as an aspirated voiceless stop. The outstanding example is *arcat'* 'silver'. IE *ṇ* (cf. Lat. *argentum*) is represented elsewhere by Arm. *an*, cf. *k'san* 'twenty'; to ascribe the lack of -*n*- in *arcat'* to the influence of *erkat'* 'iron' (cf. Hübschmann, *Arm. Gramm.* 1.424) is useless as long as this word is not clear etymologically. Rather, if the ending in *arcat'* can be explained, this can serve to explain *erkat'*.

Another case of a lost nasal before Armenian 'aspirated stops' is the first person plural of the aorist indicative and subjunctive. The explanation suggested by Pedersen (KZ 38.211 f.) is not quite satisfactory: he derives *-uk'* from an earlier **-ōes* supposedly formed on the analogy of the first singular **-ō*, but *-ak'* of the indicative from **-aes*. To explain the latter form, he has to build up a highly complicated theory. It seems much simpler to base the interpretation of *-ak'* and *-uk'* on forms actually existing in other IE languages. Taking into consideration a development like that of *dsterk'* < **-teres* (but leaving aside the question whether *-k'* merely replaced *s*, or actually developed from it), one comes to see that an ending **-omes* (preserved in Doric) would have yielded Arm. **-omk'* > **-umk'*, while an athematic **-mes* would have led to Arm. **-amk'*. Since the distribution of *-ak'* and *-uk'* is that of *ἐρέσασαυ* and *τείσσασαυ*, these endings are evidence for the loss of a nasal before *k'*.

The first plural pres. *-mk'* seems to be formed on the analogy of the first singular *-m*, cf. *duk'* 'ye' : *du* 'thou'. Meillet (*Esq.*² 122, 125) prefers to explain the lack of a nasal in *-uk'* and *-ak'* by the analogy of the corresponding forms of the first singular without *-m*. He fails, however, to offer an explanation for the *-a* in *-ak'*.

The assumption of a lost nasal, here proposed, is not disproved by the fact that *-n* is preserved in the nominative plural of *n*-stems: *-ink'*, *-unk'*. We may assume here that the *-n* was restored (or perhaps never lost at all) on account of the *-n* of the accusative plural: **-ēnns* had become *-ins* and **-ōnns* had become *-uns*, and this may have influenced the nominative, since we nowhere else find a contrast between nominative and accusative plural of the form vowel + *k'* vs. vowel + *ns*.

7. If a number of fairly transparent forms thus indicate that a nasal was lost before *t'* and *k'*, we have to check for evidence of such loss in other environments. An **n* was lost before *s* < IE *s* in the ending of the accusative plural: *ands* 'fields' < **-ons*, *hars* 'fathers' < **-ns*, *anjins* < **-nns*; before *s* < IE *k̑*: *hasanem* 'obtain' : Skt. *aśnóti*; before *z* < IE *ǵh*: *bazum* 'plenty' : Skt. *bahú-*. This means that **n* was lost before an Armenian spirant. As far as our evidence goes, we find no loss of a nasal before a stop, voiced or voiceless.

8. The only apparent exception to this statement being the treatment of **n* before *t'* and *k'*, we are led to the conclusion that these sounds were not aspirated stops as they are today, but spirants. This classification is of course a phonemic statement; but since the identification of *t'* and *k'* as spirants fits their treatment in Greek transliterations, we may conclude that the phonemic behavior of these sounds agrees with their phonetic value. In the older period of Armenian, until about the first half of the 7th century, they must have been spirants. It was only later that they changed to occlusives.

9. In more recent forms of Armenian we find the so-called aspirated stops patterning with spirants in other cases also. Thus, in Middle Armenian ('Cilician') and Modern Armenian, *r* tends to be lost before spirants (*h*, *š*, *ž*, *s*) and 'aspirates'; cf. Karst, *Grammatik* 94 ff.

As for *s*, *š*, and *h*, first traces of such a development can be found in Old Armenian: *tesanem* 'I see' : Gk. *ἐρεσασαυ*, *mah* (beside *marh*) 'death' : Skt.

mṛtyú-, *baš* (and *barš*) 'mane' borrowed from a form corresponding to Av. *barəša-* 'horse's back, neck'. Karst (97) assumes that the loss of *r* was peculiar to a popular language used beside the Schriftsprache. In Modern Armenian, the popular tendency would then have become normal usage.

Though the loss of *r* before an aspirate is not attested until the 'Cilician' period—that is, not until after the shift from [x] to [kh], we must assume that the development **rk* > *k* is older in its origins than this shift. After the shift had taken place, it would seem as if *r* was normally lost before an aspirated stop as well; and for this reason forms with an aspirate, secondarily developed in Middle Armenian, could lose their *r* analogically. An example is Old Arm. *ənd nerk'sē* 'within' > Cil. [ənt nekse], Old Arm. *drgal* 'spoon' > Cil. [tk'al]; cf. Karst 96. When this loss of *r* was transferred to forms without an old *k*, words with and without *r* came to exist side by side, but (according to Karst) as part of different forms of speech. In the light of such alternations, it is easily understood that even in forms with an old *k*, an *r* continued to exist or was reintroduced. It may be an argument in favor of Karst's interpretation that in some loanwords from Greek, in what seems like an attempt to use a particularly learned form, an *r* was added: *gramartikos* < *γραμματικός*, *markel* < *μακέλη*, *Barsel* < *Βασίλειος*; cf. Hübschmann, *Arm. Gramm.* 1.331.

Whether similar tendencies caused a spread of secondary *n* in some Cilician forms, is difficult to decide. [myenk'] < Old Arm. *mek* 'we' was probably transformed under the influence of the first plural present *-enk* < Old Arm. *-emk* (cf. Karst 102), not, as Karst prefers to think (104), by analogy to *-nk* in the nominative plural of nouns. Since, however, we do in fact find a secondary *-n-* in some Greek loanwords, even in Old Armenian (*menk'enay* : *mek'enay* < *μηχανή*, *klmindr* : *klmidr* < *κεραμῖς*; cf. Hübschmann, *Arm. Gr.* 1.331), an explanation like that of the *r* in *Barsel* seems worth considering.

10. The facts presented here oblige us to interpret *p' t' k'* as original spirants. However, one might argue for an interpretation of *p' t' k'* as affricates, on the basis of reduplicated forms like *t'awt'ap'em* 'shake', *šawšap'em* 'feel, touch', *sawšap'iwēn* 'murmur'. These words may be compared with *kaskac* 'suspicion, doubt', *kskic* 'sharp pain', *koškoč* 'beaten, destroyed', *pašpač* 'swelling, tumor'. Since, in the sequences *s—c* and *š—č*, the preconsonantal spirant of the reduplicating syllable corresponds to an affricate in the stem, the correspondent of *-w-* ought also to be a combination of occlusive and spirant; that is, *p'* ought to be something like [bv] or [pf]. One could add to such an argument that the Greeks, finding it difficult to render affricates in their own system of writing, transcribed them by simple spirants (beside the more complex transcriptions like *Τζοφνή* quoted above), as in *Ἀρσουργόνων* for *Arcruneac*, *Σουδαίων* for *Cawdēic*, *Ἀλσενών* for *Aljneac*, *Βαγαριζ* for *Bagayarič*, *Τασάτης* for *Tačat*, and *Καλαρσών* for *Kalarčk*, recorded in the Greek version of the Agathangelus.

Such assumptions can be dismissed. First, the relation of voiced *w* to voiceless *p'* is not the same as that of voiceless *s* or *š* to voiceless *c* or *č*. Second, since preconsonantal *f* in Iranian loanwords is replaced by *w* (cf. *tawt* 'heat' : Pers. *taft*), one may safely assume that an Armenian [f] (= *p'*) in such a position would also have become *w*. (My colleague Wolfgang Lentz of Hamburg Univer-

sity, to whom I owe many friendly comments and criticisms, points out that the change $f > w$ may have taken place in Iranian itself; but as long as there is no evidence for this assumption, I prefer to ascribe the change to Armenian.) The third and most important reason for rejecting the view that p' t' k' were affricates is that we find w as a correspondent not only of p' but also of t' and k' , and in complementary distribution with other allophones that cannot possibly belong to affricate phonemes. This will become clear from the next installment of this study, to be published in a later number of this journal.

OLD ENGLISH AND OLD NORSE SECONDARY PRETERITS IN *-r-*

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1. One of the fundamental changes from Proto-Indo-European to the Germanic dialects is that from a verbal system based on aspect to a system based on tense. By the time of our Germanic records the change was accomplished; aspect to be sure was still conveyed in the Germanic languages, but through morphological processes that were not consistently applied to all verbs, such as the *ga-* prefix. Tense, however, was a category marked in every verb. Moreover, unlike the PIE situation, where the imperfective (present) was sometimes marked to distinguish it from other aspects—a situation maintained in Gk. *tithēmi* 'I place' beside the unmodified stem form of the aorist *ēthēka* 'I placed'—in Germanic only the preterit was marked, with rare exceptions like Go. *standan* beside *stōþ*, *biðjan* beside *baþ*. Accordingly the Gmc. verbal system differed from the IE system in the grammatical meaning which it characteristically expressed, and in its method of marking that meaning.

The Gmc. verb system distinguished two tenses: present and preterit. The PIE verb system had distinguished a variety of aspects: imperfective, perfective, and punctual, for which forms were made from most roots, plus other aspects that were more sporadic, such as the frequentative and the causative; these distinctions were marked by a great variety of affixes and by various types of internal change. But between Proto-Indo-European and Proto-Germanic the forms marking verbal categories, as well as the number of possible categories, were greatly reduced. Corresponding to the relatively large number of aspects in Proto-Indo-European were two PGmc. tenses; corresponding to the relatively large number of morphological processes marking aspects were three marking the preterit: one a strikingly consistent pattern of internal change, found in the so-called strong verbs (e.g. Go. *niman* : *nam*); another, reduplication, attested only in Gothic (*hāitan* : *hathait*, *saian* : *satsō*); the third, affixation, primarily of a dental suffix, found in the so-called weak verbs (Go. *nasjan* : *nasida*). Compared with the amorphous situation in Proto-Indo-European, the Gmc. verb system is neat and economical.

2. As we might expect from general linguistic observations, aberrant forms occur beside the strong and weak preterits; a widely attested OE preterit for the inf. *lātan* is *leort*; the preterit generally used for the ON inf. *sá* 'sow' is *sera*. Historical linguists of the past so firmly accepted the conclusion that all Gmc. preterits are marked by one or more of the three predominant morphological processes, that they explained the aberrant forms as reflexes of older forms characterized by one of these processes. Since it was commonly assumed, until recently, that the NWGmc. 7th-class preterits were reduplicated forms somehow shortened,¹ forms like OE *leort* and ON *snera* were also explained as reduplicated,

¹ Of the many works on verbs of the 7th class, the following give good descriptions of the forms attested and bibliographical information: S. Feist, *Die sogenannten reduplicier-*

even though they contain other irregularities besides the unusual reduplication, and though no reduplicated forms are attested in North and West Germanic. In OE *leort* the root of *lætan* is considered to have been reduced to -rt-, and its unusual form accounted for by three statements: (1) the original root vowel was syncopated; (2) -l- of the stem was dissimilated to -r-; (3) the reduplicating vowel was diphthongized in the process known as breaking. ON *snera* is similarly explained as a reduplicated verb, modified from a hypothetical **se-sna* (which in turn developed from a still earlier **se-snow-a*) and shifted to the weak conjugation. We may wonder at the credibility of these complicated explanations, especially in the face of such clear forms as Go. *lat-lōt*. I shall review briefly the list of the irregular forms in Old English and Old Norse, and explore the possibility of another origin.

3.1. The OE forms which are said to be survivals of reduplicated preterits are *heht* from *hātan* 'command', *leolc* from *lācan* 'leap', *leort* from *lætan* 'let', *reord* from *rædan* 'advise', and *ondreord* from *ondrædan* 'fear'. Further information on these, such as different spellings, may be found in Sievers-Brunner, *Altenglische Grammatik*² 336 (Halle, 1951). There too are listed other forms which some scholars have regarded as reduplicated preterits: *speoft* 'spit' and *beoftun* 'beat'. I follow Sievers in deriving *speoft* from **speoftian* rather than from *spātan* 'spit', for beside it there is an attested pret. ptc. *gespeoftad*; the pl. *beoftun*, rather than *beotun*, is probably a scribal error, as is the corrected *blefla*. The preterits of the five verbs listed above, on the other hand, are adequately attested. Assuming further that *ondrædan* is a compound of *rædan*, we are left with four simple verbs.

All these irregular preterit forms occur in Anglian, or in poetic texts. It is agreed that the -eo- vowels are reflexes of earlier -e-; -e- became -eo- in *leort* and *reord* by breaking; the -eo- of *leolc* has been ascribed to velar umlaut, but this also will be attributed to breaking (below, §5.6). For all the OE verb forms which are supposedly reduplicated we find a general structure: CeCC, usually CerC.

3.2. The ON forms which are said to be survivals of reduplicated preterits are *bnere* from **bnúa* 'rub', *gnera* from *gnúa* 'rub', *grera* from *gróa* 'grow', *rera* from *róa* 'row', *sera* from *sá* 'sow', *slera* from *slá* 'strike', and *snera* from *snúa* 'turn'. For further information see A. Noreen, *Altisländische Grammatik*⁴ 340-1 (Halle, 1923), and A. M. Sturtevant, *SS* 23.64-5 (1951). The latter discusses the many difficulties created by the phonology of *gnera*, *grera*, and *snera* for those scholars who attempt to derive them from reduplicated preterits. We have no grounds for assuming a reduplication *gne-*, *gre-*, *sne-* in Germanic; and even if we do, we still have to account for the reduction of the stem to -r-. *Snera* presumably derives from *snúa* according to the pattern found in Go. reduplicated verbs, i.e. from **se-sna* or, if we wish to assume weak endings at this stage, from **se-snō se-znō*, and *grera* from **ge-gra* (**ge-grō*). Just how a form like **se-sna* could

enden Verba im Germanischen, *PBB* 32.447-516 (1907); C. Karstien, Die reduplizierten Perfekta des Nord- und Westgermanischen, *Giessener Beiträge zur deutschen Philologie* 1 (Giessen, 1921); E. Prokosch, *A comparative Germanic grammar* 176-82 (Philadelphia, 1939; abbr. *CGG*). See also Donald A. McKenzie's pertinent addition to the last in *JEGP* 39.261-3 (1940). Also useful, especially for bibliography, is J. Janko, Über germanisch e^a und die sog. reduplizierenden Praeterita, *IF* 20.229-316 (1906-7).

have developed to *snera* is a difficult problem; Sturtevant assumes a metathesis of the *-n-*, after which the change of *-s-* to *-z-* to *-r-* would be credible. Yet there is little reason to assume such complex phonological changes in Old Norse, other than the supposed necessity of deriving these forms from reduplicated verbs.

The forms in question occur throughout the ON texts. Later modifications listed by Noreen, such as the forms with rounded vowels (*rþra* beside *vera* etc.) are regular in Old Norse; *-e-* became *-ø-* by *u*-umlaut in the plural forms, and presumably was sometimes extended to the singular. Moreover, the *-e-* following the initial consonant is readily accounted for; it would not have been diphthongized (broken) after the *r* of *vera* and *grera*, nor in the other verbs in the 2d sg. For the ON verbs we find, then, a structure *Cer-*, in which *C* may be a cluster.

3.3. Common to all of these forms but OE *heht* and *leolc* is the element *-r-*. Furthermore, these verbs are inflected in Old Norse with weak endings *-a*, *-er*, *-e*, rather than the strong *-t*, *-i*.² It may be, then, that these OE and ON verbs are secondary (weak) preterits with a suffix *-r-* rather than *-d/t/p-*. To substantiate this hypothesis we must establish the likelihood of such a weak conjugation, and suggest a plausible source for the characteristic element *-r-*.

4. It is now pretty generally accepted that the IE verb system distinguished aspect, not tense; see Meillet, *Introduction*³ 196-7, and even such a conservative Indo-Europeanist as Hirt, *IG* 4.165. The form markers too are well explored, though no one theory of their origin is generally accepted. In the early period of IE studies, many scholars favored the view that these had developed from full words, such as auxiliary verb forms or pronouns. The preference for this type of explanation probably resulted from a lack of acquaintance with developments in other languages and from initial observations made in the Romance group, the laboratory for historical linguistic methodology. Because some Romance inflectional categories, e.g. the French future, had developed from periphrastic formations, it was assumed that this was the general method of forming new inflections. Bopp, *Über das Konjugationssystem der Sanskritsprache* (Frankfurt, 1816), found a source for many of the IE inflectional categories in periphrases of various sorts; the *s*-aorist, for example, he derived from the root *es* 'to be' (18 f.), and the verbal endings from pronouns (147-51). This theory of derivation has been remarkably tenacious; Hirt still favors Bopp's suggestion for the origin of the *s*-aorist, *IG* 4.253.

It was only later that linguists came to assume that no one methodology was applicable to all languages, that one could not safely infer from one language what the course of development would be in another, that the phenomena of every language must be explained in terms of its own structure. In seeking to explain any new formation, the various substructures must be taken into account—of pertinent phonemes, of pertinent forms, of pertinent semantic relations. It no longer seems adequate to suggest, like Bopp, that the *s*-aorist was

² The OE forms on the other hand have strong endings. In Old English the *r*-preterits were preserved in only one dialect, and even here in only a few verbs. Weak endings may have existed formerly in these verbs; since the *r* verbs were unproductive, strong endings may have been introduced by analogy with other 7th-class verbs.

constructed by means of the verb *es-* without concerning oneself with the morphological possibilities of the language at the time of the assumed construction. We would hardly expect, say, new morphological categories of inflection arising in English to employ prefixation; for we accept the general principle that linguistic changes are limited by the structure of a language, and prefixation is not used as an inflectional marker in English.

We assume further that limitations on linguistic change continue as the structure is modified. Since modifications of structure are gradual and circumscribed in scope, it has seemed attractive to some linguists to speak of currents of linguistic development. The locus classicus is possibly Sapir's discussion in his book *Language* 157-82 (New York, 1921). Sapir introduced for such currents the term *DRIFT*; but in this book, which was designed for the general public as well as for linguistic students, he often uses metaphors. Others have unfortunately hypostasized drift: to a drift which has been inferred from observing one pattern of changes, further changes may be ascribed; the drift observed becomes an agent in a social convention. Prokosch supports his procedure by reference to Sapir when he states, *CGG* 34, that virtually all of the Germanic changes 'follow the same "drift"'. The Germanic drift 'came to a standstill', and exhibited other personal characteristics, *CGG* 34; 'it exerts a stabilizing, or a modifying influence', *CGG* 36. An extreme instance of the hypostasis of drift is the so-called *détresse phonologique*, which according to some linguists inaugurates linguistic changes. Thus H. Lausberg, *Détresse phonologique und Mehr-lautphoneme*, *Archiv* 187.66-70 (1950), ascribes to this factor 'Notwehrmassnahmen der bedrohten Systeme'. Such a view of patterns of change we reject utterly, for we view language as an element of human culture, not as a separate being or force. Yet we know that in every language there is a repertory of allophones, phonemes, morphemes, grammatical processes, and so on, and that the speakers of a language are limited to the items available in that repertory. In attempting to provide an explanation for some linguistic change, it would be unrealistic to offer one, however plausible it might be in a different language, for which there is no evidence in the language under investigation.

Oddly enough, Franz Bopp and the other early 19th-century linguists seem to have followed this principle, though their views on the historical setting of Proto-Indo-European, and consequently their explanations, are quite different from ours. They viewed language as a recent product of man, which had undergone remarkably great structural changes in a relatively short time: in its early stages language consisted of essentially one word per idea; a more highly developed stage combined words into complex, inflected forms; in the primitive period of Indo-European, forms like Skt. *vásmi* 'I desire' had consisted of individual words—the etyma of *vás* 'desire' and *mi* 'I'. Inflections accordingly developed from agglutinated words; composition theories harmonized well with this view of Indo-European and of man's use of language.

Today the view that Proto-Indo-European was a primitive language is virtually abandoned, along with the view that in reconstructing earlier stages of Pre-Indo-European we can work back to the infant stages of *homo loquens*. Yet the old notion that IE forms developed from separate words is still widely

held, and is even the most prominent view set forth in our standard handbooks. Hirt, to be sure, has rejected Bopp's suggestion that the IE verbal endings are derived from personal pronouns, *IG* 4.148; but for the origin of the *s*-aorist he still finds composition with the root *es* the most attractive theory, *IG* 4.253, on the ground that this solution corresponds to the situation that we so often find in later times. Other scholars still favor a composition theory for the origin of the IE secondary verb forms, notably the preterits.

In the 19th century the view was held that the secondary tenses of the IE dialects developed from compounds, of which the first element was a nominal form of some sort, the second an auxiliary. By such theories Go. *habaida*, for example, is compounded of a noun element *habai-* and a verb element *-da*; for a brief review of the various theories and references to theories on similar formations in other dialects see *Lg.* 19.19-23 (1941). Inability to identify either element gradually disturbed the credibility of such theories. Recently Paul Kretschmer has suggested an origin in periphrasis, *Objektive konjugation im Indogermanischen* (Öster. Akad. d. Wiss. Phil.-hist. Kl. Sb. 225, 2. Abh., 1947). Kretschmer seeks to explain the characteristic formant of various secondary preterits—among them the *s*-aorist, the Gmc. weak preterit, the Lat. *v*-perfect, the Gk. *k*-aorist and perfect—from incorporated pronouns: the pronominal element was added to the verb as an affixed object. As support for his theory Kretschmer adduces similar tenses in Finno-Ugric, Caucasian, Basque, and other languages. Yet the presence of such formations in a thousand languages would not be evidence for Indo-European. From the linguistic work of the past century it is clear that one could support almost any theory with individual or capricious comparisons between languages, and that such comparisons accordingly are valueless without a further check. This check we obtain from the structure of the language in question. In the IE period, for example, we expect to find ready use of possessive compounds, but relatively few adjectival compounds; in the Gmc. languages of about 1200 the situation is reversed. The compounds that we find in Finno-Ugric, Caucasian, and Basque tell us nothing about the IE situation.

Unfortunately Kretschmer's suggestion was favorably reviewed by Lane, *Lg.* 27.370-2 (1951), on the ground that Kretschmer at least provides a 'uniform solution' for the origin of the diverse secondary formations in the IE dialects. But a uniform theory was proposed long ago, among others by Persson, *Beiträge* 558-9—a theory which Lane oddly labels 'piecemeal partial analyses'. Persson's 'uniform solution' is suffixation; the formants of the secondary conjugations were suffixes which came to be restricted to a specific form category. From comparison of the various IE dialects we can posit many suffixes in Proto-Indo-European and Pre-Indo-European. In some of them a characteristic meaning became predominant, and the suffix spread throughout a set of verbal or nominal forms; the diversity of suffixes no more impairs the uniformity of Persson's suggestion than does the diversity of Kretschmer's pronominal elements. To support Persson's view, we must examine the productive types of verb formation in the late IE period, note whether we can establish examples of tense formants spreading from suffixes, and decide whether this type of spread is reasonable in the structure of Indo-European and of the early dialects.

In the early period of the IE dialects there were two productive means of

distinguishing inflectional categories: internal change and suffixation. The use of both processes extended to various periods in the dialects, in those of the Gmc. group at least as late as 1200 A.D., so that we may find in them relatively late examples to clarify the early dialect situation. Compound tenses, such as *I am going, I have gone*, can be established only for the last part of this period, e.g. for Classical Sanskrit, for Latin, for the dialects after Proto-Germanic. Some means, on the other hand, were no longer productive in late Proto-Indo-European, e.g. the development of diverse endings such as *-mi, -m, -ō, -xe*, which earlier had distinguished different types of 1st singular. About the origin of the internal change there is no dispute: it was ablaut. At the time of their origin, the ablaut changes were purely phonological; only later did they come to be used to distinguish morphological categories, such as the tenses in Germanic and certain types of derivatives in Sanskrit. In this respect their history is similar to that of the umlaut vowels in German, which originally developed in particular phonetic environments, but which acquired a morphological function only after the phonological shift was completed. The umlaut vowels were associated with noun plurals, as in Ger. *Gast : Gäste*, and a similar contrast was later introduced in other nouns, such as *Acker* and *Arzt*, in which umlaut never took place. In the same way, the ablaut changes came to be so closely associated with morphological categories that they could be extended to verbs in which they had never occurred, as to OHG *scriban*, borrowed from Lat. *scribere*; others are probably 2nd-class verbs like Go. *lūkan, sūgan*.

A similar development may be assumed for the suffixed tense characteristics. These came to be phonologically distinctive, and were then adapted to morphological purposes in distinct inflectional categories. Again we may cite an example from Germanic. By the time of Old High German, the IE *s*-stem nouns with long root syllable had lost the final suffix in the nom.-acc. sg., but not in the nom.-acc. pl. (OHG nom.-acc. sg. *kalb*, pl. *kelbir*); the loss was purely phonological, and is ascribed to the Gmc. 'laws of finals'. The sg.-pl. contrast was marked in most OHG nouns by some type of suffix. Since *-ir*, by a phonological accident, was one of these suffixes, it came to be used as a plural suffix, and was then extended to many words which had not previously had an *s*-suffix, e.g. Mod. Germ. *Haupt* (cf. Lat. *caput*), *Nest* (cf. Lat. *nīdus*); for many others see H. Paul, *Deutsche Grammatik* 2.23-33. A 'meaningless' suffix thus developed into a grammatical marker.

I assume a similar origin for the IE secondary verb markers. But since our records date only from the time in which such secondary forms are full-blown, I cannot prove this theory for most of the secondary conjugations. Possibly the formation whose origin is clearest is the Armenian *-č*-aorist, in which Kretschmer did not even try to find a pronoun. Meillet derives this from the IE *sk*-suffix; in Armenian the suffix came to be used after verbal themes ending in vowels to mark the aorist; see *Esquisse d'une grammaire comparée de l'Arménien classique* 85-6 (Vienna, 1903). In other dialects the suffix had a different meaning, or, as in Germanic, none whatever. The origin of the markers in the secondary preterits of other languages cannot be so precisely identified, but more and more scholars seem inclined to assume the spread of a suffixal element. Meyer makes such an assumption for the Latin perfect; Must and Fourquet explain the Gmc.

dental preterit formant as a distinctive consonantal element that later spread.³ Though the source suggested for this element may be unconvincing, as in Must's theory, it is worth noting that many linguists now favor this type of explanation.

Of the two productive methods of tense formation in the early IE dialects, this one was the more widespread; it probably existed in greater variety than the surviving records show. A broad survey of the dialects indicates that in Germanic a *d*-formant was the only preterit marker, in Latin a *v*-formant, and so on. But other elements may have been used as well, which were gradually eliminated; in Germanic these may have had less connection with the normal dental formant than the *s* of Go. *wissa*, the *þ* of Go. *kunþa*, or the *t* of Go. *gamostedum*.

I suggest, now, that among these diverse elements was the one that appears in the *r*-preterits of Old English and Old Norse.⁴ In early Proto-Germanic, the inherited means of distinguishing the tenses, namely the difference in endings, was inadequate; and ablaut variations were confined to a relatively few verbs. On the other hand, the opposition of present vs. preterit was of great significance—as great as that of singular vs. plural was to be later in Old High German. Some means was needed to mark the tense distinction; in the verbs listed above in this paper, the means chosen was a suffix *r*. It will be of interest to determine its origin.

5.1. The OE and ON *r*-preterits are found chiefly in verbs whose roots originally ended in laryngeals or had laryngeal extensions. This statement follows from Prokosch's analysis of the Gmc. 6th and 7th classes as laryngeal bases; cf. CGG 151, PIEP 69–70. We can further support it by citing, for all except the first two of these verbs (those from which the formation spread), parallel forms from other dialects which give evidence of IE laryngeals.

5.2. For OE *hātan* we have no certain extra-Gmc. cognates. WP 1.362 tentatively suggest derivation from IE *keXy-* 'set in motion', as in Lat. *ciēō, ciere*; Feist hesitates to accept this etymology.

OE *lācan* is cognate with Go. *laikan*. These are related by Feist, GEW 319, 583, and WP 2.399–400, to Skt. *rejati* 'makes to hop', Gk. *elekizō* 'swing'. None of these cognates gives evidence of laryngeal. For *lācan*, alone of the OE and ON verbs with *r*-preterits, there are no attestable forms supporting Prokosch's analysis.

OE *lātan* is related by WP 2.394–5 to Lith. *lėidziu* 'let', Gk. *ledeîn* 'be tired', and Lat. *lētum* 'destruction'. The root accordingly is *leʔ-*, which may have had a *-y*-determinative, as well as forms extended further by *-d-*; cf. PIEP 70. The expected preterit is from *leʔyd-*.

OE *rādan* 'advise' (*ondrādan* 'fear') are related by Feist, GEW 199, and WP 1.69–76, to Skt. *rādhyati*, Lat. *rērī*. The latter is a reflex of the root with *-eʔ-* suffix; Skt. *rādh-* and OE *rādh-* reflect the suffixed root with *dh*-determinative, *reʔdh-*.

³ Karl H. Meyer, *Das lateinische Perfektum auf -vi, -ui und die slavischen Verba auf -vati*, IF 61.29–39 (1952); Gustav Must, *The origin of the Germanic dental preterit*, Lg. 27.121–35 (1951); id., *Again the origin of the Germanic dental preterit*, Lg. 28.104–6 (1952); J. Fourquet, *Anglo-Saxon éode, dyde, et la théorie du prétérit faible*, *Studia neophilologica* 14.420–6 (1941–42).

⁴ See W. P. Lehmann, *Proto-Indo-European phonology* 56–61 (Austin, 1952; abbr. PIEP) for a similar treatment of OHG *r*-preterits.

5.3. In Old Norse the *r*-formation became productive. Thus, *slá* is a regular 6th-class verb with the forms *slá sló slógom sleginn*; but in the infinitive it lost its final *h*, and the resultant form *slá* came to pattern like *sá sera*. A preterit *slera* was made from it, to take its place beside the regular form *sló*. If *slera* is analogical, other *r* forms may be also. Noreen, *Altisl. Gram.* 340, considers *grera* analogical after *rera* and *gnera*, and the hapax legomenon *bnere* presumably analogical after *snera*. Heusler, *Aisl. Elementarbuch* 92, assumes that even *snera* is analogical. On the other hand, Sturtevant, *SS* 23.64-5, suggests that the attested shapes of *snera* and *gnera*, though analogical, were modified from earlier reduplicated forms. Whatever our views, we can scarcely assume that more of these *r* forms than *grera*, *rera*, *sera*, and *snera* belong to the original *r* inflection. For these we can suggest evidence of laryngeal in the root.

Grera, preterit of *gróa* 'bud', is derived by WP 1.645-6 from the *ghrō-* which is evidenced in Lat. *grāmen* 'grass'. Whether or not we identify the unextended root with IE *gher-* (WP 1.606), we may assume a root for *gróa*, *gher-* with a suffix *-eX-*.

Rera, preterit of *róa* 'row', is derived by WP 1.143-4 from the root found in Skt. *ari-tra-*, Lat. *rē-mus* 'oar'; these forms, like Lith. *irti* 'row', point to a laryngeal extension. We may assume the PIE root **er-*, from which *róa* is made with the suffix *-eX-*.

Sera, preterit of *sá* 'sow', is derived by WP 2.459-63 from the root found in Lith. *sėju, sėti* 'sow', Lat. *sēmen* 'seed'. However one may analyze the complexities of Go. *saian*, *sá* must be derived from PIE *seX-*.

Snera, from *snúa* 'turn', is less readily related to extra-Gmc. cognates. Feist sees no difficulty in connecting it with OCS *o-snovę* 'found', but further relationships are uncertain. Feist suggests among other cognates those with which WP 2.694-5 tentatively associate it: Skt. *snāyati* 'winds about', *snāvan-* 'cord, sinew'. For these we assume PIE *sn-eX-*, with possible further extension, or (if we admit relationship with Lat. *nēre* 'spin') more precisely *sn-eX-*. *Snúa* seems to be derived from **snowan*, cf. OE *snēowan*; *snera* can be derived from the root unextended by *-w-*, i.e. *sneX-*.

5.4. Two of the OE *r*-preterits and four of the ON *r*-preterits are thus analyzed as indicating the presence of a laryngeal in the stem. From Prokosch's theory of the Gmc. verb, we would expect this structure; further, we would expect that the preterits of these verbs are constructed from the *e*-grade. Accordingly we assume for the OE preterits a structure similar to that of *rādan*, CeXC-, and for the ON preterits a structure similar to that of *róa*, CeX-. In the attested OE and ON preterits we find the patterns CerC- and Cer-. Assuming that the phonological development of *-X-* to *-r-* is plausible, we can ascribe to this origin the *-r-* which in a few verbs came to characterize a 'secondary' preterit formation. In Old Norse the type spread further, but was eventually lost in favor of the dental preterit and the ablaut verbs.

5.5. The development of some PIE *-X-* to Gmc. *-r-* has been suggested in *PIEP* 58-61. It was also proposed there, 70-1, that the NWGmc. preterit stems of verbs like *lātan* developed from a contrast of the following type:

leXd- > *lēd-* > *lāet-* > *lēt-*
leXyd- > *leXyd-* > *lēyt-* > *lēt-*

We assume that the contrast present vs. preterit was marked by the difference in vocalism, as in *lēd-* : *leXyd-*. Normally this developed further to a contrast between *ē* and *ē̄*; but in some forms the laryngeal may have survived and fallen together with the *r*-phoneme, giving rise to **lert*, whence *leort*.

A similar contrast existed between the present and preterit of *sā* 'sow'. As in *lāta*, OE *lātan*, a low front vowel was generalized in the present; since *sā*, like *lāta*, is a 7th-class verb, we can assume that it had preterit forms similar to those of *lātan*—that is, forms with a laryngeal. Most of these were lost; in the WGmc. dialects, regular weak preterits were generalized. In Old Norse, however, *-r* was used as preterit marker, and the weak endings were added after it.

5.6. If we make the assumption that *r* was the characteristic marker for the cited OE and ON verbs, we are left with two aberrant forms to explain, *leolc* and *heht*. The vocalism of *leolc* has long been difficult; since it is an Anglian form, it should have retained *-e-* before *-lc*, cf. Angl. *melcan* : WS *meolcan*. The aberrant *-eo-* has been explained by analogy with *reord*; see Karstien 149 for this suggestion, and for the difficulties in such an explanation. On the other hand, if *leolc* developed by assimilation from **leorc* < **lerc*, the vocalism is no longer troublesome; *e* > *eo* before *rC* here as in *leort*. It is as plausible to suggest that *-r* became *-l-* by assimilation as to posit the opposite change; and the suggestion has the advantage of making the vocalism regular. The *-h-* of *heht*, *hehtes*, etc. must be a result of analogy with the relatively numerous verbs of the pattern *weccēan wehte* 'wake', especially with the semantically similar *reccean* 'tell': except for the 1st and 3rd sg., these forms would have rimed.

6. I have now suggested an origin for the preterit marker of the OE and ON *r* verbs; but I do not consider my statements on the source of the *r* the essential part of this study. Primarily I have been concerned to show that the *r* should not be attributed to reduplication, but rather to the type of secondary conjugation which became productive in the early IE dialects. As for the *r* itself, a more plausible origin may be suggested elsewhere. The discussion above is an effort to lay the starred reduplicated preterits that still stalk through our handbooks in treatments of the North and West Germanic strong preterit.

The data which have come down to us make it probable that reduplication was confined to East Germanic. No reduplicated forms are attested in any of the other Gmc. dialects, except the supposed reflexes discussed in this paper. Even in Gothic some preterits and the preterit-presents lack reduplication. It is possible that in the interval between Proto-Indo-European and Gothic a number of reduplicated forms were maintained, in verbs like *áukan* 'increase'. The type may then have been extended in Gothic because the phonological development of that language precluded a distinction between present and preterit based on the contrast of *ē*¹ vs. *ē*², which was so widespread in the other dialects. We can no longer assume that Gothic, with all its innovations, preserves the most archaic features of Germanic, nor that the tremendous changes from the IE to the Gmc. verb system took place in accordance with regular patterns. Numerous devices must have been in use to distinguish formal categories, and many of these must have been lost as other formations became more and more distinctive.

ASPECT IN GOTHIC

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1.1. It has been generally held that Gothic possessed a formal aspectual system which was very much like the system in OCS.¹ In each language a simple imperfective is thought to have become perfective by the addition of a preverb—in Gothic especially by the addition of *ga-*, which in most instances did not change the meaning but merely the aspect of the basic verb (PIAG 103). And in each language every perfective verb, simple or derived, had future meaning in the present tense; though in Gothic, because there was no equivalent to the OCS iterative, a perfective verb also had present meaning. This tense ambivalence of the Gothic perfective present has been referred to as a flaw in an otherwise clear-cut aspectual system.²

When formulated to apply to Gothic alone, the above view may be said to assert, first, that there was in that language a one-to-one correspondence between aspect and form, and second, that there was also a rather close correlation between aspect and tense. Here we shall examine only the first of these assertions.

1.2. Four weaknesses cast serious doubt on the prevailing view and thus justify a fresh examination of the problem.

1.21. The initial assumption of close resemblance between Gothic and OCS in aspect has nowhere been properly tested; sporadic reference to OCS, introduced merely to confirm but never to oppose some detail of analysis,³ cannot be

¹ For an evaluation of the literature on aspect in Gothic, see Wilhelm Streitberg, *Perfective und imperfective actionsart im gotischen (PIAG)*, PBB 15.77–80 (1889); id., *Gotisches Elementarbuch*⁶ (GE) 194 (1920). Cf. Alfred Senn, *Verbal aspects in Germanic, Slavic, and Baltic*, Lg. 25.402–9 (1949). Anton Mirowicz, *Die Aspektfrage im Gotischen* (1935), controverts Streitberg's generalization about the perfectivity of Gothic compounds.

The prevailing view is essentially the one which Streitberg expounds in PIAG 70–177. He maintains it despite the strictures of Anton Beer (GE 194), and repeats it, in condensed form, in GE 194–202. It is this view that Streitberg applies to his two editions of the Gothic Bible (1908 and 1919) and to his *Gotisches Wörterbuch* (1908), now Part 2 of his *Gotische Bibel*. It is also the view which others have adapted to their analysis of aspect in Old and Middle High German (Senn 407 and fnn. 12–14), and which Senn uses as a basis for his diachronic extrapolation (Senn 409).

For a bibliography on aspect in the Slavic languages, see H. Chr. Sørensen, *Aspect et temps en slave* 186–88; C. H. van Schoonfeld, *The aspect system of the Old Church Slavonic and Old Russian verbum finitum byti*, Word 7.96–103 (1951); James Ferrell, *The meaning of the perfective aspect in Russian*, Word 7.104–35 (1951).

² Thus PIAG 122: 'Warum haben nicht alle perfectiven praesentien im gotischen futurischen sinn gleichwie im slavischen? Die frage ist berechtigt. Ihre beantwortung führt uns zugleich zum wundesten punkt des ganzen gotischen verbalsystems: nämlich dem schweren mangel, der in der nichtausbildung einer iterativkategorie liegt ... Dieser mangel an iterativen vereitelt ... die consequente ausbildung des gotischen verbalsystems analog dem des slavischen und verleiht ihm einen nicht in abrede zu stellenden unklaren und schwankenden character ...'

³ PIAG contains only four references to OCS, each supporting Streitberg's analysis: 85, in connection with the imperfectivity of *ligan* 'lie', *sitan* 'sit', *standan* 'stand'; 91, to prove

considered a test. What was needed was an analysis of every Gothic form and of its counterpart in Slavic; for only in this way would we really have known to what extent Gothic and OCS were alike and to what extent they diverged. In the light of my own study I may assert that such testing would have shown the initial assumption to be untenable.

1.22. Another weakness derives from the generally accepted notion that the aspectual system of OCS and the systems of the descendant Slavic languages are essentially alike, since in either of them every simplex becomes perfective through prefixation, and every iterative is imperfective.⁴ Yet this notion does not stand up under scrutiny. Although prefixed OCS forms invariably referred to the future when used in the present tense, they were not always perfective;⁵ and, contrary to accepted opinion, not all iterative forms were imperfective, although they always referred to the present (cf. §2).

Streitberg⁶ was of course unaware of these divergences from the prevailing notion, and should not be blamed for accepting the aspect system of 10th-century OCS, in the form in which it was known to him, as a fixed term of comparison. Yet it was this acceptance that led him to stress the importance of an iterative category in Gothic, to insist that the correlation between aspect and form was essentially alike in both languages, and implicitly to accept the analysis of Gothic aspect in terms of another language rather than of Gothic itself.

1.23. A third weakness results from the belief that a given Gothic verb had but one basic meaning in all its occurrences and, correspondingly, one basic aspect, either imperfective or perfective. An examination of every verb occurrence has shown, however, that in terms of the relation between meaning and aspect there were in Gothic four kinds of verbs: some had only one overt meaning and one aspect, e.g. imperfective *auhjon* 'make noise'; some had more than one meaning but still only one aspect, e.g. imperfective *fijan* 'abhor, hate'; others, like *frijon* 'love', had only one meaning but aspects that varied with the context (§ 2.11); and some had more than one meaning and also more than one aspect, e.g. *bairan* 'bear, lie in travail', used imperfectively, but *bairan* 'give birth to', only perfectly.⁷

the perfectivity of *gafraujinond* 'exercise lordship' and *gawaldand* 'exercise authority'; 94, to indicate the perfectivity of *gagaggan* 'sich versammeln'; and 116, to render OHG *gefullen* 'fulfill'.

⁴ Thus Senn asserts (402) that 'The system of verbal aspects in Old Church Slavic is in principle identical with that now operating in Great Russian, Ukrainian, White Russian, and Polish'; hence, by classifying as imperfective (407) both primary and secondary Russian iteratives, he also includes the OCS iterative in the same category.

⁵ See, for instance, the imperfective *thou shalt love ... hate* (Gk. ἀγαπήσεις ... μισήσεις) in *Thou shalt love thy neighbor, and hate thine enemy* (M 5.43), each of which is rendered in OCS by the prefixed forms *vŭzljubiši ... vŭznenavidiši* but elsewhere by an appropriate imperfective: Pol. *będziesz miłował ... nienawidził*: Russ. *ljubi ... nenavidi*: Croat. *ljubi ... mrzi*.

⁶ Cf. fn. 1.

⁷ Note perfective use of Goth. *bairan* in *ip Aileisabeip usfullnoda du bairan* (Gk. τέκεν: OCS *roditi*) *jah gagar* (Gk. ἐγέννησεν: OCS *rodi*) *sunu* 'Now Elisabeth's full time came that she should be delivered; and she brought forth a son' (L 1.57); also in L 2.6.

Clearly, the assumption of a one-to-one relation between a given verb and its aspect is an oversimplification. Yet this assumption underlies the attempt to prove perfectivization of initially imperfective Gothic simplicia and, in the end, the resemblance between the aspectual systems of OCS and Gothic (PIAG 82-103, 126-35).

1.24. There is, lastly, the weakness which stems from two related but untenable claims: that Gothic lacked an iterative category, and that this lack was a flaw in that language. The first claim presupposes an identity between the aspect systems of OCS and of Gothic which, as we have seen, has not been demonstrated; indeed, the missing iterative was introduced ad hoc to round out the assumption of identity, which supposedly extends to every formal feature in the two aspect systems. The second claim is indefensible: by admitting a flaw in a structure, it denies that continuous intelligibility is an essential trait of language.

2.1. ASPECT. We shall examine Goth. *frijoda* 'loved' and *gaggiþ* 'walks, comes', each in two occurrences, and then refer to the aspect of Goth. *giban* 'give' and *niman* 'take'.

Goth. *frijoda* 'loved': Mk. 10.21:⁸ *Ip Iesus insaihvands du imma frijoda* (Gk. ἠγάπησεν : OCS *vūzljubi*) *ina* 'Then Jesus beholding him LOVED him'. Both Greek and OCS have here the aorist; OCS has the aorist of *vūzljubiti*, which is the perfective to *ljubiti* 'love'. Accordingly, Goth. *frijoda* 'loved' means here 'loved instantly' or 'began to love'; the emphasis of the verbal meaning is on its intensive quality at the point of its inception, and not on its duration or extension. — J 11.36: *þaruh gepun þai Iudaius: sai, hwiwa frijoda* (Gk. ἐφίλει : OCS *ljublēaše*) *ina* 'Then said the Jews, Behold, how he LOVED him'. Both Greek and OCS have here the imperfect; OCS has the imperfect of the simplex *ljubiti* 'love'. Goth. *frijoda* 'loved' means here 'used to love'; the emphasis of the verbal meaning is on its extensive (durative) quality, apart from its inception or any other termination.

Goth. *gaggiþ* 'walks, comes': J 6.45: ... *hazuh nu sa gahausjands at attin jah ganamaggiþ* (Gk. ἐρχεται : OCS *poidetū*) *du mis* 'Every man, therefore, that hath learned of the Father, COMETH to me'. OCS has here the perfective of *iti* 'go', with the meaning 'will come'. The emphasis of the meaning of *gaggiþ* is not on the duration or extension of the activity of walking as such but on its intension at the point of its termination. — J 10.4: ... *jah þan þo swesona (lamba) ustiuhþ, faura inaaggiþ* (Gk. πορεύεται : OCS *xoditū*), *jah þo lamba ina laistjand* 'And when he putteth forth his own sheep, he GOETH before them and the sheep follow him'. OCS has here the present of the imperfective simplex *xoditi* 'walk'. The emphasis of the meaning of *gaggiþ* is on the duration or extension of the activity of walking, apart from either its inception or its termination.

In each of these two sets of occurrences, we have a contrast in the emphasis of the verbal meaning: activity in its intension, at the point of inception (*frijoda* in Mk. 10.21) or of termination (*gaggiþ* in J 6.45) vs. activity in extenso, apart

⁸ The abbreviations for the Gospels and the Paulinian Epistles are, with the exception of Mk. for Mark, those which Streitberg uses in *Die gotische Bibel*.

from any termination (*frijoda* in J 11.36, and *gaggiþ* in J 10.4). It is to these two kinds of verbal emphasis that the term aspect applies: perfective (= pf) to the first kind, imperfective (= ipf) to the second.

Goth. *giban* 'give', *niman* 'take': To these verbs, and others like them (§§4.22; 5.11, 12, 21; 6.1, 2), there applies only an intensive emphasis which has no relation to extension or termination. We shall classify them as non-terminative perfectives (= Pf).

2.2. Aspect refers, then, to the emphasis that is implicit in a verbal meaning. We speak of perfective aspect when this emphasis is intensive, of imperfective when it is extensive. Some verbal meanings are used intensively AND extensively, others only intensively. On one hand there are such forms as *fall in love* (pf) vs. *love* (ipf), *sit down* (pf) vs. *sit* (ipf), *notice* (pf) vs. *look* or *see* (ipf), *beat* (ipf) vs. *finish beating* or *slay* (pf), *proceed* or *start walking* (pf) vs. *walk* or *go* (ipf) vs. *come* or *arrive* (pf); on the other hand there are such forms as *give*, *take*, *lose*, *find*, *perceive*, all of which are perfective but without reference to a specific termination, and hence Pf.

The formal means which correspond to these distinctions in verbal meaning vary from language to language. They may be lexical, as *beat* (ipf) vs. *slay* (pf); or syntactic, as *sit* (ipf) vs. *sit down* (pf), or Ger. *sitzen* (ipf) vs. *sich setzen*; or morphological, as OCS *biti* 'beat' (ipf) vs. *ubiti* 'slay' or *zabiti* 'kill' (pf), or Ger. *schlagen* (ipf) vs. *erschlagen* (pf).

2.3. To determine whether Gothic possessed a formally explicit aspectual system, we shall ask what, if any, were the morphological means corresponding to the aspectual contrast between intensive and extensive, and to the far fewer Pf meanings. Syntactical means we shall disregard completely.

3.1. ITERATIVE. This formal category, known from OCS and its descendants and generally classified as imperfective,⁹ is neither necessarily imperfective nor necessarily iterational. In our examination of the Gothic material, we shall therefore be justified in excluding any reference to an iterative and in limiting ourselves to the two phases of aspect.

The following pairs of OCS verbs, one the basic form and one the iterative, illustrate the relation between the iterative and aspect, and that between the iterative and iteration.

3.2. Iterative and aspect. (a) The aspect of an OCS iterative is Pf if its basic form is Pf, but is ipf in all other instances. Thus, *dajō* 'I give' in 'my peace I give' (Gk. *δίδωμι* : Goth. *giba*) unto you' (J 14.27) and *damū* 'I will give' in 'All this I will give' (Gk. *δῶσω* : Goth. *giba*) thee' (L 4.6) are both Pf, but *raždajātū* 'is in travail' in 'A woman when she is in travail' (Gk. *τεκεῖν* : Goth. *bairiþ*) hath sorrow' (J 16.21) or *krūštajō* 'I baptize' in 'I indeed baptize' (Gk. *βαπτίζω* : Goth. *daupja* : Pol. *chrzcze*) you with water' (L 3.16) are ipf, as against *roditū* 'shall bear' in 'thy wife shall bear' (Gk. *γεννήσει* : Goth. *gabairiþ*) thee a son' (L 1.13), which is terminational and pf, or against *krūstitū* 'will baptize', which is pf in 'He that ... is baptized' (Gk. *ὁ βαπτισθεῖς* : OCS *iže krūstitū sę* : Pol. *ochrzczi się*) shall be saved' (Mk. 16.16) but ipf in 'He shall baptize' (Gk. *βαπτίσει* : Pol. *będzie chrzczić*) you with the Holy Ghost' (L 3.16).

⁹ Cf. A. Vaillant, *Manuel du vieux slave* 306-7 (1948).

(b) An OCS iterative always refers to the present when it is in the present tense, just as its basic form always refers to the future; thus, the iteratives *dajō* 'I give', *raždajatū* 'is in travail', and *krūštajō* 'I baptize' all refer to the present, while the basic *damū* 'I will give', *roditū* 'shall bear', *krūstitū se* 'is baptized', and *krūstitū* 'will baptize' refer to the future.

3.3. Iterative and iteration. Iteration is not a necessary concomitant of the iterative, for (a) it is present in some but not in all iteratives, and (b) it is present also in some basic forms. Thus (a) it is present in *krūštajō* 'I baptize', but absent in *dajō* 'I give' and *raždajatū* 'is in travail'. Again (b), it is present in both *krūštajō* 'I baptize' and *krūstitū* 'he shall baptize', and in *iždenqitū* 'shall put out' in 'shall put you out (Gk. ἀποσυναγώγους ποιήσουσι : Goth. *dreiband*) of the synagogue [more than once, that is]' (J 16.12) as well as in the iterative *izgonitū* 'casteth out' in 'He casteth out (Gk. ἐκβάλλει : Goth. *usdreibiþ*) the devils through the prince of the devils [again, more than once]' (M 9.34).

3.4. In OCS verb morphology, then, the iterative, by always referring to the present as against the future of the basic form, was primarily one term in a temporal contrast, and only secondarily (since it was not always ipf) one term in an aspectual contrast. It had no automatic relation to iteration.

This excursus into the OCS iterative bears on the study of aspect in Gothic. It underscores the value of viewing the iterative in relation to the rest of the OCS system, and so leads us to the surmise that the Gothic system, in which there was no iterative as a formal category, must have functioned quite differently.

4.0. The material of this study is drawn from the Gothic Bible in Streitberg's edition.¹⁰ It consists of 535 different verbs, in an estimated total of more than 12,000 occurrences;¹¹ the bulk of these derives from only about half of all the verbs.¹² We shall examine this material for form and for aspect, but shall cite only as much of it as is needed to illustrate the argument.

4.1. Some Gothic verbs have a non-prefixed or simple base in all their occurrences, e.g. *finþan* 'recognize, perceive', *frijon* 'love', *audagjan* 'call blessed'. Others have only a prefixed base, e.g. *bi-gilan* 'find', *ana-biudan* 'command', *faur-biudan* 'forbid'. We shall refer to these as isolate forms: ISOLATE SIMPLICIA and ISOLATE DERIVATIVES. Other verbs have a simple base in some of their occurrences, a base with prefix in others, e.g. *bairhtjan* 'reveal' : *ga-bairhtjan* 'reveal', *beidan* 'wait' : *ga-beidan* 'endure', *us-beidan* 'look for'. We shall refer to these as aggregate verbs, each consisting of an AGGREGATE SIMPLEX and one or more AGGREGATE DERIVATIVES. The following sections will treat simplicia (§4.2), derivatives with *ga-* (§5.1), and derivatives with *P-* (§5.2).

¹⁰ Streitberg, *Die gotische Bibel*, (1. Teil², Heidelberg, 1919; 2. Teil, 1910); abbr. GB.

¹¹ This total has been arrived at by adding the 5947 Gothic compounds that Alan Lake Rice cites in Chart 1 of his *Gothic prepositional compounds* (Language dissertation No. 11, 1932), the 1171 Gothic simplicia which render Greek compounds and which Rice also cites, and the thousands of items in my own files, which have been checked against sample counts of simplicia in GB.

¹² Here belong 234 aggregates (§4.1), e.g. *niman* 'take' and its derivatives (GB 2.101-2), and all isolate simplicia and derivatives of high frequency, e.g. *briggan* 'bring', *fijan* 'hate', *frijon* 'love', *leipan* 'go' and its derivatives (GB 2.83-4), *urraisjan* 'raise, awaken', *urraisjan* 'arise'.

4.21. ISOLATE SIMPLICIA. Some of these are used only in the ipf, e.g. *auhjon* 'make ado' or *gretan* 'weep': *hva auhjoh* (Gk. *θορυβεῖσθε* : OCS *mlīwile*) *jah gretiþ* (Gk. *κλαίετε* : OCS *plačete se*) 'Why make ye ado, and weep' (Mk. 5.39). Others occur only as pf, e.g. *maurþrjan* 'kill': *ni maurþrjais* (Gk. *φονεύσεις* : OCS *ubiješi*); *iþ saei maurþreiþ* (Gk. *φονεύση* : OCS *ubijetū*) *skula wairþai stauai* 'Thou shalt not kill; and whosoever shall kill shall be in danger of the judgment' (M 5.21). Some are ipf in one context, pf in another, e.g. *agan* 'be afraid': (ipf) *jah ni qeþun mannhun waiht; ohtedun sik* (Gk. *ἐφοβοῦντο* : OCS *boěxq se*) 'neither said they any thing to any man; for they were afraid' (Mk. 16.8) and (pf) *gasaihwand Iesu gaggandan and marein ... jah ohtedun sis* (Gk. *ἐφοβήθησαν* : OCS *uboěšę se*) 'they see Jesus walking on the sea ...; and they were afraid' (J 6.19).

The last example, like *frijon* 'love', is aspectually indifferent (= ind). So also pf *hazjau* 'I shall praise' : Gk. *ἐπαινέσω* : Pol. *pochwałę*, but ipf *hazja* 'I praise' : Gk. *ἐπαινῶ* : Pol. *chwale* (both in K 11.22); pf *sildaleikida* 'he marveled' : Gk. *ἐθαύμασεν* : OCS *divi se* : Pol. *zadziwił się* (M 8.10), but ipf *sildaleikidedun* 'they were amazed' : Gk. *ἐθαμβοῦντο* : OCS *užasaaxq* : Pol. *zdumiewali się* (Mk. 10.32); pf *saurgaiþ* 'you should be grieved' : Gk. *λυπηθῆτε* : Pol. *żębyście byli zasmuceni* (k 2.4), but ipf *saurgaiþ* 'take ye thought' : Gk. *μεριμνᾶτε* : OCS *pečete se* : Pol. *troszczycie się* (M 6.28).

One may also classify as ind verbs which in our material appear only as ipf, or only as pf, but which are related in meaning to others that occur with either aspectual phase. Thus *fijan* 'hate', which occurs only as ipf, is grouped with ind *frijon* 'love'; ipf *audagjan* 'call blessed' or *awiljudon* 'thank' with ind *hazjan* 'praise'; pf *gaurjan* 'make sorry' : Gk. *λυπεῖν* : Pol. *zasmucić* (k 2.2; 7.8), or *gailjan* 'make glad' : Gk. *ἐφραλνεῖν* : Pol. *rozweselić* (k 2.2), with ind *saurgan* 'grieve'.

4.22. AGGREGATE SIMPLICIA.¹³ Some of these appear only as ipf, e.g. *brinnan* 'burn': *jains was lukarn brinnando* (Gk. *ὁ καίμενος* : OCS *gorę*) *jah liuhtjando* (Gk. *ὁ φαῖνων* : OCS *svitę*) 'he was a burning and shining light' (J 5.35). Others appear only as pf, e.g. *nasjan* 'save': *saei allis wili saiwala seinā nasjan* (Gk. *σῶσαι* : OCS *sūpasti*) 'For whosoever will save his life' (L 9.24). Others are Pf, e.g. *fahan* 'catch; take': *sumai þan ize wildedun fahan* (Gk. *πιάσαι* : OCS *jeti*) *ina* 'some of them would have taken him' (J 7.44); *qiman* 'come': *jah gemun* (Gk. *ἦλθον* : OCS *prido*) *ahvos* 'and the floods came' (M 7.25); or *giban* 'give': *þanuh Peilatus uslaubida giban* (Gk. *ἀποδοθῆναι* : OCS *vūdati*) *þata leik* 'Then Pilate commanded the body to be delivered' (M 27.58). Some, finally, are ind, e.g. *hausjan* 'hear; listen': (ipf) *jah þata waurd þata hauseiþ* (Gk. *ἀκούετε* : OCS *slyšite*) *nist mein* 'and the word which ye hear is not mine' (J 14.24), and (pf) *jah stibnos meinazos hausjand* (Gk. *ἀκούσουσιν* : OCS *uslyšetū*) 'and they shall hear my voice' (J 10.16); or *gaggan* 'walk; come' (§2.11).

Here again, some verbs may be classified as ind even though they appear only as ipf, or only as pf. Thus ipf *brinnan* 'burn' is grouped with ind *liuhtjan* 'give light, shine': ipf *liuhteip* 'giveth light' : Gk. *λάμπει* : OCS *svētītū* (M 5.15) and pf *liuhtida* 'hath shined' : Gk. *ἐλαμψεν* : Pol. *rozświecił* (k 4.6), or

¹³ The derivative forms which complement the above simplicia are all to be found in GB 2.

with *skeinan* 'shine, shine out': ipf *skeinip* 'shineth': Gk. λάμπει : OCS *svīliti* *se* (L 17.24) and pf *skeinan* 'shine out': Gk. λάμψαι : Pol. *rozświeciła* (k 4.6). Similarly, pf *nasjan* 'save' is grouped with ind *hailjan* 'heal': ipf *hailjan* 'heal': Gk. *ἰάσθαι* : OCS *čēliti* (L 5.17) and pf *hailjan* 'heal': Gk. *ἰαθῆναι* : OCS *iscēliti* (L 6.17), or with ind *leikinon* 'heal': ipf *leikinon* 'be healed': Gk. *θεραπεύεσθαι* : OCS *čēliti se* (L 5.15) and pf *leikinodedi* 'would heal': Gk. *θεραπεύσει* : OCS *iscēliti-i* (L 6.7).

4.3. Summing up, the present study, unlike the prevailing view (which distinguishes between an ipf and a pf simplex), distinguishes between a Pf and an ind simplex. The latter, by virtue of its meaning, may appear as ipf in one context, as pf in another, or—because of the incompleteness of our corpus—only as ipf or only as pf. In other words, the separate ipf and pf occurrences do not represent independent aspectual types, but merely two phases of the same type, the ind simplex.

Indeed, if the simplicia were all we had to go by, their widespread aspectual indifference would lead us to conclude that Gothic had no other formal means of distinguishing aspects. But Gothic has many derivative verbs also, which may confirm or modify our findings.

5.0. DERIVATIVES. Of the total of 5947 compounds that Rice (fn. 11) cites in his study (125), 2516, or more than two-fifths, are derivatives with the prefix *ga-*; the remaining 3431 are distributed among the remaining 27 prefixes (Rice 125). This startling disproportion in a sense explains why *ga-* has received so much more attention than other prefixes, and why it has been assigned such an important role in the aspectual scheme of Gothic. The derivatives with *ga-* are further noteworthy for the fact that nearly seven-tenths of them (1712) correspond to Greek simplicia, as against less than four-tenths of the other derivatives (1359). In other words, the probability that a given derivative corresponds to a Greek simplex is nearly twice as great when its prefix is *ga-*. No wonder that *ga-* has been referred to as the 'farbloße Partikel' (PIAG 166).

But the disparity is even more significant in the light of the claim that *ga-* is the perfectivizing particle par excellence (PIAG 103). If this claim were true, we should have to assume that a great many Greek simplicia were inherently perfective.

In what follows I shall treat first the derivatives with *ga-*, then those with other prefixes (symbolized by P-), and lastly those whose base occurs both with *ga-* and with P-. Whatever the grouping of derivatives, or the subgrouping into isolates and aggregates, the aspectual distribution is here the same as among the simplicia. Some forms are ipf, some are pf, some are now ipf and now pf; others again, which appear only as ipf or only as pf, are aspectually ind; and some, finally, are Pf.

5.1. Derivatives with *ga-* include isolates and aggregates.

5.11. Isolates: (a) ipf *ga-pairban* 'be temperate': *ip hrazuh saei haifstjan sniwiþ*, *allis sik gaparbai* (Gk. *ἐγκρατεῖναι* : Pol. *się powściąga*) 'And every man that striveth for the mastery is temperate in all things' (K 9.25); *ga-beistjan* 'leaven': *niu wituþ patei leitil beistis allana daig gabeisteiþ* (Gk. *ζυμοῖν* : Pol. *zakwasza*) 'Know ye not that a little leaven leaveneth the whole lump' (K 5.6). (b) pf *ga-*

nisan 'become whole': *jabai patainei atleka waistjai is, ganisa* (Gk. σωθήσομαι : OCS *sūpasna bōdō*) 'if I but touch his garment, I shall be whole' (M 9.21); Pf *ga-geigan* 'gain' and *ga-sleiþjan* 'forfeit': *jabai gageigaiþ* (Gk. κερδήσῃ : OCS *priobreščetŭ*) *þana fairhu allana jah gasleiþeiþ* (Gk. ζημιωθῇ : OCS *otŭščetitŭ*) *sik saiwalai seinai* 'if he shall gain the whole world, and lose his own soul' (Mk. 8.36). (c) *ga-daupnan* 'die': (ipf) *þarei maþa ize ni gadaupniþ* (Gk. τελευτᾷ : OCS *umiraatŭ*) 'where the worm dieth not' (Mk. 9.48), (pf) *þatei gadaupniþ* (Gk. ἀποθανείσθε : OCS *umīrete*) *in frawaurhtaim izwaraim* 'that ye shall die in your sins' (J 8.24); *ga-teihan* 'preach; show': (ipf) *þanei weis gateiham* (Gk. καταγγέλλομεν : Pol. *opowiadamy*) *talzjandans all manne* 'When we preach, warning every man' (C 1.28), (pf) *jah þata anawairþo gateihþ* (Gk. ἀναγγελεῖ : OCS *vŭzvēstītŭ*) *izwis* 'and he will shew you things to come' (J 16.13).

5.12. Aggregates: (a) ipf *ga-karon* 'take care of':¹⁴ *ip jabai has seinamma gagga fauragaggan ni mag, hwiwa aikklesion gakarop* (Gk. ἐπιμελήσεται : Pol. *pieczę będzie miał*) 'For if a man know not how to rule his own land, how shall he take care of the church of God' (T 3.5); *ga-sokjan* 'seek': *bigitans warþ þaim mik ni gasokjandam* (Gk. ζητοῦσιν : Pol. *którzy mnie nie szukali*) 'I was found of them that sought me not' (R 10.20). (b) pf *ga-filhan* 'bury': *uslaubei mis ... gafilhan* (Gk. θάψαι : OCS *pogreti*) *attan meinana* 'suffer me ... to bury my father' (M 8.21); Pf *ga-fahan* 'take': *sokidedun þan ina gafahan* (Gk. πιάσαι : OCS *jēti*) 'Then they sought to take him' (J 7.30). (c) ind *ga-sitan* 'sit': (ipf) *jah jainar gasat* (Gk. ἐκάθητο : OCS *sēdēaše*) *miþ siponjam seinaim* 'and there he sat with his disciples' (J 6.3), (pf) *jah usgibands andbahta gasat* (Gk. ἐκάθισεν : OCS *sēde* : Pol. *usiadł*) 'and he gave again to the minister, and he sat down' (L 4.20); *ga-hausjan* 'hear': (ipf) *gahausidedun* (Gk. ἤκουον : OCS *slyšajaxō*) *þan þo alla þai Fairisaieis* 'and the Pharisees heard all these things' (L 16.14), (pf) *gahausida* (Gk. ἤκουσεν : OCS *uslyšavŭ*) *þiudans Herodes* 'And the king Herod heard of him' (Mk. 6.14).

5.13. Forms classed as ind: (a) ipf *gaswikunþeiþ* 'commendeth' : Gk. συνίστησιν : Pol. *zaleca* (k 10.18), with ind *gateihan* 'preach; shew'; *gatimid* 'agreeth' : Gk. συμφωνεῖ : Pol. *zgadza się* (L 5.36), or *gadob* 'becometh' : Gk. τρέπει : Pol. *przystoi* (E 5.3) with ind *ga-motan* 'find place': ipf *gamot* 'hath place' : Gk. χωρεῖ : OCS *vīmēštaatŭ sę* (J 8.37), pf *gamoteima* 'receive' : Gk. χωρήσατε : Pol. *przyjmujcie* (k 7.2). (b) *gaqiunand* 'shall be made alive' : Gk. ζωοποιηθήσονται : Pol. *ożywnieni będą* (K 15.22) with *gadaupnan* 'die'.

5.2. Derivatives with P- also include isolates and aggregates.

5.21. Isolates: (a) ipf *us-agljan* 'weary': *ibai und andi qimandei usagljai* (Gk. ὑποτιάσῃ : OCS *zastožitŭ*) *mis* 'but by her continual coming she weary me' (L 18.5); *us-baugjan* 'sweep': *usbaugēiþ* (Gk. σαρπῶ : OCS *pometetŭ* : Pol. *umiata*) *razn* 'sweep(s) the house' (L 15.8). (b) pf *faur-dammjan* 'stop': *unte so hvoftuli ni faurdammjada* (Gk. φραγῆσεται : Pol. *będzie zathumiona*) *in mis* 'no man shall stop me of this boasting' (k 11.10); Pf *fra-liusan* 'lose' and *bi-gitan* 'find': *jabai fraliusiþ* (Gk. ἀπολέσῃ : OCS *pogubitŭ*) *drakmin ainamma, niu ... sokeiþ ...*, *unte bigitiþ* (Gk. εὑρη : OCS *obrēšetŭ*) 'if she lose one piece, doth not seek ... till

¹⁴ The simplicia which complement the above aggregate derivatives, as well as those cited in §5.22 and in §5.32, are all to be found in GB 2.

she find it' (L 15.8). (c) ind *fra-gistnan* 'perish': (ipf) *ik hundrau fragistna* (Gk. ἀπόλλυμαι : OCS *gybljō*) 'I perish with hunger' (L 15.17), (pf) *þai balgeis fragistnand* (Gk. ἀπαλόνται : OCS *pogybnōtū*) 'the bottles shall perish' (L 5.37); *af-hvarþjan* 'choke': (ipf) *afhvarþjand* (Gk. συμπνίγονσιν : OCS *podavlējōtū*) *þata waurd* 'choke the word' (Mk. 4.19), (pf) *þai þaurnjus afhvarþidedun* (Gk. ἀπέννιξαν : OCS *podavi*) *þata* 'the thorns choked it' (L 8.7).

5.22. Aggregates: (a) ipf *bi-hlahjan* 'laugh to scorn': *jah bihlohun* (Gk. κατεγέλων : OCS *rogaazo se*) *ina gasaiw andans þatei gaswalt* 'And they laughed him to scorn, knowing that she was dead' (L 8.53); *bi-kukjan* 'kiss': *ni swaiþ bikukjan* (Gk. καταφιλοῦσα : OCS *oblobyzajošti*) *foþuns meinans* 'hath not ceased to kiss my feet' (L 7.45); *ufar-wahsjan* 'grow exceedingly': *unte ufarwahseip* (Gk. ὑπεραυξάνει : Pol. *bardzo rośnie*) *galaubeins izwara* 'because your faith groweth exceedingly' (th 1.3). (b) pf *bi-skeinan* 'shine round about': *jah wulþus frauþins biskain* (Gk. περιέλαμψεν : OCS *osiš* : Pol. *oświecila*) *ins* 'And the glory of the Lord shone round about them' (L 2.9); *us-saihan* 'look up': *jah gatawida ina usaihan* (Gk. ἀναβλέπειν : OCS *prožrēti*) 'and made him look up' (Mk. 8.25); Pf *uf-ligan* 'fail; faint': *ei þan ufligaiþ* (Gk. ἐκλείπητε : OCS *oskōdēate* : Pol. *ustaniecie*) 'when ye fail' (L 16.9); *us-giban* 'render': *us-nu-gibiþ* (Gk. ἀπόδοτε : OCS *vūzdaþite*) *þo kaisaris kaisara* 'Render therefore unto Caesar the thing which be Caesar's' (L 20.25); *fra-giban* 'grant': *fragiþ* (Gk. δός : OCS *daždū*) *ugkis* 'Grant unto us' (Mk. 10.37). (c) ind *us-bairan* 'bring forth; bear': (ipf) *ubils manna ... usbairid* (Gk. προσφέρειν : OCS *iznositū*) *ubil* 'an evil man ... bringeth forth that which is evil' (L 6.45), (pf) *jah sauhtins usbar* (Gk. ἐβάστασεν : OCS *ponese*) 'and bare our sicknesses' (M 8.17); *bi-laikan* 'mock': (ipf) *duginnaina bilaikan* (Gk. ἐμπαίζειν : OCS *rogati se*) *ina* 'begin to mock him' (L 14.29), (pf) *jah biþe bilailaikun* (Gk. ἐνέπαξαν : OCS *poroggaše se*) *ina* 'And when they had mocked him' (Mk. 15.20).

5.23. Forms classed as ind: (a) ipf *bihlahjan* 'laugh to scorn' with ind *bilaiþan* 'mock'. (b) pf *us-beidan* 'endure, bear': *usbeidands* (*was*): Gk. ἡνεγκεν : Pol. *znosił* (R 9.22) with ind *usbairan* 'bring forth; bear'.

5.3. Derivatives with *ga-* and *P-* confirm the distributional picture with which we are by now familiar. It also suggests that the inherited text is a genuine sample of 4th-century Gothic, since we find seven of the nine possible varieties that result from combining the three aspectual phases of *ga-* derivatives with the three of *P-* derivatives.¹⁵

5.31. Isolates occur in four varieties.

(1) Either derivative is ipf: *ga-redan* 'care for, strive for, provide for' vs. *ur-redan* 'be subject to ordinances': *garedandans* (Gk. προνοούμενοι : Pol. *pilnie się starajęc*) *auk goda ni þatainei in andwairþ ja gudis* 'providing for honest things, not only in the sight of the Lord' (k 8.21) vs. *ha þanaseips swe qiwai in þamma fairhau urrediþ* (Gk. δογματίζεσθε : Pol. *się ustawami bawicie*) 'as though living in the world, ye are subject to ordinances' (C 2.20).

¹⁵ (1) either der(ivative) if ipf; (2) *ga-* der is ipf, *P-* der is pf; (3) *ga-* der is ipf, *P-* der is ind; (4) either der is pf; (5) *ga-* der is pf, *P-* der is ipf; (6) *ga-* der is pf, *P-* der is ind; (7) either der is ind; (8) *ga-* der is ind, *P-* der is ipf; (9) *ga-* der is ind, *P-* der is pf. The second and third of these combinations do not actually occur.

(2) Either derivative is perfective: *ga-taurnan* 'put asunder' vs. *dis-taurnan* 'break' vs. *af-taurnan* 'make a rent': *jap̄be kun̄bi gataurniḡ* (Gk. καταργηθήσεται : Pol. *wniwecz się obróci*) 'whether there be knowledge, it shall vanish away' (K 13.8) vs. *aip̄bau distaurnanā* (Gk. πηγυνῶται : OCS *prosedotŭ sę*) *balgeins* 'else the bottles break' (M 9.17) vs. *aip̄bau jah sa niuja aftaurnid* (Gk. σκίσει : OCS *razderetŭ*) 'then both the new maketh a rent' (L 5.36).

(3) *Ga-* derivative is perfective, P- is ipf: *ga-tilon* 'obtain' vs. *and-tilon* 'hold to one': *ei jah ḡai ganist gatilona* (Gk. τυχῶσιw : Pol. *dostpili*) 'that they may also obtain salvation' (t 2.10) vs. *aip̄bau ainamma andtiloḡ* (Gk. ἀνθέξεται : OCS *drŭzitiŭ sę*) 'or else he will hold to one' (L 16.13).

(4) Either derivative is ind: *ga-laubjan* 'believe' vs. *us-laubjan* 'permit; suffer; command': (ipf) *ande nu jainis melam ni galaubeiḡ* (Gk. πιστεῦετε : OCS *věry ne emlete*) 'But if ye believe not his writing', (pf) *hwiwa meinaim waurdam galaubjaiḡ* (Gk. πιστεύσητε : OCS *věro imete*) 'how shall ye believe my words' (both citations from J 5.47) or *jah jabai meinaim hausjai waurdam jah galaubjai* (Gk. πιστεύω : OCS *sŭzranitiŭ*) 'And if any man hear my words and believe not' (J 12.47) vs. (ipf) *galaisjan qinon ni uslaubja* (Gk. ἐπιτρέπω : *pozwalam*) 'But I suffer not a woman to teach' (T 2.12), (pf) *ḡanuh Peilatus uslaubida* (Gk. ἐκέλευσεν : OCS *povelę*) *pata leik* 'Then Pilate commanded the body to be delivered' (M 27.58).

5.32. Aggregates occur in six varieties.

(1) Either derivative is ipf: *ga-spillon* 'preach' vs. *us-spillon* 'relate': *iḡ ḡu gagg jah gaspillo* (Gk. διάγγελλε : OCS *vŭzvęštai*) *ḡiudangardja gudis* 'but go thou and preach the kingdom of God' (L 9.60) vs. *jah gawandjandans sik appostauleis uspillodedun* (Gk. διηγήσαντο : OCS *povędaše*) *imma, swa filu swe gatawidedun* 'And the apostles, when they were returned, told him all they had done' (L 9.10).

(2) Either derivative is perfective: *ga-steigan* 'descend' vs. *us-steigan* 'ascend': *ḡas gasteigiḡ* (Gk. καταβήσεται : Pol. *zstopi*) *in afgrandiḡa* 'Who shall descend into the deep' (R 10.7) vs. *ḡas usteigiḡ* (Gk. ἀναβήσεται : Pol. *wstopi*) *in himin* 'Who shall ascend the heaven' (R 10.6).

(3) *Ga-* derivative is perfective, P- is ind: *ga-bairan* 'give birth to' vs. *us-bairan* 'bring': *jah gabar* (Gk. ἔτεκεν : OCS *rodi*) *sunu seinana ḡana frumabaur* 'And she brought forth her first-born son' (L 2.7) vs. (ipf) *ḡiuḡeigs manna ... usbairid* (Gk. προσφέρει : OCS *iznositiŭ*) *ḡiuḡ* 'A good man ... bringeth forth that which is good' (L 6.45), (pf) *jah sauhlins usbar* (Gk. ἐβάστασεν : OCS *ponese*) 'and bare our sicknesses' (M 8.17).

(4) *Ga-* derivative is ind, P- is perfective: *ga-wrikan* 'avenge' vs. *fra-wrikan* 'persecute': (ipf) *ni izwis silbana gawrikandans* (Gk. ἐδικοῦντες : Pol. *mścijcie*) 'avenge not yourselves' (= be not vengeful) (R 12.19), (pf) *iḡ ḡuḡ niu gawrikai* (Gk. ποιήσῃ τὴν ἐχθικησιν : OCS *imatŭ sŭtvoriti mŭsti*) *ḡans gawalidans seinans* 'And shall not God avenge his own elect' (L 18.7) or *jah uns frawrekun* (Gk. ἐκδιώξαντων : Pol. *wygnali*) 'and have persecuted us' (Th 2.15).

(5) *Ga-* derivative is ind, P- is ipf: *ga-siggan* 'set' vs. *dis-siggan* 'go down': (ipf) *andanahtja ḡan waurḡanamma, ḡan gasaggg* (Gk. ἔδω : OCS *sŭxoḡdaše*) *sauil* 'And at even, when the sun set' (Mk. 1.32), (pf) *ibai aufto managizein*

saurgai gasigggai (Gk. καταποθῆ : Pol. pożart) *sa swileiks* 'lest perhaps such a one should be swallowed up with overmuch sorrow' (k 2.7) vs. *sunno ni disigggai* (Gk. ἐπιδύετω : Pol. zachodzi) *ana þwairhein izwara* 'let no sun go down upon your wrath' (E 4.26 AB).

(6) Either derivative is ind: *ga-þulan* 'suffer' vs. *us-þulan* 'endure': (ipf) *jabai gaþulam* (Gk. ὑπομένομεν : Pol. cierpimy), *jah miþþiudanom* 'if we suffer, we shall also reign with him' (t 2.12), (pf) *jah manag gaþulandei* (Gk. παθοῦσα : OCS postradaviši) *fram managjam lekaim* 'And he had suffered many things of many physicians' (M 5.26) vs. (ipf) *aglons usþulandans* (Gk. ὑπομένοντες : Pol. cierpliwi) 'patient in tribulation' (R 12.12), (pf) *wairþiþ mel þan haila laisein ni usþuland* (Gk. ἀνέξονται : Pol. scierpić) 'For the time will come when they will not endure sound doctrine' (t 4.3).

6.0. ASPECTUAL RELATION BETWEEN SIMPLEX AND DERIVATIVE. Our analysis so far has shown that aspect in Gothic was the same for derivatives and simplicia; we are led to infer that a given prefix helped to determine the aspect only as it shared in the meaning of the form as a whole. We shall now examine the relation of simplicia to derivatives with *ga-* and with *P-*, to determine whether either type of prefix implies a specific mode of aspectual differentiation as a given form shifts from simplex to derivative.

6.1. If the derivative is formed with *ga-*,

(1) An ipf simplex may yield (a) an ipf: *fraujinob* 'hath dominion': Gk. κυριεύει : Pol. panuje (R 7.1) vs. *gafräujinond* 'exercise leadership': κατακυριεύουσιν : OCS obladaǵiū : Pol. panuję (Mk. 10.42); (b) a pf: *natjan* 'wash': Gk. βρέχειν : OCS močiti (L 7.38) vs. *ganatida* 'washed': Gk. ἔβρεξεν : OCS omoči (L 7.44); (c) a Pf: *sagg* 'was setting': Gk. δύνωτος : OCS zaxodeštju (L 4.40) vs. *gasigggai* 'swallowed up': Gk. καταποθῆ : Pol. pożart (k 2.7); (d) an ind: *lisand* 'gather': Gk. συνάγουσιν : OCS sūbirajotū (M 6.26) vs. ipf *galesun* 'gathered': Gk. συνάγουσιν : OCS sūbirajotū (J 15.6), pf *galesun* 'gathered': Gk. συνήχθη : OCS subra se (Mk. 4.1).

(2) a pf simplex may yield (a) a pf: *lauseiþ* 'shall deliver': Gk. ῥύσεται : Pol. wybarwi (R 7.24) vs. *galauseiþ* 'will deliver': Gk. ῥύσεται : Pol. wyrwie (k 1.10); (b) an ind: *nasjan* 'save': Gk. σώσαι : OCS sūpasti (L 9.24) vs. ipf *ganasida* 'healed': Gk. ἰάτο : OCS cělēaše (L 6.19), pf *ganasjan* 'save': Gk. σώσαι : OCS sūpasti (L 6.9).

(3) an ind simplex may yield (a) an ipf: ipf *mikilidedun* 'glorified': Gk. ἐδόξαζον : Pol. chwalili (G 1.24), pf *mikilidedun* 'glorified': Gk. ἐδόξασαν : OCS proslaviše (M 9.8) vs. *gamikilida* 'rejoiced': Gk. ἐμεγάλυνεν : OCS radovazę se (L 1.58); (b) a pf: *liuhteip* 'giveth light' (M 5.15), pf *liuhtida* 'hath shined' (k 4.6) (cf. §4.22) vs. *galiuhteip* 'will bring to light': Gk. φωτίζει : Pol. oświeci (K 4.5); (c) a Pf: ipf *gaggiþ* 'goeth': Gk. πορεύεται : OCS xoditū (J 10.4), pf *gaggiþ* 'cometh': Gk. ἐρχεται : OCS pridetū (J 6.45) (cf. §2.11) vs. *gagaggiþ* 'shall come to pass': Gk. γίγεται : OCS byvaatū (Mk. 11.23) and *gaggiþ* 'shall turn to': Gk. ἀποθήσεται : Pol. wynijdzie na' (Ph 1.19); (d) an ind: ipf *hauseiþ* 'ye hear': Gk. ἀκούετε : OCS slyšite (J 14.24), pf *hausida* 'heard': Gk. ἤκουσεν : OCS uslūša (L 1.41) vs. ipf *gahausidedun* 'heard': Gk. ἤκουον : OCS slyšajazę (L 16.14), pf *gahausida* 'heard': ἤκουσεν : OCS uslūšavū (Mk. 6.14).

(4) a Pf simplex may yield (a) a Pf which is in free variation with it: *fahan* 'catch' : Gk. πιάσαι : OCS *jěti* (J 7.44) vs. *gafahan* 'seize' : Gk. πιάσαι : OCS *jěti* (J 7.30); (b) a Pf which is not in free variation with it: *nimiþ* 'taketh' : Gk. λαμβάνει : OCS *priimetŭ* (M 10.38) or *nimiþ* 'receiveth' : Gk. δέχεται : OCS *priemletŭ* (M 10.41) vs. *ganimis* 'thou shalt conceive' : Gk. συλλάβη : OCS *začineši* (L 1.31) or *ganimiþ* 'learn' : Gk. μάθετε : OCS *naučite se* (M 9.13); or *qimiþ* 'cometh' : Gk. ἐρχεται : OCS *prixoditŭ* (J 10.10) vs. *gaqimau* 'I might attain' : Gk. καταντήσω : Pol. *bym doszedł* (Ph. 3.11); (c) an ipf: *qimiþ* 'cometh' (above) vs. *gaqimiþ* 'it is fit' : Gk. ἀνῆκεν : Pol. *przystoi* (C 3.16); (d) an ind: *qimiþ* 'cometh' (above) vs. ipf *gaqimand* 'resort to' : Gk. συνέρχονται : OCS *sünemlŭtŭ se* (J 18.20), pf *gaqemun sik* 'resorted to' : Gk. συμπορεύονται : OCS *prido* (Mk. 10.1).

6.2. If the derivative is formed with P-,

(1) an ipf simplex may yield (a) an ipf: *hlahjandans* 'that laugh' : Gk. γελῶντες : OCS *smějŭšeti se* (L 6.25) vs. *bihlohun* 'laughed to scorn' : Gk. κατεγέλων : OCS *rogaazŭ se* (L 8.53); (b) a pf: *steigip* 'climbeth' : Gk. ἀναβαίνων : OCS *prělaze* (J 10.1) vs. *ussteigip* 'shall ascend' : Gk. ἀναβήσεται : Pol. *wstąpi* (R 10.6); (c) a Pf: *ligip* 'lieth' : Gk. βέβληται : OCS *ležitŭ* (M 8.6) vs. *ufligaiþ* 'ye fail' (L 16.9); (d) an ind: *þulau* 'shall I suffer' : Gk. ἀνέξομαι : OCS *trŭpljo* (Mk. 9.19) vs. ipf *uþulandans* 'patient' (R 12.12), pf *uþuland* 'will endure' (t 4.3) (cf. §5.32).

(2) a pf simplex may yield (a) a pf: *stigggan* 'make war' : Gk. συμβαλεῖν : OCS *sŭniti se* (L 14.31) vs. *bistuggun* 'beat upon' : Gk. προσέκοψαν : OCS *opŭrěse se* (M 7.27); (b) an ind: *filhan* 'bury' : Gk. θάψαι : OCS *pogreti* (M 8.22) vs. ipf *anafilha* 'I commit' : Gk. παρατίθεναι : Pol. *zalecam* (T 1.18), pf *anafilh* 'commit' : Gk. παράθω : Pol. *powierz* (t 2.2).

(3) an ind simplex may yield (a) an ipf: ipf *saei haifstjan sniwiþ* 'that striveth for mastery' : Gk. ὁ ἀγωνιζόμενος : Pol. *który się potyka* (K 9.25), pf *snau* 'is come' : Gk. ἔφθασεν : Pol. *przyszedł* (Th 2.16) vs. *faurbisniwandein* 'going before' : Gk. προάγουσαι : Pol. *uprzedzają* (T 5.24); (b) a pf: ind *snian* (above) vs. *faursnau* 'is come before' : Gk. προέλαβεν : OCS *varila estŭ* : Pol. *poprzedzila* (Mk. 14.8); (c) a Pf: ipf *sat* 'sat' : Gk. ἐκάθητο : OCS *sěděaše* (M 26.69), pf *sat* 'sat' : Gk. κακάθεικεν : OCS *všělŭ* (Mk. 11.2) vs. *dissat* 'came' : Gk. ἔλαβεν : OCS *prijetŭ* (L 7.16); (d) an ind: ipf *laikid* 'leap for joy' : Gk. σικιρήσατε : OCS *vŭzigrate* : Pol. *weselcie się* (L 6.23), pf *lailaik* 'leaped' : Gk. ἐσκιρήσεν : OCS *vŭziga se* (L 1.41) vs. ipf *bilaiikan* 'mock' : Gk. ἐμπαίζειν : OCS *rogati se* (L 14.29), pf *bilailaikun* 'mocked' : Gk. ἐνέπαιξαν : OCS *porogase* (Mk. 15.20).

(4) a Pf simplex may yield (a) a Pf which is in free variation with it: *niman* 'take away' : Gk. λαβεῖν : OCS *vŭzjeti* (M 5.40) vs. *andnimai* 'shall receive' : Gk. λάβη : OCS *imatŭ prijeti* (M 10.30); (b) a Pf which is not in free variation with it: *niman* 'take away' (above) vs. *binimaina* 'steal' : Gk. κλέψωσιν : OCS *ukradetŭ* (M 27.64); (c) an ipf: *niman* 'take away' (above) vs. *disnimandans* 'possessing' : Gk. κατέχοντες : Pol. *trzymający* (k 6.10); (d) an ind: *qimiþ* 'cometh' (cf. §6.1) vs. ipf *usqimiþ* 'killeth' : Gk. ἀποκτείνει : Pol. *zabija* (k 3.6), pf *usqimai* 'will he kill' : Gk. ἀποκτενεῖ : OCS *ubietŭ* (J 8.22).

6.3. The preceding analysis clearly shows that the relation of simplicia was

the same to both derivatives with *ga-* and those with *P-*: the aspectual distribution is the same in either case. It establishes beyond all doubt the complete lack of dependence of Gothic aspect on form as such. This conclusion is corroborated by the many instances of free variation, most of them between simplicia and derivatives with *ga-*.¹⁶ Free variation between an underlying simplex and its derivative is conceivable only in a system in which formal shift is not necessarily accompanied by a shift in meaning or aspect. Its occurrence argues decisively against the assumption of a formal aspectual system, and for the kind of aspectual patterning that we have found to obtain on each derivational level: patterning that is independent of form as such and allows for a variety of uses in consonance with meaning as determined by the context.

¹⁶ Here is a sampling of pairs of free variants, alike in meaning, aspect, and grammatical form. While such completely congruent variants are less frequent than those which are alike only in meaning and aspect, they prove that free variation between simplex and derivative was not uncommon in Gothic.

1. Free variation between simplex and derivative with *ga-*, both variants being (a) in the ipf: *saihis* 'seest' (Mk. 5.31) : *gasaihis* 'seest' (L 7.44) : Gk. βλέπεις : OCS *videši*; *sat* 'sat' (M 26.69) : *gasat* 'sat' (J 6.3) : Gk. ἐκάθητο : OCS *sěděaše*; (b) in the pf: *filhan* 'bury' (M 8.22) : *gafilhan* 'bury' (M 8.21) : Gk. θάψαι : OCS *pogreti* (GB 13 has here the emendation *ga*) *filhan*; *taujan* 'do' (M 9.28) : *gataujan* 'make' (M 5.36) : Gk. ποιῆσαι : OCS *sūtvoriti*; *hausida* 'heard' : OCS *uslyša* (L 1.41) : *gahausida* 'heard' : OCS *uslyšavū* (Mk. 6.14) : Gk. ἤκουσεν; *snauh* 'is come upon' : Pol. *przyszędł* (Th 2.16) : *gasnau* 'hath attained' : Pol. *doszedł* (R 9.31) : Gk. ἐφθασεν; *aistand* 'will reverence' : OCS *usramlējōtū* (L 20.13) : *gaaistand* 'will reverence' : OCS *posramlējōtū* (Mk. 12.6) : Gk. ἐτραπήσονται; *lauseiþ* 'will deliver' : Pol. *wybawi* (R 7.24) : *galauseiþ* 'will deliver' : Pol. *wyrwie* (k 1.10) : Gk. ῥύσεται; *þahaidedi* 'should hold his peace' (L 18.39) : *gaþahaidedi* 'should hold his peace' : Gk. σωπήσῃ : OCS *umlūčitū* (Mk. 10.48); (c) in Pf: *fahan* 'have taken' (J 7.44) : *gafahan* 'take' (J 7.30) : Gk. πιάσαι : OCS *jēti*.

2. Free variation between simplex and derivative with *P-*, both variants being (a) in the ipf: *wisiþ* 'abideth' (J 8.35) : *þairhwisiþ* 'remaineth' (J 9.41) : Gk. μένει : OCS *prěbyvatū*; (b) in the pf: *þwahan* 'wash' (J 9.7) : *afþwahan* 'wash' (J 9.11) : Gk. νίψαι : OCS *umy se*; *bairiþ* 'bring' (Mk. 9.19) : *albairiþ* 'bring' (Mk. 12.15) : Gk. φέρετε : OCS *prine-sele*; (c) in the Pf: *gif* 'give' (M 6.11) : *fragif* 'grant' (Mk. 10.37) : Gk. δός : OCS *daždū*.

THE PERSONAL-PRONOUN SYSTEM OF CLASSICAL ARABIC

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The present article is intended to display a procedure of morphemic analysis that makes maximum use of morph-segmentation, and classifies morphs into morphemes in terms of recurrent partials within a fixed frame of reference. The frame of reference consists of a text in which substitutions are made at various points and the resulting other changes are observed. Thus, in English we find *one boy, one girl, one man*, but *two boys, two girls, two men*. From this one separates out the partials *one, boy, girl, man, two, -s*, and replacement of *a* by *e* (this example is deliberately put in orthographic terms); *-s* and the replacement of *a* by *e* are then grouped together in a class of morphs, a morpheme. We can now give this morpheme a label, if we wish, such as PLURAL, but we could just as well call it something else; the referential meaning (meta-linguistic or 'real' meaning) does not concern us in the analysis, but there is no need to ignore it in choosing our labels.

The Arabic material here analyzed was assembled and checked by Rice in the course of analyzing the Classical Arabic morphemic structure. Trager had long noticed the indications of patterning in the Arabic pronouns, and took the opportunity to suggest and then to elaborate the analysis which is now set forth.

One additional theoretical point should be made: we call our partials MORPHS and class them into MORPHEMES. If these entities do not correspond to previous 'definitions' of the terms, we are not at all concerned. We are trying to produce an adequate and useful analysis; the labels we use are conveniently chosen where we find them, and are defined by the use we make of them.

1. The paradigms to be analyzed are the following. (In our transcription, ' is the glottal stop, · indicates vowel length, and *h* is the pharyngeal voiceless spirant.)

INDEPENDENT PERSONAL PRONOUNS: (Column I)

'first person singular'	'ana·
'first person plural'	naḥnu
'2d sing. [masculine]'	'anta
'2d sing. feminine'	'anti
'2d pl. [masculine]'	'antum
'2d pl. feminine'	'antunna
'2d [pl.] dual'	'antuma·
'3d sing. [masculine]'	huwa
'3d sing. feminine'	hiya
'3d pl. [masculine]'	hum
'3d pl. feminine'	hunna
'3d dual'	huma·

The categories named will be symbolized hereafter by the symbols in the first column below (3FD is '3d feminine dual'). Column I indicates the forms in the table above; the other columns show the following: II, suffixes indicating possessor, or object of a verb; III, suffixes of the subject, in the 'suffix tense'; IV, suffixes and prefixes of the subject, in the 'prefix tenses'.

I	II	III	IV
0	-i· ~ -ya, -ni·	-tu	'-
1	-na·	-na·	n-
2	-ka	-ta	t-
2F	-ki	-ti	t-...-i·(na)
2P	-kum	-tum	t-...-u·(na)
2PF	-kunna	-tunna	t-...-na
2PD	-kuma·	-tuma·	t-...-a·(ni)
3	-hu	-a	y-
3F	-ha·	-at	l-
3P	-hum	-u·	y-...-u·(na)
3PF	-hunna	-na	y-...-na
3PD	-huma·	-a·	y-...-a·(ni)
3FD	—	-ata·	t-...-a·(ni)

The forms of 0 in II occur thus: -i· after consonants except *y*, -*ya* after long vowels, both as possessive; and -*ni·* as object of a verb. The suffix tense is the traditional 'perfect' (*katabtu* 'I wrote'); the basic prefix tense is the 'imperfect' (*'aktubu* 'I'm writing'). The endings -(na) and -(ni) in IV are affixes that disappear under storable syntactic conditions and need not be further considered here.

2. We begin the analysis with the obviously parallel forms for the 2d person in columns II and III. These could be segmented thus:

-k-a	-t-a
-k-i	-t-i
-k-u-m	-t-u-m
-k-u-nna	-t-u-nna
-k-u-m-a·	-t-u-m-a·

Further inspection of the material shows that the final *a* of -*ka* and -*ta* may be lost under storable syntactic conditions; we conclude that it is a part of the morphs -*ka* and -*ta*, and say that it automatically disappears before additional affix-material beginning with a vowel. For this we shall write (*a*); the segmented forms will be rewritten -*k(a)*, -*k(a)-i*, -*k(a)-u-m*, ... -*t(a)*, -*t(a)-i*, etc. We next note that -*um*, -*unna*, -*uma·* appear also in 3d-person forms, and we conclude that there is no need to separate the *u* from what follows it; if, however, -*um* appears by itself and also followed by -*a·*, it seems economical to divide -*unna* into -*um-na*, with automatic replacement of *m* by *n* before *n*. We now summarize:

morphs of 2: $-k(a) \sim -t(a)$

morphs of F: $-i \sim -na$

morph of P: $-um$

morph of D: $-a$

F $-i$ occurs after 2, $-na$ after P; D occurs after P.

The next step is to look at the '2' forms in column IV. The prefix $t-$ is obviously an allomorph of 2; then $-i$ is an F morph, $-u$ is a P morph, and $-na$ and $-a$ are the same as before—F and D respectively. However, since these last two suffixes elsewhere are preceded by P, we postulate before them in this case also an occurrence of P, in a zero allomorph -0 . So far, then, we have these forms ($\sqrt{\text{'morpheme'}}$, $\sqrt{\text{'allomorph'}}$):

$\sqrt{2}$: $\sqrt{-k(a)} \sim \sqrt{-t(a)} \sim \sqrt{-t-}$
 \sqrt{F} : $\sqrt{-i} \sim \sqrt{-i-} \sim \sqrt{-na}$
 \sqrt{P} : $\sqrt{-um} \sim \sqrt{-u-} \sim \sqrt{-0}$
 \sqrt{D} : $\sqrt{-a}$

A summary of the total analysis of these forms is given below (§3).

It is now convenient to examine the '2' forms in column I. These look as if they might be segmented into $'an-$ and the forms of column III. This would be an unusual analysis, since the forms in III appear after verb stems, and the constructions in which we find $'anta$ etc. are nominal rather than verbal. Moreover, if $'an-$ is to be separated out as some kind of stem, we should find it also in the '0' form $'ana$ 'I'. Such a segmentation will get us nowhere, however, since the terminal $-a$ would not be parallel to any other 1st singular form. It seems best therefore to leave both $'ana$ and $'anta$ etc. unsegmented. We then complete the statement of $\sqrt{2}$ by putting as its first allomorph the longer form $\sqrt{-ant(a)}$; this combines with \sqrt{F} , \sqrt{P} , \sqrt{D} in the same way as do $\sqrt{-k(a)}$ and $\sqrt{-t(a)}$.

Having mentioned $'ana$, we go on to examine all the 1st person '0' and '1' forms. These show a completely suppletive set in 0, and an initial n in 1. Nothing is gained by segmentation; we set them up simply as follows:

$\sqrt{0}$: $\sqrt{-ana} \sim (\sqrt{-i-} \sim \sqrt{-ya}; \sqrt{-ni-}) \sim \sqrt{-tu} \sim \sqrt{-}$
 $\sqrt{1}$: $\sqrt{-na\eta nu} \sim \sqrt{-na-}$ (columns II and III) $\sim \sqrt{-n-}$

Turning our attention to the 3d-person forms, we find considerable resemblance between columns I and II, specifically these:

	I	II
P	<i>hum</i>	<i>-hum</i>
PF	<i>hunna</i>	<i>-hunna</i>
D	<i>huma</i>	<i>-huma</i>

The second set appears as $-him$, $-hinna$, $-hima$ after stems ending in $-i$, $-i-$, or $-y$, so that they are not completely parallel with $-kum$, $-tum$, etc. Let us suppose however that this effect is a property of the beginning part of the forms, and segment them into $-h-um$, $-h-um-na$, $-h-um-a$; the forms of column I are similarly segmented. We have isolated \sqrt{F} and \sqrt{D} here as before, and may regard $h-$

and *-h-* as forms of $\sqrt{3}$. For further analysis of *h-* and *-h-* we turn to the free pronoun forms 3 and 3F in column I.

The pronouns 'he' and 'she' are *huwa* and *hiya*. In an analysis of the so-called weak roots with *u* and *y*, made by Rice, the postulation of a morphophoneme $\sqrt{\cdot}$ ('semivowel') takes care of most of the difficulties encountered in the usual analyses of those forms; conditions can be stated under which $\sqrt{\cdot}$ is automatically replaced by /w/, /y/, /'/, or /·/. This morphophoneme $\sqrt{\cdot}$ is called for in many positions in the language, and it seems appropriate to test its usefulness in the 3d person pronoun stem. Accordingly we set up the form for 'he' as $\sqrt{hu}a$. The final *a* can be treated like the finals of 'anta, -ka, -ta, that is as (a). The internal $\sqrt{\cdot}$ is /w/ after *u* before a vowel, giving us the actual form *huwa*; when (a) is lost, the resulting form is *hu*· (/u·/ and /uw/ are separate phonemically). In the F form we postulate the structure $\sqrt{hu}(a)-i$; (a) is lost before *i*, whereupon the sequence *u·i* is automatically replaced by *i·i* > *i·*; the form *hi*· actually exists, parallel to *hu*·; then, when conditions call for a final (a), we automatically get $\sqrt{\cdot}$ > /y/ after *i* before (a), resulting in the free form *hiya*. In *huwa* and *hiya*, then, the stem is set up as \sqrt{hu} ·; to get *hum*, *hunna*, *huma*·, we must have reduction of this to *h-* alone, otherwise the forms would be **hu·m* etc. Accordingly, we designate \sqrt{hu} · as having special morphophonemic properties, and write $\sqrt{h[u]}$ ·; the brackets are defined as meaning loss of *-u·-* before the vowel-initial affixes *-um*, *-um-na*, *-um-a*·.

In column II the masculine affix is *-hu*. This can be constructed as $\sqrt{-h[u]}$, the definition of the brackets being extended to include only loss of $\sqrt{\cdot}$ in this situation. In 3F we segment into $\sqrt{-h[u]}a$ ·, with loss of *-u·-* before a vowel; the *-a*· is a new F allomorph. The forms *-hum*, *-hunna*, *-huma*· are *-h[u]·-um*, *-h[u]·-um-na*, *-h[u]·-um-a*·. Forms like *-hi*, *-him*, etc. result from the properties of *-u·-* after preceding *i*, *i·* or *y*.

In column III the masculine 3d person has *-(a)*. The 3F has another new allomorph $\sqrt{-t}$, before which (a) is /a/. The 3P form has the already encountered *-u·* (2P in IV), with loss of (a) before vowel. The 3PF form shows F *-na*, and here again we postulate P as *-Q*·, paralleling 2PF. Then in 3PD we must have P *-Q*· before D *-a*·, since all D forms have a preceding P by this analysis. Finally we have a 3FD form, not found in columns I and II: the final is D *-a*·, before it there is F *-t*; thus F and P are seen to be mutually exclusive before D.

In column IV the prefix is *y-* in 3, 3P, 3PF, and 3PD, but *t-* in 3F and 3FD. We set this *y-* up as an allomorph \sqrt{y} · ('third person', which is lost before F *-t*). This F allomorph is different from that of 3F in III, since it occurs as a prefix. In 3P we have *-u·*, in 3PF *-Q-na*, in 3PD *-Q-a*·, as in 2P, 2PF, 2PD. In 3FD we must analyze the final *-a*· as D without a preceding *-Q*·, because this, a P allomorph, is excluded by the presence in the sequence of F *-t*; thus 2PD *taktuba*· and 3FD *taktuba*· are phonemically identical, but morphemically the first is *t-aktub-Q-a*· while the second is *(y)-t-aktub-a*·.

3. We now summarize the Classical Arabic pronominal forms structurally, using a number-and-letter notation to facilitate structural comparisons. The symbols 0-, 1-, 2-, 3- will be used respectively for 1st singular, 1st plural, 2d, and 3d person (stems or affixes). The categories in the four columns previously

presented will be indicated by a second digit, -1, -2, -3, -4; thus 'ana' is 01, 'anta' is 21, and so on. The letter F will designate the 'feminine' morpheme, the allomorphs being numbered from 1 to 6, in the order in which they appear (horizontal, then vertical) in the table; P is 'plural', with allomorphs numbered 1 to 3 in the same way; D is 'dual'. (Note that the terms *feminine*, *plural*, *dual* are labels, not definitions or descriptions; it is well known how far from corresponding to the usual assumed connotations of the terms they are in Arabic usage.)

The analyzed forms are presented in the accompanying table.

01	'ana	02	-i ~ -ya, -ni	03	-tu	04	'
11	naħnu	12	-na	13	-na	14	n-
21	'anl(a)	22	-k(a)	23	-t(a)	24	t-
21-F1	'anl(a)-i	22-F1	-k(a)-i	23-F1	-t(a)-i	24-F2	t...-i
21-P1	'anl(a)-um	22-P1	-k(a)-um	23-P1	-t(a)-um	24-P2	t...-u
21-P1-F3	'anl(a)-um-na	22-P1-F3	-k(a)-um-na	23-P1-F3	-t(a)-um-na	24-P3-F3	t...θ-na
21-P1-D	'anl(a)-um-a	22-P1-D	-k(a)-um-a	32-P1-D	-t(a)-um-a	24-P3-D	t...θ-a
31	h[u'] (a)	32	-h[u']	33	-(a)	34	(y)-
31-F1	h[u'] (a)-i	32-F4	-h[u']-a	33-F5	-(a)-t	34-F6	(y)-t-
31-P1	h[u'] (a)-um	32-P1	-h[u']-um	33-P2	-(a)-u	34-P2	(y)---u
31-P1-F3	h[u'] (a)-um-na	32-P1-F3	-h[u']-um-na	33-P3-F3	-(a)-θ-na	34-P3-F3	(y)---θ-na
31-P1-D	h[u'] (a)-um-a	32-P1-D	-h[u']-um-a	33-P3-D	-(a)-θ-a	34-P3-D	(y)---θ-a
				33-F5-D	-(a)-t-a	34-F6-D	(y)-t...-a

LOGICAL SYNTAX AND SEMANTICS

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Though considerations of meaning in linguistics can be replaced, up to a point, by rigorous STRUCTURAL procedures, i.e. procedures involving solely the kinds and order of the elements of the language under investigation, they cannot be replaced by DISTRIBUTIONAL procedures, despite the claim recently made by Harris.¹ Distributional procedures may be sufficient to establish the rules by which all longer expressions (especially sentences) can be constructed out of the elements, but they are inadequate for the establishment of certain other rules that would mirror the so-called logical properties and relations of sentences and other expressions.

It is worth while to quote at this point what the logician Rudolf Carnap had to say on this topic some twenty years ago:²

By the *logical syntax* of a language, we mean the formal theory of the linguistic forms of that language—the systematic statement of the formal rules which govern it together with the development of the consequences which follow from these rules.

A theory, a rule, a definition, or the like is to be called *formal* when no reference is made in it either to the meaning of the symbols (for example, the words) or to the sense of the expressions (e.g. the sentences), but simply and solely to the kinds and order of the symbols from which the expressions are constructed.

The prevalent opinion is that syntax and logic, in spite of some points of contact between them, are fundamentally theories of a very different type. The syntax of a language is supposed to lay down rules according to which the linguistic structures (e.g. the sentences) are to be built up from the elements (such as words or parts of words). The chief task of logic, on the other hand, is supposed to be that of formulating rules according to which judgments may be inferred from other judgments; in other words according to which conclusions may be drawn from premisses.

But the development of logic during the past ten years has shown clearly that it can only be studied with any degree of accuracy when it is based, not on judgments (thoughts, or the content of thoughts) but rather on linguistic expressions, of which sentences are the most important, because only for them is it possible to lay down sharply defined rules. And actually, in practice, every logician since Aristotle, in laying down sharply defined rules, has dealt mainly with sentences. But even those modern logicians

¹ Zellig S. Harris, *Methods in structural linguistics* 8 fn. 7 (Chicago, 1951).

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² Rudolf Carnap, *The logical syntax of language* 1-2 (New York and London, 1937). The German original, *Die logische Syntax der Sprache*, was published in Vienna in 1934.

who agree with us in our opinion that logic is concerned with sentences, are yet for the most part convinced that logic is equally concerned with the relations of meaning between sentences. They consider that, in contrast with the rules of syntax, the rules of logic are non-formal. In the following pages, in opposition to this standpoint, the view that logic, too, is concerned with the *formal* treatment of sentences will be presented and developed. We shall see that the logical characteristics of sentences (for instance, whether a sentence is analytic, synthetic, or contradictory; whether it is an existential sentence or not; and so on) and the logical relations between them (for instance, whether two sentences contradict one another or are compatible with one another; whether one is logically deducible from the other or not; and so on) are solely dependent upon the syntactical structure of the sentences. In this way, logic will become a part of syntax, provided that the latter is conceived in a sufficiently wide sense and formulated with exactitude. The difference between syntactical rules in the narrower sense and the logical rules of deduction is only the difference between *formation rules* and *transformation rules*, both of which are completely formulable in syntactical terms. Thus we are justified in designating as 'logical syntax' the system which comprises the rules of formation and transformation.

What Carnap in 1934 called 'the prevalent opinion' continues to be prevalent among contemporary linguists. The establishment of the 'statements which enable anyone to synthesize or predict utterances in the language' (*Methods* 372) is still regarded as the sole aim of descriptive linguistics, 'as the term has come to be used' (id. 5); Fries's recent book *The structure of English* (New York, 1952) has the subtitle *An introduction to the construction of English sentences*. It is the rules of formation which have caught the exclusive attention of the structural linguist;³ the rules of transformation continue to be relegated—with one notable exception, to be mentioned presently—to the limbo of an extra-linguistic logic.

This unfortunate disregard of Carnap's conception of a LOGICAL SYNTAX is not entirely the linguists' fault. Carnap himself and the logicians who followed his lead were too preoccupied with constructed language systems to devote much time and effort to an application of their views to the description of ordinary languages. Carnap even believed (op.cit. 2) that 'the statement of the formal rules of formation and transformation [of natural languages] would be so complicated that it would hardly be feasible in practice'. This belief is certainly correct for a COMPLETE statement of the rules, but fairly good approximations should be achievable with some effort. This effort has been expended, as a matter of fact, for the rules of formation; there is no good reason for not doing the same for the rules of transformation. Indeed, beginnings for such an undertaking exist already; but these are the work of logicians and philosophers, who are often biased by their underlying metaphysical conceptions and their adherence to Aristotelian or Scholastic ways of thinking.

It is the duty of the structural linguist to scrutinize Carnap's conception carefully; after all, Carnap is not a linguist proper. If and when his conception is found to be linguistically sound, as I think it is, we shall have to give up Harris's

³ The term 'structural linguist' is used throughout this paper to mean 'some American structural linguists', of whom Bloch, Harris, Hockett, Smith, and Trager are a representative sample.

contention (*Methods* 5) that 'the main research of descriptive linguistics, and the only relation which will be accepted as relevant ..., is the distribution or arrangement within the flow of speech of some parts or features relative to others'. Instead, LOGICAL ANALYSIS, based upon the relation of DIRECT CONSEQUENCE,⁴ will have to be given equal rights with DISTRIBUTIONAL ANALYSIS.

This will entail, of course, a radical change in the official conception of elicitation techniques. Until now the informant has been required only to supply repetitions of sound sequences, to judge whether two sound-sequences are the same or not, to tell whether certain sound-sequences are sentences or not, and to determine whether 'two items are the "same" in a particular aspect of meaning or "different"'.⁵ It now becomes necessary to develop techniques of elicitation for logical analysis which will have the same degree of reliability and validity as those developed for distributional analysis. Direct questioning ('Do *oculist* and *eye-doctor* mean the same to you?') may serve as a first approximation, to be replaced in time by more objective methods. Notice that Harris himself indicates, at one place, an elicitation technique to this effect when he says,⁶ 'one can read the text sentence ... in company with an informant, and then stop and say to him, in an expectant and hesitant way, "That is to say, ...," waiting for him to supply the continuation.' However, Harris seems not to be aware that what he gets from the informant by this technique is much more than information about distribution. Since structural linguists have no doubt been using such techniques in their practical work, it is of utmost importance that they make these techniques explicit, become aware of their theoretical function, and analyze and improve upon them.

Most structural linguists seem to have recognized that not all aspects of linguistics can be handled by distributional analysis alone; but there is one who attempted the seemingly impossible. Wishing to exploit this kind of analysis to its utmost, Harris has claimed that he can describe in purely distributional terms both synonymy relations (say between *oculist* and *eye-doctor*) and the active-passive relationship (say between *plays* and *is played by*); and he would probably undertake, if challenged, to do the same with respect to the difference between Latin *aut* and *vel*. According to his basic postulate (*Methods* 7 fn. 4), 'It may be presumed that any two morphemes having different meanings also differ somewhere in distribution.' It would then seem to follow that any two morphemes with the same distribution have the same meaning. If we were to grant these presumptions, then indeed many of the transformational aspects of language, if not all of them, would be reducible to the formational aspects. But in spite of some initial plausibility, the presumptions are false. Whatever conviction they carry is due to the fact that many of the terms involved are equivocal. It will be worth while to analyze some of these equivocations.

The first is *language* itself. This term is sometimes understood as the totality of all possible sentence-types (or utterance-types), sometimes as the totality of all actually uttered sentence-tokens,⁷ or perhaps as the totality of all sentence-

⁴ See *Logical syntax* 170.

⁵ *Structure of English* 8 fn. 6.

⁶ Discourse analysis, *Lg.* 28.20 fn. 13 (1952).

⁷ Following the usage of Charles S. Peirce, a SIGN-TYPE is the abstract class of all con-

tokens that have been uttered or will be uttered until the extinction of the users of this language.

Now if we consider a language as the totality of all possible sentence-types, Harris's presumption is clearly false. *Green* and *red* are surely different morphemes, but their distribution within this totality (with respect to English, of course) is ALMOST exactly the same, i.e. the same up to a subset of special environments which will cause trouble to any consistent and would-be simple description. This subset, to use a fitting mathematical metaphor—which is meant, however, more to help us dodge the problem than to solve it—is of measure zero. It would contain, for instance, the environments *-horn*, into which only *green* would fit but not *red*, and *-skin*, into which only *red* would fit but not *green*. Except for such cases, within the totality of all possible sentence-types, there is for any sentence containing *red* a (significant) sentence that contains *green* instead, and vice versa.

However, if we take a language to be the totality of all sentence-tokens uttered up to a certain time (or alternatively, the totality of all sentence-types of which tokens have been uttered up to a certain time), then—disregarding a coincidence of cosmic dimensions—no two morphemes will show the same distribution. For this interpretation Harris's presumption would indeed turn out to be true, but in such a trivial fashion that it can hardly be what he meant. The relations between *oculist* and *eye-doctor*, *oculist* and *dentist*, *oculist* and *beauty*, *oculist* and *green* are of four different types, each of which must be the concern of the structural linguist. To state only that within a given totality of sentence-tokens all these morphemes exhibit different distributions is surely not the whole truth—in fact only a small part of it. Even to state that *oculist*, *eye-doctor* and *dentist* have almost equal distribution within the totality of all sentence-types, whereas *oculist* and *beauty* have overlapping distribution, and *oculist* and *green* have almost exclusive distribution, though incomparably more revealing, still misses the essential difference between the first two pairs. Sameness of distribution, within the type-totality, is perhaps a necessary but certainly not a sufficient condition for sameness of meaning; hence, difference of meaning is perhaps a necessary but certainly not a sufficient condition for difference of distribution, with respect to the same totality.

Since 'sameness of distribution' and 'sameness of meaning' are certainly not convenient terms, other terms are usually employed instead. Here, however, another equivocation becomes effective. *Oculist* and *dentist* are SUBSTITUTABLE in the sense that any⁸ sentence containing the one will turn into a sentence (not necessarily a sentence with the same meaning or even with the same truth-value) when this is replaced by the other. *Oculist* and *eye-doctor* are substitutable in the sense that any sentence containing the one will turn into a sentence with necessarily the same truth-value when this is replaced by the other. For both these

create SIGN-TOKENS which (by some criterion) belong-to-the-same-type. Cf. Y. Bar-Hillel, A quasi-arithmetical notation for syntactic description, *Lg.* 29.49 (1953); Cherry, Halle, and Jakobson, Toward the logical description of languages in their phonemic aspect, *Lg.* 29.45 fn. 13 (1953).

⁸ From now on the qualifier *almost* will be omitted.

essentially different relations, the term *substitutable* (or *replaceable*, or *commutable*, or even *equivalent*) is used indifferently, more often than not without even a qualifying adverbial. A consistent use of qualifiers like *distributionally* and *logically* (or, more fancifully, *salva significatione* and *salva veritate*) could assist in avoiding the pitfalls connected with this equivocation; but a convention to use, say, *commutable* for the first sense and *interchangeable* for the second, would be even better.

There has been recently a lively discussion of the degree of interdependence of the various structural 'layers'.⁹ It seems to me that part of the purists' insistence on a sharp demarcation between phonology and grammar is based on the assumption that a treatment of the syntactical-transformational aspects of language is in constant danger of succumbing to an infestation by meaning, an evil from which those aspects of language that can be shown to be independent of syntax can be saved. Indeed, so long as syntax, as traditionally handled, was a MEANING SYNTAX, it was methodologically worth while to adopt the procedure of Trager and Smith in *An outline of English structure* (1951), which, based upon purely distributional analysis, rigidly discriminates between ascending levels of complexity of organization. If only one could establish phonemics without recourse to grammar, the danger of letting semantic considerations creep in would be considerably reduced if not completely eliminated. I believe that some of the attractiveness of this attitude is reduced by recognizing that a description of the transformational aspects of syntax can be just as free of meaning as other parts of a linguistic treatment.

The fear of allowing meaning to intrude is of course only one reason for the sharp-level approach. Another, probably more important, is the fear of circularity. If (say) the term 'morpheme' is used in the definition of the phoneme, and vice versa, then certainly this procedure looks viciously circular, and the logical soundness of the science that uses such definitions is gravely jeopardized. Pike, however, has given sound reasons why this fear, in its generalized form, is groundless. Moreover, it is possible to show that certain types of concept introductions which look circular are not so in fact—types in which the elimination of the newly introduced term does not involve an infinite regress. As a matter of fact, concept formations of these kinds are in regular use in mathematics, and especially in mathematical logic, where they are known as special cases of RECURSIVE DEFINITIONS. It seems rather likely (though a detailed proof would require many man-hours of work) that Pike's nine-step procedure (op.cit. 120) can be formalized and adequately represented by a set of such definitions. Since I have treated this topic elsewhere at some length,¹⁰ I shall say no more here.

It is an interesting fact, deserving the attention of sociologists of science, that at approximately the same time, but in complete independence of each other, Bloomfield and Carnap were fighting the psychologism that dominated their respective fields, linguistics and logic. They both deplored the mentalistic mud

⁹ Kenneth L. Pike, More on grammatical prerequisites, *Word* 8.106-21 (1952), and the references mentioned there.

¹⁰ On recursive definitions in empirical science, *Proc. 11th Internat. Congress of Philosophy* 5.160-5 (Brussels, 1953).

into which the study of meanings had fallen, and tried to reconstruct their fields on a purely formal-structural basis. I think it is correct to say that the difference between the structural linguist and the formal logician is one of stress and degree rather than of kind. Both are essentially attempting to construct language systems that stand in some correspondence to natural languages—though most linguists would say that they are just describing the latter. But whereas for the linguist the closeness of this correspondence is the criterion by which he will judge the adequacy of the language system he is setting up, which alone entitles him to consider himself as describing a given natural language, the logician will look primarily for other features of his system, such as simplicity of handling, fruitfulness for science, and ease of deduction and computation, with close correspondence to a natural language as only a secondary desideratum. Constructed language systems are judged by the linguist according to the degree to which they approximate a natural language; natural languages are judged by the logician according to the degree to which they approximate efficient, well-constructed language systems.

I have gone into these generalities in order to emphasize the following point. A few years after Carnap's elaborate attempt to show that *prima facie* semantic considerations can be satisfactorily mirrored in formal syntax, he reversed himself completely, reintroduced semantics into logic, and dedicated to it most of his later studies.¹¹ This development is not surprising if we remember that semantics was no longer, by this time, the hodgepodge that went under this name in the first quarter of our century and so much repelled Bloomfield. In the early thirties, Polish logicians of the Warsaw-Lwów school—mainly T. Kotarbinski, A. Tarski, and K. Ajdukiewicz—gave this science a foundation which made it fully competitive with LOGICAL SYNTAX. This was achieved both through an extensive and skillful use of the symbolism of mathematical logic, and by deliberately abstracting from the users and usages of the signs under study and considering only their relations to what was signified by them. Bloomfield's strictures against semantics and the use of meaning for linguistic description, though valid against the state of that field at the time he wrote, do not hold against this revitalized science, in the form given to it by Tarski,¹² Carnap, Quine,¹³ and others. I have no intention of bridging the abyss between those linguists who STILL use semantic considerations in their analysis and those who use them AGAIN, just as I do not wish to minimize the corresponding difference between philosophers. I believe that only those who have followed the syntactical method to its very end will be able to appraise adequately the status of the new semantics in descriptive linguistics.

My plea for the reintroduction of semantics into the theatre of operations of

¹¹ *Foundations of logic and mathematics*, International encyclopedia of unified science, Vol. 1, No. 3 (Chicago, 1939); *Introduction to semantics* (Cambridge, Mass., 1942); *Meaning and necessity* (Chicago, 1947).

¹² *Der Wahrheitsbegriff in den formalisierten Sprachen* [written 1933], *Studia philosophica* 1.261-405 (1936); *The semantic conception of truth and the foundation of semantics*, *Philosophy and phenomenological research* 4.341-76 (1944). The latter is reprinted in *Readings in philosophical analysis* 52-84 (ed. H. Feigl and W. Sellars; New York, 1949) and in *Semantics and the philosophy of language* 13-47 (ed. L. Linsky; Urbana, Ill., 1952).

descriptive linguistics will be strengthened, I think, if we follow Carnap and Quine in showing that the term 'semantics' has been understood, traditionally as well as by the Polish school of logicians, to contain two rather separate theories with two different sets of concepts. The one theory deals with the INTENSIONAL or CONNOTATIONAL aspects of language or other sign systems, i.e. MEANING (in a restricted sense of this word), the other with the EXTENSIONAL, DENOTATIONAL, or REFERENTIAL aspects. Carnap calls them¹⁴ THEORY OF INTENSION and THEORY OF EXTENSION respectively, Quine calls them¹⁵ THEORY OF MEANING and THEORY OF REFERENCE. The first member of each pair deals with such concepts as logical truth, logical equivalence, and synonymy; the second member deals with truth, equality in truth-value, and coextensiveness. To find out whether a given statement is logically true, whether two statements are logically equivalent, or whether two expressions are synonymous, one needs to know only their intension (connotation, meaning); to find out whether a certain statement is true, whether two statements have the same truth-value, or whether two expressions are co-extensive, one must also make detailed observations, or rely on experience. But it is certainly not the linguist's business, or for that matter the logician's, to find these things out. That *morning star* and *evening star* are coextensive (denote the same physical entity) is hardly of interest to the logician qua logician or to the linguist qua linguist; it is, rather, the astronomer's duty to find this out. Whether the statement *All cats have tails* is true is certainly not an exclusively linguistic problem, but rather one for the zoologist.

A linguist who decides that it is not his concern to find out which English statements are true and which English expressions are coextensive is fully justified; he can safely disregard these (pseudo)-semantic aspects of language. Some linguists, however, have thrown away the baby with the bath water. By totally discarding semantics, they have committed two sins. First, it is very definitely the linguist's concern that *oculist* and *eye-doctor* are not only commutable but synonymous (co-intensive), and a description that contained no statement of this fact would be seriously inadequate. (If this is agreed upon, then it must be equally the linguist's concern to state that from *All Greeks are men* and *Socrates is a Greek*, follows *Socrates is a man*; and he should no longer—except for pragmatic reasons—leave such statements to his colleague the logician.) Secondly, both THEORY OF MEANING (in addition to meaning itself) and THEORY OF REFERENCE (though not reference itself) are of vital importance to him, since—like every other scientist—he has to worry from time to time about methodological questions.

It has been the purport of this paper to establish the following four points.

(1) There exists a conception of syntax, due to Carnap, that is purely formal (structural) and adequate in a sense in which the conception prevalent among American structural linguists is not. This conception entails a certain fusion

¹³ For instance, Notes on existence and necessity, *Journal of philosophy* 40.113–27 (1943), reprinted in *Semantics and the philosophy of language* 77–91.

¹⁴ So far as I know, Carnap has not used these terms in print, but they fit the main argument of *Meaning and necessity*, and were used by him in correspondence.

¹⁵ For instance, Two dogmas of empiricism, *Philosophical review* 60.22 (1951).

between grammar and logic, with grammar treating approximately the formational part of syntax and logic its transformational part. The relation of COMMUTABILITY may be sufficient as a basis for formational analysis, but other relations, such as that of formal CONSEQUENCE, must be added for transformational analysis. Since modern techniques of elicitation have been developed mainly with distributional analysis in view, a new approach is required that will yield reliable techniques of elicitation for the establishment of synonymy and the like.

(2) A recent attempt by Harris to reduce the transformational part of syntax to its formational part is based on a series of equivocations in the terms *language*, *equivalent*, *commutable*, and their cognates, and so is without foundation.

(3) The tendency exhibited by many contemporary structural linguists to set up sharp demarcation lines between the various linguistic subfields is presumed to be based, first, on the attempt to keep linguistics as far as possible independent of concepts open to the intrusion of meaning and, secondly, on a fear of circularity in the definition of basic terms. But Carnap has shown that even the transformational aspects of syntax can be described without appeal to meaning; and recent methodological studies of concept formation indicate that certain procedures with a circular look are in fact harmless because the terms introduced by them can be finitely eliminated.

(4) The generalized fear of letting meaning intrude into linguistics seems to rest mainly on the fact that in the first quarter of this century the study of meaning was indeed in a bad methodological state. But since then, mainly through the efforts of Polish logicians, semantics has become a well-defined, rigorous field. This change has caused Carnap to reintroduce semantics into logic, and should cause descriptive linguists to follow Carnap's lead.¹⁶

¹⁶ For a somewhat different approach to the problems discussed here, see Haskell B. Curry, *Mathematics, syntactics, and logic*, *Mind* 62.172-83 (1953).

REVIEWS

Syntaxe latine. By ALFRED ERNOUT and FRANÇOIS THOMAS. (Nouvelle collection à l'usage des classes, Vol. 38.) Pp. xvi, 416. Paris: Librairie C. Klincksieck, 1951.

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This is an important and excellent book. It deserves, and demands, an extended review.

There are two other works with which this is naturally compared. One is *Syntaxe grecque* by Jean Humbert, also one of the valuable Klincksieck publications (Paris, 1945). The scientific objectivity of the ET¹ approach seems to me vastly superior to Humbert's tendency to find philosophical and psychological implications in the facts of syntax.² The other is the standard treatment of Latin syntax by Joh. Bapt. Hofmann, in the revision made jointly by him and Manu Leumann of the Stolz-Schmalz *Lateinische Grammatik* (Munich, 1928). This twenty-five-year-old volume, though sorely in need of its oft-promised and long-overdue revision, will not be supplanted, though it may be supplemented, by ET. The two works in their relative strength and weakness are typical of German and of French scholarship. The German work is more exhaustive:³ its text covers almost half again as much space, and its indexes more than half again;⁴ and

¹ ET stands for 'Ernout and Thomas' or for '*Syntaxe latine* by Ernout and Thomas', according to the context. *Morphologie* and *Recherches* refer to other works by the authors individually: Ernout, *Morphologie historique du latin* (Paris, 2nd ed. 1927, 3rd ed. 1953); and Thomas, *Recherches sur le subjonctif latin* (Paris, 1938). SO represents the reviewer's monograph, *Subjunctive and optative: Their origin as futures* (New York, 1953), to which a number of references are made in order to avoid the repetition here of what has been said elsewhere.

² For examples, see SO: on Humbert, 4 fn. 10, 6, 14-5, 15 fn. 31; on ET, 6 fn. 12. However, even in ET there is an occasional trace of the teleological taint that usually accompanies the psychological approach in linguistic explanations. There are suggestions not merely that authors are aware of questions of style (as of course they are), but that changes in language are conscious and deliberate. In unreal conditions 'peut-être y eut-il un essai pour utiliser l'adjectif en -ndus' (337). The alternation of primary and secondary tenses in indirect discourse was 'un moyen d'alléger cette forme de style particulièrement lourde' (363); I believe it really springs from the same tendency as led to the alternation of perfect and historical present in direct discourse, and implies no special difficulty in indirect discourse (nor does it even constitute a special peculiarity of indirect discourse, though it has been given a special name, *repraesentatio*—on which see below, fnn. 32 and 148). Colloquial Latin preferred *quod* clauses to infinitives because they avoided ambiguity (272); is not ambiguity usually cleared up automatically by situation and context? The perfect in result clauses was an attempt to distinguish result from purpose (353); I think it was used because it best expressed what needed to be expressed (cf. fnn. 23, 97, and 141). Caesar (311) and Petronius (298) were actuated by purism, Livy and Tacitus (224) by a desire to react against a certain colloquial usage; possibly, but how do ET know it?

³ Some points, however, are treated more fully by ET; thus, they devote almost a page (120-1) to a very interesting discussion of agreement with an implied genitive, a point dealt with by Hofmann in less than five lines (393).

⁴ This is a rough estimate only, allowing for the greater size of the pages of the German work but not for the much greater compactness of its arrangement, especially in the

above all it is rich in bibliographical material. But the French work is far easier and pleasanter to read: it achieves clearness rather than compactness of presentation; and it has the advantage of citing passages in full, giving us the benefit of the context together with very fine French translations, which by revealing the grammarians' interpretation further clarify the grammatical points under consideration.

I have compared ET with Hofmann because it will certainly appeal to precisely the same group of readers. It is evidently intended, like its companion volume Ernout's *Morphologie*, for university students at every stage of their advancement (vii); but, even though we must grant that the training of students in France is more thorough and scholarly than in this country, I believe that the outstanding features of this work qualify it to serve rather as a manual for scholars than as a text for students just out of the lycée.⁵ These features include the successful endeavor to explain the origin and evolution of syntactic facts (cf. viii), and above all the treasure house of examples culled from every stage and every style of Latin literature, particularly stimulating in the juxtaposition of variations in usage occurring either for some good reason or for no apparent reason at all! Truly salutary is the presentation side by side of two seemingly parallel passages from a single author with inexplicable alternations in construction,⁶ or the quotation of a single line in which perfect and imperfect tenses are coordinated seemingly with no nuance of distinction.⁷ Such clever details of selection and arrangement are a reminder that our 'rules' are crutches which we have fashioned to support our own halting steps through the field of Latin, whereas the Roman strode freely without them.

But in sorry contrast with this wealth of illustrative material is the poverty of the bibliography. Though this is described as naming those scholars to whom the authors are specially indebted (ix) and as limited to general or significant

indexes. In dealing with these, I am taking into account the fact that they cover morphology and stylistics as well as syntax.

⁵ Yet there are occasional strangely elementary touches that startle us by their incongruity. For instances of what I mean, see 9, 287, 289, 328, 347, 358, 372. The disproportionate emphasis on works in French, and the frequent approach to Latin via French, to both of which I shall return later, may have the same origin.

⁶ Thus, from Cicero, restrictive clauses with indicative vs. subjunctive (288-9), attraction vs. non-attraction of a relative to the gender of a predicate noun (117); from Caesar, the halting of the enemy eight miles from camp in the accusative, and a little later six miles in the ablative (94).

⁷ The line is Catullus 63.64, *ego gymnasi fui flos, ego eram decus olei* (189). Wilamowitz-Möllendorff (*Hermes* 14.198) placed a period after *flos*, but this destroys the parallelism of the two clauses, and, by uniting the first half of the line more closely with the preceding one, interferes with the chronological sequence there presented of successive steps, in reverse, of Attis's life. Friedrich in his edition of Catullus calls the shift in tense an 'Inkorrektheit' induced by metrical necessity; but a poet like Catullus is not such a slave of his meter—even the difficult one employed in 63—as to be driven by its exigencies to an 'Inkorrektheit'. The examples which Friedrich cites from the *Aeneid* are not true parallels; in 4.675 *petebas* involves a notion of progressive action lacking in *fuit*, and in 12.147 *sinebant* is really parallel in time not to *visa est* but to the dependent infinitive *pati*. In Catullus the use of two different tenses seems to me simply to involve approaching parallel situations from different viewpoints.

works (x), still we cannot but regret the omission of others, particularly those on this side of the Atlantic. In striking contrast to Hofmann, to whom we shall have to continue to turn as the starting-point of research despite his lack of references to the last quarter-century, hardly any citations are given in the course of the text, and those that are included are almost exclusively confined to French publications.⁸ In the preliminary bibliography, to be sure, a fair number of works in German are listed (15 in all, plus 3 in French translation); why, then, so few (only 2 from England and 3 from America) in English? The two English books, on the syntax of Plautus and Terence respectively, may well have provided helpful examples, but they are not in a class with Handford's monograph on the Latin subjunctive (1947), in my opinion the best treatment of the subject in any language. Of the American books, Buck's *Grammar of Oscan and Umbrian* was doubtless useful, and Bennett's *Syntax of early Latin* is indispensable to all workers in Latin syntax; but if the Hale and Buck Latin grammar is included, then why not those of Lane and of Gildersleeve and Lodge? Granted that England and America have less to offer in book form than France and Germany, what of American periodicals? What of *TAPA*, *AJP*, and perhaps *CP*, and of the special serial volumes issued by various universities? And what, more recently, of *Language* and *Word*? ET list just twelve periodicals, five apiece from France and Germany, one apiece from Belgium and Spain.

The proofreading has been amazingly accurate. It is said there is no book without a misprint; I have caught just two indubitable ones, both trivial—*nominatiuu* (11) and a comma after *intro* in *Rud.* 1177 (318). More serious is the inconsistency in punctuation in the citation of *Bell. Gall.* 1.44.8 (358); if the direct form is properly reconstructed as *quid tibi vis cur venias?*, the comma after *vellet* in the indirect form (given as *quid sibi vellet, cur veniret*) should be omitted (I would rather keep it, and offer for the direct form *quid tibi vis, cur venis?*). — The negatives *nemo*, *nihil*, *neque* surely belong with *non*, not with *ne* (195). — 'Accusatif d'objet' must be a slip for 'accusatif de sujet' (275), and 'objet interne' and 'objet externe' should be reversed (31). — In a paragraph dealing with 'écrivains non classiques' (376), it is surprising to find Sallust, Livy, later Ovid, Vergil, Juvenal, even Cicero himself.

I have checked many of the references through interest in context, but this has also served as a test of accuracy, which the book has met admirably. Here I have found only three mistakes: *Leg. Agr.* 2.63 (308) should be 2.64, *Leg. Agr.* 2.92 (348) should be 2.93, and *Aen.* 9.426 (108) should be 9.427. I question the insertion of *Ver.* in the reference to Cicero, '*Ver., Diu. Caec.* 19' (321). We find *Per.* (for Plautus' *Persa*) in the bibliography (xv), but *Pe.* in the text (e.g. 207). More serious than any of the foregoing is the reference to 'Ernout, *op. cit.*,

⁸ For instance, there actually is a reference (170) to a German article by Hofmann himself, but it is one that appeared in a French publication (*Mélanges Marouzeau*); cf. fn. 5. To note just two instances of American publications that might well have been included, Hale's pioneer work on the subjunctive in the 'ideal' second person singular, long ago though it appeared, well deserves citation; and *The ab urbe condita construction in Greek* by Frank Pierce Jones (Language Dissertation No. 28, 1939), would have furnished qualification for the sweeping statement in regard to the *ab urbe condita* construction in Latin that 'le grec connaît à peine ces tournures' (237).

(174); I wasted considerable time in an attempt to track this down before I discovered that the *op. cit.* was *MSL* 15, listed almost two pages earlier (173), though a reference to Ernout's *Morph.* had intervened. — The method of citation is highly sensible. Arabic numerals rather than Roman are regularly employed. References to Cicero use only the numbers of the small sections, not those of chapters.

The orthography is consistent. The letters *i* and *u* are used for consonants and vowels alike. This is unquestionably preferable to the common American practice (followed even by so precise a scholar as Buck) of designating consonantal *i* by *i* and consonantal *u* by *v*. However, it would certainly be more convenient if every one followed the few scholars (e.g. Bennett in his Latin grammar) who use *j* for consonantal *i*. Such spellings as *uolui* (172) and *parui* (335) are logical but misleading, and *adiuui* (344) has a bewildering appearance. — In general quantities are not marked. This seems to me regrettable; but still more regrettable in my opinion is the fact that macrons are occasionally—though by no means consistently—introduced, evidently in a desire to avoid possible confusion between two words identical in spelling but different in vowel quantity.⁹ I believe that in a manual of grammar the macron should be employed as a guide to pronunciation rather than to lexicography, and that its use should be consistent and uniform.¹⁰

A valuable feature of the work is the determination of the authors, stated as early as the Préface (ix), never to lose sight of the fact that Latin syntax 'est une construction originale, résultat d'un développement autonome, qu'elle doit s'exposer et s'expliquer en elle-même et pour elle-même'. This ringing declaration of independence is made specifically with reference to Greek: Hellenisms due to classical writers' deliberate imitation of their Greek models, or Christian writers' translation of the New Testament from koine Greek into Latin, are of course explained as such, and occasional interesting comparisons are made; but, very properly, Latin syntax is not approached from the viewpoint of Greek syntax.¹¹ All the more striking then is the serious error into which ET fall in their repeated habit of treating the facts of Latin in terms of French.¹²

⁹ The conjunction *nē* generally has the macron (e.g. 183, 195, 291, 349) but sometimes lacks it (e.g. 197 line 4); note too *nē ... quidem* (132) but *ne ... quidem* (380), and *nēdum* (132) but *nedum* (133). The old negative *nē* has a breve (130, 248, 256) but not the interrogative particle *-nē* (136, 137, 229, 268); perhaps the hyphen is considered sufficient identification. The second-conjugation infinitive *pendēre* is written both with (27) and without (86) the macron; the third-conjugation *pendēre* is written without the breve (46). The impersonal *rēfert* 'concerns' has the macron once (155) but more often lacks it (88, 217, 257, 278). *Tōtus* 'whole' never has the macron; yet there is another adjective *tōtus*. On two successive pages (99–100) we find *eā, hāc, antēā, postēā*; but *antehac, posthac, hactenus, quatenus*.

¹⁰ Not quite so objectionable is the occasional use of a macron or a breve to emphasize the occurrence of a particular case, as *flammā* (61), *Cynthiā* (12).

¹¹ Special sinners in this respect among American scholars have been Hale and the authors of the Allen and Greenough Latin grammar 'founded on comparative grammar'. See *SO* 17–8 on the first, and 17 fn. 34 on the second.

¹² Emphasis on similarities, and still more on differences, between the foreign language being studied and the learner's mother tongue is suitable and valuable in the instruction of tyros; but ET, whatever its intentions, is really useful only for veterans.

Many phenomena are explained by the statement that in respect to them Latin is like French¹³ or different from French;¹⁴ in many of these cases English or some other modern language would serve as well as French, and in some even better.¹⁵ If the French uses were cited as features of modern Latin, they would be quite justified; but when this is done, as in the cases of derivatives from the Latin comparatives *melior*, *peior*, etc. (147), from Latin indefinites (165-6),¹⁶ or from Latin conjunctions (370), why are not other Romance languages cited as well as French?

Perhaps it is almost impossible to approach a foreign language wholly without prejudice induced by one's vernacular.¹⁷ ET cite as an example of the omission of a possessive adjective (153) the vocative Latin *patrue* (so too English *uncle*; but French *mon oncle*). They view as worthy of comment the fact that a verb needs no pronoun-subject (123), which a speaker of Italian would take in his stride; and that there is no indefinite pronoun like French *on* (124), which a speaker of English would take in his stride. The latter would not find necessary their stress on the existence in Latin of completely separate forms for the comparative and the superlative, their care in distinguishing the two,¹⁸ and their seeming surprise that when two objects are compared the comparative and not the superlative is used to indicate 'la qualité la plus haute' (143), and that *multo* can reinforce not only the comparative but also the superlative (147).¹⁹ On the other hand, he might go along with them in singling out as worthy of note the use of the future tense in a conditional clause (318), and he might have added the same use in a temporal clause, which they ignore (they are obviously influenced by the fact that in references to the future French uses the present

¹³ For examples, see 110, 145, 177, 326, 349, 354, 368. Similarly, it is said that the Greek verb, differing from the Latin, usually stands in the middle of the sentence 'as in French' (127)—and as in many other languages too.

¹⁴ For examples, see 114, 151, 159, 283, 334.

¹⁵ E.g. the English construction is closer than the French to the (rare) Latin use of *facio* with an infinitive (278); the special French distinction between *je le fais venir* and *je lui fais lire un livre* has no counterpart in Latin or English. Latin is said not to use a demonstrative as French does (163) in *conferre vitam Trebonii cum Dolabellae* 'comparer la vie de T avec celle de D'; in English we can use either the French construction 'compare the life of T with that of D' or the Latin 'compare T's life with D's'. (Incidentally, the example of the demonstrative cited by ET from *Arch.* 28 is quite different; here *hanc* corresponds to *nullam aliam*.)

¹⁶ Exceptionally, Italian *caduno* is cited along with French *chacun* (168), but there is no mention e.g. of Italian *alcuno* along with French *aucun* (165).

¹⁷ Thus in rewriting Cicero's *nec intellegebam fieri diutius posse* to show the force of *nec*, ET change (surely unwittingly) the word order to one characteristic of French—*et intellegebam non posse fieri diutius* (375). This is of a piece with the interesting slip, pointed out by ET themselves (310), which Cicero made when, in quoting *Aul.* 178 *cum exibam*, he substituted, doubtless automatically, the subjunctive *exirem* for the indicative *exibam*, thus making Plautus's mood usage conform to his own.

¹⁸ Further, we would call *maior* in *maior puerorum* a comparative pure and simple, and not, as they do (41), a comparative 'en fonction de superlatif'.

¹⁹ As a parallel for the colloquial use of redundant *magis* with a comparative (148) we might have cited the corresponding Shakespearian *his more braver daughter* (so too in the superlative *the most unkindest cut of all*).

tense with *si* but the future tense with *quando* or *lorsque*).²⁰ Similarly, after declaring, superfluously, that the lack of a future subjunctive did not often produce any 'gêne'²¹ (334), ET list several passages (335-6) in which the simple (not the periphrastic) present or imperfect subjunctive refers to the future;²² these are all indirect questions or clauses of doubting, and I naturally thought of adding clauses of fearing, but these would be less likely to occur to a Frenchman, who can no more distinguish by a simple verb than a Roman could between 'I'm afraid he is sick' and 'I'm afraid he *will* be sick', and who neither suffers nor envisages any 'gêne' as a result.²³

More serious than this implicit acceptance of the norm in terms of French usage is the giving of rules and explanations for Latin in terms of French translations. Why cite *aquam bibo* in connection with the partitive genitive (40)? Latin no more distinguishes *aquam amo* and *aquam bibo* than does English *I like water* and *I drink water*; it is only the Romance languages that make separate categories of the general noun (French *j'aime l'eau*) and the partitive (French *je bois de l'eau*). On the other hand, French uses the same construction (a prepositional phrase with *à*) for rest and motion; so ET are more ready than I to accept Trimalchio's *fui in funus*, Fr. *j'ai été à un enterrement* but Eng. *I was to a funeral*, as a normal form of expression (28).²⁴ Latin often uses an adjective in agreement with the subject and closely connected with the verb; ought we say that the adjective is used 'avec la valeur d'un adverbe' (142) just because French (or

²⁰ Similarly, to me *Epid.* 201 *quis est qui revocat?* seems a rather surprising Celticism to meet in a Roman poet. But ET, doubtless because of its similarity to the French *qui est-ce qui rappelle?*, cite this 'fuller' form of interrogation (139) without special comment.

²¹ If a language habitually refrains from making a certain distinction, the inability to do so causes its users no inconvenience.

²² This is of course natural, since the subjunctive itself points toward the future, as ET clearly indicate elsewhere (184); furthermore, in the few instances where ambiguity might result, a periphrasis is available. But this was rare in early Latin; and it must not be forgotten that in the parent language (as still in Hittite) there were only two tense-realms, past and present-future. Note that in early Latin the present infinitive, like the present subjunctive, frequently refers to the future, and for it too the more precise periphrastic substitute is rare (cf. Bennett, *Syntax of early Latin* 1.426-7).

²³ Despite these comments, I do not deny that as a result of its smaller tense range the subjunctive is sometimes less flexible than the indicative. It is interesting to note that in the transcription of indirect into direct discourse (366) ET, doubtless rightly, offer two possible indicative equivalents with different meanings (*redeunt* and *leguntur* or *redibunt* and *legentur*) for the subjunctives *redeant* and *legantur* (but I think the only possible equivalent for *videant* is *videbunt*, not *vident*). The past subjunctives, too, though there are more of them, are in some ways inadequate; in the result clause in past time, the imperfect and pluperfect are often unsatisfactory from the standpoint of aspect, and the perfect from that of sequence. However, the difficulty here is not due so much to a deficiency on the part of the subjunctive mood, as to the fact that it is really forced to do duty here where an indicative might seem to be called for; cf. fnn. 95 and 97.

²⁴ Yet elsewhere (95) surprise is expressed at the confusion of notions so distinct as those of *quo* and *ubi*. They were probably 'confused' in the parent speech, where dative and locative were a single case; and they are again 'confused' in many modern languages (*where*, *où*). However, the standard use of *ad* 'in the neighborhood of' doubtless stems from the joint idea of approaching a point and then stopping. Cf. further fn. 56.

some other language)²⁵ would use an adverb or an adverbial phrase there? Among the examples cited is *illi inanes revertuntur*, translated 'ils reviennent les mains vides'. We might well render this 'they return empty-handed'; must we conclude that in English too the adjective has the force of an adverb? — It may be the custom in French to analyze *il est honteux de mentir* by calling *il* the subject and *de mentir* the 'complément d'objet' (217; cf. 178 and 225); but in Latin *turpe est mentiri* there is nothing to correspond with French *il*, and the infinitive is certainly, it seems to me, the subject and not the 'complément d'objet', despite ET.²⁶

The circumstance that Latin has a true passive and French has not leads the authors into what seem to me surprising errors as to both voice usage and tense usage. Thus we read (175): 'Le passif personnel, en effet, n'implique pas nécessairement que le sujet subit l'action.' As an example is given *dabitur tibi amphora*: 'la question de savoir qui la donnera n'intéresse pas'. Is there not confusion here of subject and agent? (It is only in the active or the middle that they are the same.) Certainly *amphora* is the subject of *dabitur*, and does receive or undergo (subit) the action. An example in the first person is added: *lepidus vocor* is 'on m'appelle charmant' and not 'je m'appelle'. Of course the Latin passage means what 'on m'appelle charmant' means, because here French idiom demands *on* and not the reflexive;²⁷ but the English construction 'I am called charming' comes much closer to the Latin, and no matter how we translate, *vocor* is a true passive and the 'subject' (included in the verb) does receive the action.

The present passive is said to have the value of a perfect in *vicus montibus continetur* (194), and the perfect *ab hoste circumdati sumus* is said to correspond as well as *circumdatur* to the active *hostis nos circumdat* (ib.). Both these statements I deny: *vicus montibus continetur* corresponds to *montes vicum continent*, and *ab hoste circumdati sumus* corresponds to *hostis nos circumdedit*. Both in French and in English, the combination of 'be' with the passive participle varies in force: with some verbs the completion or result of a past action is implied, as in *the die is cast*, *the book is written*, *the door is closed*, *la porte est fermée*, and to indicate present (durative) action we must use some special form, *the door is being closed*, *la porte se ferme* or *on ferme la porte*; but with other verbs the periphrasis of itself implies a durative action, *the book is read everywhere*, *children should be seen and not heard*, *he is loved*, *il est aimé*.²⁸ Our translation of *continetur*

²⁵ On the other hand German in such an instance would not distinguish in form between an adjective and an adverb.

²⁶ This is of course why impersonal verbs of the type of *pudet* are accompanied by an infinitive (nominative) and not by a gerund (genitive)—a circumstance which ET comment on (225) but do not explain.

²⁷ In some instances, however, the two are interchangeable, as *ici on parle français* and *le français se parle ici*.

²⁸ The fact that *je suis aimé* comes, as ET remind us (235), not from *amor* but from *amatus sum*, should no more affect our thinking on earlier Latin tense usage than the fact that the French imperfect subjunctive comes from the Latin pluperfect. The periphrasis did of course eventually lose its perfect force, and the participle become a mere tenseless adjective, as it had indeed been originally. At all stages of the language we find examples of passive participles that seem to have become ordinary adjectives, such as *dilectus* 'dear' (which *amatus* resembles) and *paratus* 'ready'; and of periphrases that it is easier to analyze

as 'est enfermé' (or 'is enclosed') and of *circumdati sumus* as 'sommés entourés' (or 'are surrounded'), does not alter the fact that the mountains continually enclose the town,²⁹ whereas the enemy have completed the action of surrounding us; we could say 'we have been surrounded by the enemy' but not 'the town has been enclosed by the mountains'. As for *ab hoste circumdamur*, that might require some such rendering as 'nous sommes en train d'être entourés', 'we are being surrounded'.

Still more unfortunate is the habit of making French translation the basis of an explanation or the criterion of arrangement. Sometimes *nam* 'ne se traduit pas' (380);³⁰ *neque* is sometimes equivalent to *sans* (131), and *ut non* to *sans que* or *sans* (293); contrariwise, a special note (224 fn. 1) sums up the various 'tours' for translating French *sans* and the infinitive (why not English *without* and the gerund?).³¹ In Livy 3.28.1 *imperavit ut sarcinas conici iubeant*, the verb *iubeant* is said to be in the present 'sans doute parce qu'il n'a que la valeur d'un auxiliaire "il donna l'ordre de faire rassembler les bagages"'. L'indication temporelle n'a qu'un intérêt secondaire; en français, l'infinitif *faire* dans "faire rassembler" n'en apporte aucune' (355). If Livy had written *iussit sarcinas conici iuberi*, the infinitive *iuberi* would of course have been in the present, because the infinitive does not vary for sequence. But the subjunctive does normally vary for sequence; Livy's departure therefrom is probably an instance of 'repraesentatio', particularly common in this author.³² Even granting that *iubeo* might be called an auxiliary, we must remember that finite forms of auxiliaries vary in tense like any other verbs; and the accident that to translate this particular 'auxiliary' in the subjunctive we use a French 'auxiliary' in the infinitive cannot explain the tense of the Latin subjunctive.³³

The approach to Latin via another language is also evident in the general

as a combination of *sum* and a predicate adjective than as genuine passives, such as Plautus, *Stich.* 680 *cena cocta ut esset*, and Livy 1.19.3 (Ianus) *clausus fuit*. ET call *cocta esset* a pluperfect (350), but really *cocta* resembles *parata* (cf. inf., fn. 145); and they call *clausus fuit* a 'perfect in the past' (194), but *clausus* is used exactly like the adjectives *apertus* and *clausus* in the immediately preceding clause. However, in all these instances, with the possible exception of *dilectus*, the participle, though without its full past force, does keep some notion of the result of an earlier process.

²⁹ Caesar's *Gallia est divisa* is different, for *divido* is dynamic and *contineo* static; the action of dividing happened once and for all, and is not in constant continuance like the containment by the mountains, so that *Gallia dividitur* would be impossible here, and *Gallia est divisa* represents not action but the result of action, whether we call it the perfect passive of *divido* or the present of *sum* plus an adjective (on this cf. fn. 28).

³⁰ In the example given (*Tusc.* 3.56) *nam*, as often, gives the reason not for the previous statement but for the making of it (somewhat as does *ut* or *ne* in the 'absolute' purpose clause, or *si* in the illogical conditional clause; cf. fnn. 65 and 111); it could certainly be translated by English *for*, and I think also by French *car*.

³¹ Notes like these assuredly have their uses, but they belong in a text-book on 'thème' or 'version', not in a manual of syntax.

³² Cf. R. S. Conway's edition of Livy 2 187-97 (Cambridge, 1902). See further, on this stylistic feature in general, fn. 2 above.

³³ Even in French, the subjunctive would be in the imperfect, despite the unpopularity of this tense: surely we would say *il ordonna qu'on fit assembler les bagages* rather than *qu'on fasse*.

organization of the book. Thus the treatment of *si* clauses is completed by two pages (331–2) dealing with half a dozen other conjunctions because ‘l’usage est de rattacher aux conditionnelles des constructions ayant avec elles une similitude de sens, quoique d’origine différente’. Whose ‘usage’? Surely *dum* proviso clauses should be associated with other *dum* clauses, and *ut* ‘à supposer’ clauses, even in a classification based on meaning, with concessive clauses;³⁴ while *absque* clauses do not belong with subordinate clauses at all, for they never developed beyond the original paratactic stage, but died out instead.³⁵

Classification by meaning is the basis of the entire treatment of ‘circumstantial’ subordinate clauses. Though I do not believe that consideration of meaning can or should be ruled out of descriptive grammar, I do believe that it is more satisfactory to classify such clauses in accordance with the introductory word, *quod*, *cum*, *ut*, *si*, etc. (as Hofmann does), than in accordance with their function, final, consecutive, causal, etc. (as ET do). It is impossible in any case to be consistent in such a treatment. Thus under the general heading of concessive clauses (297–300) we have: (1) properly grouped together *-si* conjunctions, including *etsi*, *etiamsi*, etc.; (2) *quamquam*, *quamvis*, *licet*; (3) *cum*. At the close of group 1, we have examples of simple *si* (with which I would have begun the category) and also of *ut*, which seems alien, ‘granted that’ rather than ‘(even) if’.³⁶ In group 2, *quamvis* belongs with *quamquam* in form and with *licet* in mood governed, but *quamquam* has little in common with *licet*; it is much closer in use to *etsi* in group 1. As for *cum*, its concessive force springs purely from the context, usually emphasized by *tamen* in the main clause; it seems highly undesirable to separate *cum* ‘concessive’ from *cum* ‘causal’,³⁷ or either from *cum* circumstantial; and *cum* circumstantial should be treated with *cum* temporal, from which

³⁴ They are indeed noted there too (298), but in a section devoted to *-si* clauses, where they do not belong. Cf. fn. 36.

³⁵ *Absque* was a combination of the preposition *abs* ‘apart from’ (as is proved by the invariable presence of an ablative beside it) and the enclitic *-que*, which so far as I know has not been explained. I suggest that it served originally to mark the coordination of the two unreal clauses, as I assume must also have been the original force of *et*, *etiam*, and *ac* in *etsi*, *etiamsi*, and *simul ac*, when the protases so introduced were coordinate with (not, as later, subordinate to) the following apodosis. As the force of the *-que* was lost sight of, the entire composite came to be used as a preposition, *absque me* ‘apart from me, without me, were it not for me’. This preposition ET properly list elsewhere (97), but again they classify by meaning, classing it with its synonym *sine* rather than with its primitive *ab(s)*.

³⁶ The subjunctive with *si* and *etsi* is potential; with *ut*, volitive. Even in a purely descriptive grammar, cognizance must be taken of the historical difference, for it involves the negative—*non* with the first group, *ne* with the second at least as a rule (cf. Hofmann 764). To be sure, ET (331) state categorically that the negative with *ut* ‘à supposer que’ is *non*, but their examples hardly bear them out, for the conjunction in them is really affirmative *ut*: in *Tusc.* 1.16, *non* belongs with a single word, the verb *efficias*, contrasted with the affirmative *efficies* immediately following; and in *Mil.* 46 we have a stereotyped phrase after *ut*, *neminem alium nisi*. Cf. too *Att.* 2.15.2 and 8.12c.1; and see further Blase, *Glotta* 11.159. Similarly in *Att.* 11.21.1 *non potuisti facere ut epistulam non mitteres*, which elsewhere (260) ET class with examples of ‘chevauchements d’emploi’ because of the use of *non* ‘malgré la nuance d’intention’, *non* belongs closely with *mitteres*; note the immediately following clause, not quoted by ET, *tamen mallet non esse missam*.

³⁷ Cf. the double force of English *whereas*.

it is derived. This leads me to another unfortunate feature of the ET classification: in treating causal and concessive clauses with *cum* before temporal clauses,³⁸ adversative clauses with *-si* before conditional clauses, etc., it ignores the factor of historical semantic development.

Meaning is also the basis of the numerous lists of 'concurrences' or 'confusions' of two different constructions. Sometimes there is little or no distinction in meaning between the two 'competing' or 'confused' types of expression: so in the chapter on the genitive we find listed various synonymous ablatives (42, 44, 45, 52),³⁹ and in the chapter on the dative we find the predicate nominative listed with the dative of purpose (66). Sometimes there is a real difference in meaning between the variant forms, as dative vs. accusative with *caveo* (58) and dative vs. ablative of separation (60-1),⁴⁰ listed in the chapter on the dative, and dative vs. ablative with *fido* (79), listed in the chapter on the ablative.

These juxtapositions are often highly convenient and genuinely illuminating, more so than would be the isolated treatment of the two rival constructions each in its proper place, with a bare cross reference to the other;⁴¹ as I believe that methodological technique should be a servant and not a master, I am not raising serious objection. But I do protest against the chronicling of a supposed 'concurrence' of two usages that are actually quite different. This is done particularly often with reference to the genitive and the dative. It is twice said (8 and 63) that the dative of the possessor alternates with the genitive of possession, whereas actually the former indicates what someone possesses and the latter indicates who possesses something.⁴² In *Capt.* 174, *mi* is said to be equivalent to

³⁸ Cf. too the reference (297) to *postquam* in the sense of its French derivative *puisque* (a meaning that I agree it has in *Phorm.* 1, though perhaps not in *Most.* 647). But *postquam*, like English *since*, was certainly temporal before it was causal; and so I think it would be better to present it first in its meaning of 'after'.

³⁹ The reader when he reaches the corresponding section in the chapter on the ablative is then referred back to the chapter on the genitive (74 et al.). An alternative and more logical method is the handling of such 'concurrences' in a separate section: thus in Chapter 8, on nominal forms of the verb, one section deals with the infinitive (215-20), another with the gerund (221-6), and a third with the 'concurrences' of infinitive and gerund. A particularly valuable section (94-7) on confusions of accusative and ablative is separated from both the chapter on the accusative and that on the ablative; but Chapter 6, in which it occurs, dealing in large part with spatial and temporal relations, overlaps the earlier chapters on accusative and ablative.

⁴⁰ *Capiti* in *Ecl.* 6.16 (61), if a dative, must be a real instance of confusion with the ablative; but I prefer the alternative suggestion by ET themselves, that it is an ablative. On the other hand, the use in late Latin of the dative in place of *ab* and the ablative with verbs of asking seems to me due to the analogy of the use with *dico*.

⁴¹ This practice too is sometimes followed by ET; thus under '*ut* complétif' we find *iubeo* and *impero* listed with a statement that with *iubeo* the infinitive is usual (256); and under the infinitive with a verb of '*volonté*' we find them both again, with a statement that with *impero* an *ut* clause is common (278). But even in these cases, the treatment is not wholly consistent: the use of *iubeo* plus a dative plus an infinitive certainly does not belong where it is placed (256) under examples of *ut* clauses. (The particular example there cited, Catullus 64.140, is far from typical; the dative *miserae* here may have been induced by *mihi* in the preceding clause.)

⁴² In the second instance (63) ET show that they are aware of the difference. Why then introduce the genitive at all in a chapter on the dative?

meus (63); but *mi est natalis dies* means 'I have a birthday (today)', while *meus est natalis dies* would imply that someone has a birthday and I am that someone.⁴³ Comparison is made (ib.) of *sese Caesari ad pedes proiecerunt* and *cum se ad Caesaris pedes abiēcisset*, whereas *Caesari* is dative of reference (not dative of possession as ET call it) with *proiecerunt*, and *Caesaris* genitive of possession with *pedes*.⁴⁴ As a parallel for the dative gerundive construction in *decemviri legibus scribundis* is offered the genitive in *curatores monumentorum tuendorum* (67); but the presence of the verbal noun *curatores* would probably account for the use of an objective genitive as opposed to the dative of purpose with *decemviri*.

I now give some miscellaneous comments, mainly following the order of the book.⁴⁵

Nouns are classified as substantives, pronouns, and adjectives (1). I would recognize two classes: formally, noun-adjective and pronoun; functionally, substantive (including noun and pronoun) and adjective. — *Flectere*, seen in French *flexion*, said to mean 'change' (7), is rather 'bend', as *clino*, seen in French *déclinaison*, is 'slant'. The ancient conception of the paradigm is that of a main form with other forms deviating from it.⁴⁶ — Reduction in cases is a feature of the development of Indo-European languages (9); but the eight cases of Indo-European seem an extension of an earlier, smaller group. Hittite suggests the original identity of dative and locative, and of ablative and instrumental.⁴⁷

Benedicens nos episcopus, projecti sumus (12) is a true example of the nominative absolute (though most people would call it an anacoluthon), but *hi contemnentes eum, assurgere ei nemo voluit* might be explained as partitive apposition.⁴⁸ So too *decem legati quisque discesserunt*, though ET call this different (ib.), and the passages of early Latin cited as examples of loose agreement (107–8). — *Cynthia* in Propertius 1.18.31, called a nominative (12), is probably a vocative (cf. Vergil, *Ecl.* 6.44).

The accusatives in *vivere vitam* and *id gaudeo* are not objects because the verbs are intransitive (15).⁴⁹ Circular reasoning? Why not say that with an 'inner

⁴³ Had *hic* been used, we might have expected the possessive: *hic (dies) est natalis meus* 'this day is my birthday'.

⁴⁴ Perhaps *proicio*, a compound with *pro*, is somehow more likely to take a dative than *abicio*, a compound with *ab*. However, we do meet the dative with *me abicio* (e.g. *Att.* 8.9.1) as also the genitive with *me proicio* (e.g. *Sest.* 26). None the less, the two constructions should not be viewed as completely interchangeable.

⁴⁵ The statements preceding page references in parentheses are quotations or paraphrases of material in ET; those following them are my answers or objections. Separate comments on the same general subjects are combined in a single paragraph but divided from one another by dashes.

⁴⁶ See Schwyzler, *Griechische Grammatik* 1.416 fn. 1, 2.53 and 302; Hahn, *TAPA* 82.32 fn. 9.

⁴⁷ Hittite must represent an earlier not a later stage; the Indo-European ablative and instrumental are in Hittite sandhi variants (Sturtevant, *Lg.* 8.1–10).

⁴⁸ ET recognize instances of this widespread phenomenon in the accusative (32–3) only.

⁴⁹ These accusatives are called respectively 'accusatif de qualification' (21) and 'accusatif

object' the verbs are transitive? — *Me miseret* is translated 'il y a en ce qui me concerne pitié' (17). Rather 'it causes me to feel pity'?

The five deponents (*fruor* etc.) take the accusative in early Latin (17). Probably *utor* less freely than the others; the ablative may have started with it. *Fungor* is classed with verbs of privation (72), and the ablative with it is classed as separation (17); but I think this ablative was borrowed from that with *fruor*,⁵⁰ which *fungor* resembles in form and sometimes in meaning (cf. *Most.* 48 *fungi fortunas meas*), and thus expresses means as do the ablatives with the other four verbs. — *Nuptum* in *nuptum dare* (27) should be classed with supines. — The translation of *Bell. Civ.* 1.87.3 'P. et A. se voyant réclamer la solde par les légions' (31), which implies that the words *Petreius atque Afranius* serve as subject of a subsequent verb, should rather be 'Quand la solde fut réclamée à P. et à A.'.

(35) Is *communis* the 'contraire' of an adjective expressing possession? — The genitive to express point of departure helps us understand the replacement of the ablative by the genitive in Greek (36-7). Was not this rather due to identity of the two cases in form in many instances? — (40) The genitive does function as an adjective in *virgines Vestae* (= *virgines Vestales*), but not in *scelus viri* (= *vir scelestus*, not *scelus virile*) or *monstrum mulieris* (= *mulier monstrosa* rather than *monstrum muliebre*).⁵¹ With *scelus viri* I would compare *strata viarum*, which ET class rather with *prima virorum* (43). — *Quorum utrique* is contrasted with *consul uterque* (42). Since the relative naturally agrees with its compound antecedent, *cui utrique* would have been impossible. — (46) The genitive of price (*magni, pluris*) is scarcely a genitive of quality unless we assume an ellipsis (hardly a happy solution) of a noun such as *preti*. — (48-9) *Animi* is perhaps a locative, not a genitive; the parallel use of *mentis* may be due to misunderstanding. — (52) *Morti* in Lucretius' *morti damnatus* may be an old ablative rather than a dative.⁵² — (53) In *ad Dianae* etc. (like English *to St. Paul's*) we need not see an ellipsis;⁵³ *Dianae* is a genitive which has become an indeclinable used in any case, like dative *frugi*, or like the locution *a.d. III Kal. Mart.* etc.

Plenus is rendered 'plein de' (44), *aptus* and *accommodatus* 'adapté à' and *idoneus* 'propre à' (60); delete the prepositions.

(57) I would hesitate to speak of 'une forme mixte de dat.-ablatif'; an

de relation' (23), but this is a mere matter of terminology. Their behavior in the passive indicates that they are direct objects. For the first type cf. *Pseud.* 524 *pugnam pugnabo* (cited by ET 21) and *Men.* 989 *depugnato proelio* (cited 22); to explain the latter as formed 'par analogie avec l'accusatif d'objet' (22) seems to me a begging of the question and quite unnecessary. The second type is said to remain accusative in the passive (23); some examples cited for it are adverbial expressions, which I agree with ET are not direct objects, as *Rosc. Am.* 8 *hoc solum pugnatur*, but in *Rab. Post.* 17 *ego hoc cogor*, surely *hoc* is a retained object (the active would be *me hoc cogunt*).

⁵⁰ Of the five, *fungor* alone seems never to have the ablative in early Latin.

⁵¹ Though ET (also Hofmann 391) class these two together, the fact that *scelus* is abstract and *monstrum* concrete makes a difference. A *mulier* really is (or at least can be) a *monstrum*; but a *vir* is not a *scelus*.

⁵² Cf. fn. 40.

⁵³ Humbert, in his treatment of the comparable Greek construction (300), is in this case superior to ET.

author using an ambiguous form like *mendaciis*, if asked to substitute for it a noun with different forms for dative and ablative, doubtless would be ready to commit himself to one case or the other.⁵⁴ — In *Ep.* 294 *illum conveniam atque adducam*, one would rather expect the dative (59); here the accusative may be due in part to the influence of *adducam*, but Plautus regularly uses the accusative with *convenio* 'meet'. — The poetic employment of the dative for limit of motion, called an 'extension toute factice' (60), is rather a vestige of the original use. — (66) *Pabulum ovibus* (dative of reference) does not belong with *satui semen* (dative of purpose).

(68, 73, 74) I have never understood why the ablative with *cum* expressing accompaniment should be classed as a type of instrumental, except for the accident that the same preposition (*avec*, *with*, *mit*) is used to translate both. In Latin the two constructions are sharply differentiated, since means never has a preposition.⁵⁵ — *De* prevailed over *ab* and *ex* because of its initial consonant (69); yet *ad* had no difficulty in surviving (to be sure it had no rival beginning with a consonant). — As an example of the ablative, *Lem.* (71) in *Phil.* 9.15 should be written out *Lemonia*, as indeed it is in the Oxford and Teubner texts. — ET see no difference between accusative and ablative with *super* (84), but I think motion (followed by rest) is implied in *super eam* [scil. *aspidem*] *adsidere* (ib.), as also in *in equum Troianum includis* as opposed to *in cella incluserit* (87), since shutting persons up inside the horse implies the difficulty of first getting them INTO the monster's interior.⁵⁶ — Ablative phrases containing *totus*, as *toto oppido* and *tota Asia* (87), *tota vita* and *tota nocte* (95), classed as means, to me seem local or temporal; the presence of *totus* accounts for the absence of a preposition. — *Re* in *refert*, called an ablative of accordance (88 and 155), seems to me rather one of specification.

The ablative absolute is pronounced 'une construction de valeur mixte', connected with various types of ablative, and an elaborate outline enumerates the types of words used—names of magistrates, agent-nouns, adjectives, participles, etc. (88). Such details are interesting but unnecessary. The ablative absolute is capable of exemplifying any type of ablative and any type of accompanying word, but peculiar in that it involves predication. The predicate ablative can be a noun or an adjective (cf. the nominal sentence) or a participle (cf. the verbal sentence). Such predication normally involves two terms, one corre-

⁵⁴ Similarly ET speak elsewhere (334) of a 'forme mixte' of future perfect indicative and perfect subjunctive. Cf. below, fn. 113.

⁵⁵ The so-called ablative of accompaniment without a preposition in military expressions doubtless arose as an ablative of means, since to a general his troops were instruments rather than companions. Perhaps the use with *certior factus* of *per exploratores* (as in *Bell. Gall.* 1.12.2) rather than the agent expression *ab exploratoribus* (as ib. 1.21.1) stems from the same attitude. The type probably began with a pure expression of means, as *militibus murum perducere* (cf. 1.8.1); an ambiguous expression like *copiis dimicare* (5.49.6) or *ingenti exercitu impetum facere* (Livy 1.23.3) would furnish the transition to a genuine instance of accompaniment, as *pedestribus copiis contendere* (*Bell. Gall.* 3.11.5).

⁵⁶ Likewise, despite ET (96), I see motion in *ostende huc manus*, which involves the suspect's stretching out his hands toward the interlocutor. (So too in *ad* 'in the neighborhood of'; on this cf. fn. 24.)

sponding to the subject and the other to the predicate;⁵⁷ but an impersonal verb, just as it needs no subject, may stand alone as an ablative participle (e.g. *pugnato, consulto*) without an accompanying substantive. However, in the example cited by ET (89) as illustrating an impersonal participle with a clause depending on it, *cognito vivere Ptolemaeum*, surely the clause corresponds to the subject of a passive verb.⁵⁸

The accusative in *abhinc triennium profectus est* answers the question *ex quo tempore?* (91; so too 26) and the ablative in *hoc triduo sciemus* answers the question *quando?* (91). Rather *quamdiu abhinc?* in both instances. In *Troiam decem annis ceperunt*, *annis*, called ablative of means (91), is rather ablative of time within which. — (100) *Re in refert* might have been added to the 'improper prepositions' (*causa, tenus*, etc.).

Od. 6.155-7, given as an example of loose agreement (107), is rather one of a group of passages that seem to mark the evolution of the Greek genitive absolute. — (108) The quotation *Aen.* 9.427 *me, me adsum qui feci*, is misleading to one who does not know the passage; it would be better to omit the accusatives *me, me* (which are not involved in the grammatical point illustrated) or to insert the subsequent *in me convertite ferrum* which justifies them. — The use of a singular participle with a plural substantive in the ablative absolute is archaic (109-10). In the examples here cited for *praesente* and *absente* with *nobis*, we may have agreement by sense, since *nobis* in them stands for *me*; but in some other instances the participles seem to approach prepositions. However, *auctore Metello et Mucio*, compared with *absente nobis* (111), is really different, for it corresponds to the use of a singular verb with a compound subject. — The agreement with the appositive in *Corinthum, Graeciae lumen, extinctum* (113) is probably facilitated by the figure of speech involved in *lumen extinctum*; *Corinthum extinctam* would be unlikely. — In *nec plus quam quattuor milia hominum effugerunt* (113) we have agreement by sense; *plus* is almost equivalent to *plures*; moreover, the whole phrase is synonymous with *quattuor milia sola hominum*.

The so-called 'attraction of the antecedent into the case of the relative' (117) is shown by Hittite to represent the original state of affairs, and to involve no attraction at all.⁵⁹ The only difference between *Tusc.* 1.41 *quam quisque norit artem in hac se exerceat* and *Aen.* 1.573 *urbem quam statuo vestra est* is one of word order, *quam artem* vs. *urbem quam*. Nor need we view as an 'extreme case' *Agr.* 34.2 *in creta et uligine et rubrica et ager qui aquosus erit, semen serito*; the relative clause *ager qui aquosus erit* is parallel to the ablatives *uligine* and *rubrica* (this is facilitated by the failure to repeat *in* with these ablatives). The only examples given by ET which represent a shift from the original construction are *Truc.* 746 *illis quibus invidetur, i rem habent*, and Petronius 134.8 *hunc adolescentem quem vides, malo astro natus est*; a demonstrative in the second clause is normal,

⁵⁷ A finite verb without a noun subject may stand alone, since the subject is sufficiently implied by the termination of the verb; but the participle of course cannot provide that, so *regno* and *regnat* become *me regnante* and *eo regnante*.

⁵⁸ Just as in *turpe mentiri est*, discussed above, I would, in opposition to ET, call *mentiri* the subject of *est*.

⁵⁹ Sturtevant, *Curme* volume 141-9 (Language monograph No. 7, 1930); and Hahn, *Lg.* 22.68-85.

but in the first clause represents a secondary development.⁶⁰ — (119–22) As examples of agreement by sense, we can easily understand *mea Glycerium* (proper nouns are often exempt from common rules) and Cato, *Agr.* 81 *eadem omnia indito; id permisceto* (ALL the ingredients, *omnia*, are put into the dish separately, and then the mass as a unit, *id*, is mixed).

Is the impersonal passive (124) really 'en voie de disparition'? The type *pugnatum est* continues common. — The rare impersonal use of a third singular *velit* in *De Or.* 1.30 is perhaps induced by the infinitive *posse*; an active infinitive without a subject automatically becomes impersonal. — The present participle in the dative is used in an impersonal sense (125); this is true of any participle in any case,⁶¹ so long as it does not refer to a particular noun⁶² (like the infinitive without a subject).

(126) There are many other 'attributive' verbs, e.g. *sto*⁶³ (which gave some forms of 'be' in Romance) and *consisto*.⁶⁴ — (127–9) On word order add: (1) sentence connection is often a determining factor; (2) any departure from normal word order gives emphasis. — That in ancient times the genitive regularly preceded the noun modified (128–9) is contra-indicated by the archaic *pater familias*. — Vergil exemplified the 'disjunctions' of nouns and adjectives (129); add Horace, whose *callida iunctura* in this respect (e.g. *Carm.* 1.5) was outstanding.

(132) In *Amph.* 330 *ne* need not be equivalent to *nedum*; we may have a prohibition, or an 'absolute' purpose clause,⁶⁵ as in Horace, *Ep.* 1.7.82–3 *ne te morer*.

(141) In Catullus 68.46 the use of *anus* and *charta* probably involves personification; note *loquatur*. — 'Le plus haut degré possible se marque par *quam* avec une forme de *possum* exprimée ou non' (147). If *possum* is not expressed, is *quam* really used 'with' it?

(152) In *Sest.* 49 *semel* before *iterum* might have its regular force 'once'; a surer example for its use instead of *primum* is Livy 1.19.3. — In Lucretius 4.39 and *Fin.* 5.37 *nostri* and *sui* are used in their original neuter sense (154); why not as genitives? (So too Horace, *Carm.* 3.30.6 and Ovid, *Am.* 1.15.42). — (155) I would discuss *refert* before *interest*, since it set the model that its synonym *interest* followed. — In *Romanis multitudo sua auxit animum*, the reflexive is due to an implied *cum multitudinem suam respicerent* (156); it would be simpler to equate the sentence with *Romani multitudine sua animum auctum habuerunt* or, more naturally, *animo aucti sunt*. — The middle voice expresses reciprocity in *Aul.* 116 *copulantur dexteras* (158). No, for the meaning is not 'ils se serrent la main', but, as ET themselves translate, 'ils me serrent la main' ('me' is implied by the context). — *Is* disappeared from poetry for metrical reasons (161). What metrical reasons? Actually, *is* was not a poetic word; the poets preferred the more vivid

⁶⁰ Hahn, *Lg.* 22.74–81, 25.356–7, 359–61.

⁶¹ Mainly in the oblique cases, but the nominative is not impossible. See Hofmann 457.

⁶² Cf. the use of a participle with an article in Greek, or with a demonstrative in English.

⁶³ As in Lucretius 2.181, Vergil, *Aen.* 6.300, 6.313 (*stant orantes* here is a vivid substitute for *orabant*). Cf. Merrill on Lucretius 1.564.

⁶⁴ Cf. Merrill on Lucretius 1.168.

⁶⁵ *Nam* clauses, as already noted (fn. 30), may be similarly used; so, too, *si* clauses, e.g. Vergil, *Ecl.* 3.23 *si nescis, meus ille caper fuit* (cf. fn. 111).

ille. — In *cum Lycurgo nostras leges conferre*, the ellipsis is avoided by transposition (163). The order of mentioning Lycurgus (and his laws) vs. (us and) our laws is immaterial; neither is there an ellipsis, there is simply a lack of strict logic ('compendiary comparison'). — *Illud* with a word quoted as a word suggests the ultimate appearance of the article (163); surely *illud*, like Greek *tó*, is used here not like the article that developed in Romance but as a means of providing an indeclinable form with case.⁶⁶ (164) In *Trin.* 493 *aequo mendicus atque ille opulentissimus*, if *ille* were an article it would be needed with *mendicus* too; it is used here either as 'that famous' (perhaps with a touch of irony, since the rich man will be no better off than the beggar in Acheron) or, more probably, as a true demonstrative, referring back to *opulentus* in 469. — Among interrogatives followed by *quis* is listed *uter* (164); slip for *utrum*? — (166) I do not understand the assignment to *nonnullus* of 'valeur de négation partielle'.

Latin 'ne connaît plus que l'opposition actif/passif' (171); but in some medio-passives the passive has a middle force (ib.). I would prefer to classify Latin verbs as active and medio-passive (a much better name than passive), saying that medio-passive forms have for the most part passive meaning, but occasionally this has become intransitive (*videor* = 'I seem' rather than 'I am seen'), and some vary between middle and passive meaning (*vertor* = 'I turn myself' or 'I am turned' by some outside agency). I would add the following details. Some active verbs have no corresponding passive forms; they may use other active verbs to supply this lack, so that the latter seem to have passive meanings (*facio fio*, *perdo pereo*, *vendo veneo*, *verbero vapulo*). Some medio-passive verbs, the so-called deponents, have no corresponding active forms; of these a number seem themselves to have active meanings (*sequor*), while others seem rather to have middle meanings (*vescor*). There is some shifting between the two voices so far as meaning goes: the active *verto* used with a reflexive or intransitively may be synonymous with the medio-passive *vertor* used medially. — *Licere*, defined as 'mettre en vente' (173), is rather 'bid' or 'appraise' (cf. *polliceri* 'make an offer, promise'). — Latin has no sure example of the type **hanc rem paratur* (174). One thinks instantly of Ennius, *Scen.* 241 (Vahlen) *vitam vivitur*. To find the (supposed) justification for its exclusion, one must turn to Ernout's article in *MSL* 15,⁶⁷ where (291) he explains that *vitam* is an accusative 'de l'objet in-

⁶⁶ So, too, *tó* with an infinitive, and, rarely, *illud*, as in Pliny, *Epis.* 8.9.1 *illud iucundum nihil agere*.

⁶⁷ There too (290) he makes the statement that in Latin 'il n'y a pas ... d'exemple sûr d'indicatif impersonnel accompagné d'un pronom régime', but then oddly he proceeds to contradict himself by showing that there are a few examples: 'on ne peut guère citer qu'un exemple d'Atta, 7 (*Rib. Com.*) ... et un autre de Plaute, *Cas.* 185 ... De même, dans quelques endroits de Plaute, on trouve l'impersonnel suivi de l'accusatif: *M.G.* 24 ... *M.G.* 254.' Atta's *te veretur* is not an apposite example, for *veretur* is a transitive deponent (here used impersonally like *pudet*); and, since *mentior* is also a deponent in Plautus, *Mil.* 254 *quae mentibitur* would have to mean 'the lies which he will tell' rather than 'the lies which will be told' (however, *mentibimur* seems to me the better reading). But in *Cas.* 185 *me despiciatur* we have a true passive with an object (cf. directly after it 189 *vir me habet despiciatam*); so too *Mil.* 24 *epityra estur*. Lindsay (*Syntax of Plautus* 53) in addition to the three examples from Plautus just cited, quotes two others, *Pseud.* 817 *teritur sinapis scelera* and 1261 *mammia mammicula opprimitur*; but Ernout seems right in dismissing them, the

térieur', and cites Riemann and Goelzer as his authority.⁶⁸ Obviously neither Riemann and Goelzer, nor Ernout and Thomas, would call *vitam* in *vitam vivo* (as in Plautus, *Merc.* 473 *vitam cupio vivere*) a direct object either;⁶⁹ hence the whole question hinges around a mere matter of nomenclature. — The function of the passive is often 'de mettre en relief la notion verbale plutôt que l'agent', as in *Phorm.* 854 *ab dis solus diligere* (176); I see no fundamental difference between the passive here and the active elsewhere in the same author, *And.* 973 *solus es quem diligant di*.

Throughout the treatment of tenses (e.g. 183) ET follow Meillet's system of recognizing three 'temps d'*infectum*' and three 'temps de *perfectum*', though they recognize (206) that the present subjunctive is not always an *infectum* or the perfect subjunctive a *perfectum*. I believe we need not two categories but three, durative, punctual, and perfective, with the realization that three of the tenses (present, future, and perfect) serve for two different aspects, and one (perfect) for two different periods of time.⁷⁰ — The treatment of the force of preverbs in aspects is interesting but at times inconsistent: *con* (and *ob*) served chiefly to mark 'l'aspect déterminé' (185), but *con* (and *per* and *in*) did not necessarily give 'l'aspect déterminé' (186); in the case of verbs with an inchoative *infectum* (in *-sco*), preverbs often stressed the sense of the *perfectum* (186), but *ob* and *ad* reinforced that of the *infectum* (ib.).

I of course agree that the future can have the force of the subjunctive in early Latin (192),⁷¹ but I do not agree that in the formula *ibo intro*, common in comedy, *ibo* is used in place of a subjunctive (195) and alternates with *eamus*. One naturally says 'let us go' of group action, but boldly and positively 'I will go' of one's own. There are passages in which either 'I will go' or 'let me go' would fit the context, but in many instances only the former is possible.⁷² — In a prohibition the perfect subjunctive is not a *perfectum* (197); an explicit remainder that here we have a clear trace of aspect might well have been added (cf. inf., fn. 81).

The tendency of *non* to appear instead of *ne* in prohibitions, particularly common in the imperial period (130), is attributed to 'l'idée latente de potentiel'

former as doubtful (*scelera* is probably an adjective) and the latter as textually uncertain. However, unless we insist that *Cas.* 185 is spurious and that *Mil.* 24 must be emended, Hofmann (380) probably goes too far in ruling out all Lindsay's examples.

⁶⁸ O. Riemann and H. Goelzer, *Grammaire comparée du grec et du latin* 2.245. Their statement runs as follows 'Les verbes passifs ne doivent pas [!] avoir de complément signifiant l'objet direct et immédiat de l'action. Mais ils peuvent avoir un complément direct qualificatif de l'action.'

⁶⁹ On this construction see above, fn. 49.

⁷⁰ See *SO* 2 fn. 5.

⁷¹ So, too, the subjunctive can have the force of the future. See on both alternations *SO* 123-9.

⁷² Sometimes the tone is peremptory: a god speaks (*Amph.* 1007, 1039) or an unamiable man (Euclio in *Aul.* 659, Ballio in *Pseud.* 903). Sometimes *ibo* is parallel with another first person indicative, future (*Aul.* 620, *Pseud.* 1245, *Rud.* 1263, *Stich.* 567) or future perfect (*Capt.* 192); once with a third person future, which certainly would not replace a subjunctive (*Ep.* 319). In *Most.* 848-9 *ilicet* with its full etymological force 'you have leave, go in' is followed by *ibo intro igitur*, certainly 'I'll go in then', not 'let me go in then'.

(197). The subjunctive in prohibitions seems to me clearly volitive, with no hint of potentiality; but in early Latin great confusion of negatives prevailed,⁷³ and in late Latin *non* tended everywhere to replace *ne*, which left no trace in Romance. — The 'subjunctif de possibilité' combines the uses of the 'optatif de possibilité' and the 'subjunctif éventuel' (200; cf. 195); directly afterwards the 'potentiel éventuel' is defined as the 'subjunctif de possibilité lié à une condition' (ib.). Apparently then there is no difference between 'éventualité' and 'possibilité' in Latin. But if there is not, what was the difference in Indo-European? Would it not be simpler and clearer to assume that functionally as well as formally⁷⁴ the Indo-European 'subjunctif d'éventualité' survives in the Latin future, and the 'optatif de possibilité' in the Latin potential subjunctive? I also question the insertion of the phrase 'lié à une condition', even though this is slightly qualified thereafter. Whether the potential subjunctive is combined with a conditional clause or not is purely fortuitous. — The special use of *velim* 'je voudrais' may have contributed to the retention of the optative form (201). Possibly; yet what of *sim*, and what of the subjunctive of the first conjugation if Bréal and Meillet are right (as I believe they are⁷⁵) in regarding this too as an optative? — Originally there was no special expression for unreality (202). I think there was: the one preserved in Hittite and Greek, past indicative with a modal particle.⁷⁶ — In the unreal condition, *dicerem* 'je dirais' loses its past value (202 and 320; cf. 206). Rather *dicerem*, precisely like *dirais*, represents a future thrown into the past, 'I was on the point of saying', with implications that one got no further. — I do not agree that the indicative is 'très voisin de l'irréel' (208). In the unreal conditions cited (208–10) the notion of unreality is supplied not by the mood of the verb but by other factors: the adverb *paene*; the imperfect tense implying incompleteness (*trudebantur* 'they were in the process of being pushed' but the act was not finished); a modal verb such as *possum* or *debeo*, which itself has in the indicative the force of most verbs in the subjunctive. On the other hand, there is no unreality at all about such expressions as *aequum est*, *melius est*; the state of being right etc. is a true state, and it is only our idiom ('il serait juste', 'it would be better') that leads us to expect the subjunctive. Again to equate *non putavi* with *je n'aurais pas cru*, or *quis arbitratus est?* with *qui eût pensé?*, is a mere matter of translation; they could have been rendered 'je n'ai pas cru', 'qui a pensé?'.⁷⁷

Of the subjunctive we read: 'Il ne possède pas de futur, ... et il a fourni lui-

⁷³ For examples, see Bennett, *Syntax of early Latin* 1.167–73, and cf. 364–5. For the reason for such confusion, see *SO* 52–8. For the use of *non* rather than *ne* with the prohibitive subjunctive in particular (with which we may compare the use of *na* with the optative in Sanskrit), see *SO* 41 and 55, *Lg.* 29.252–3 and particularly 253 fn. 43.

⁷⁴ All Latin futures come from Indo-European subjunctives (cf. fn. 82), and I question whether any Latin subjunctives do (cf. fnn. 75 and 78, and see further *SO* 60 fn. 113, 71–3, 150).

⁷⁵ Bréal, *MSL* 6.411; Meillet-Vendryes, *Traité de grammaire comparée des langues classiques* 278 (cf. sup., fn. 74). The question is not raised in ET; elsewhere, Ernout (*Morphologie*⁹ 161) apparently treats this form as a subjunctive in origin, but Thomas (*Recherches* 1 fn. 1) envisages the possibility of its being an optative.

⁷⁶ See *SO* 54–7.

⁷⁷ Cf. their comment (276) on the lack of a special expression for the conditional in the

même le futur à la 3^e et à la 4^e conjuguons' (184). This confuses two altogether disparate entities: the first *il* refers to the Latin subjunctive, and the second to the Indo-European subjunctive.

The same confusion appears in the discussion of the archaic forms of the subjunctive (207-8). Of these ET recognize three types: (1) *fuam* (and *forem*); (2) *duas* and *duim*; (3) *fazo* and *faxim*, *amasso* and *amassim*. They call *fuam*, *duas*, and other *-a-* forms subjunctives;⁷⁸ indeed of *fuam* they expressly state that it must have served as a subjunctive when *sim* was still considered an optative.⁷⁹ This must be said in relation to Latin; there is no evidence for subjunctives with *-a-* stems and secondary sequence in Indo-European.⁸⁰ But of *fazo*⁸¹ and *amasso* they say that they have 'perdu leur valeur de subjonctif et elles ne servent plus que comme futurs'. The attribution of subjunctive value to these forms must be made in relation to Indo-European; in Latin they are futures precisely like *ero*, *amabo*, and *duces*. ET unite here three contradictory points of view: a form (such as *sim*) originally an optative in Indo-European, but used at the earliest stage of Latin known to us as a subjunctive, is called an optative; a form (such as *fuam*) of non-specified Indo-European origin, but used at the earliest stage of Latin known to us as a subjunctive, is called a subjunctive; a form (such as *fazo*) originally a subjunctive in Indo-European, but used at the earliest stage of Latin known to us as a future, is called a subjunctive that has lost its force. If *sim* is called an optative, *fazo* might, I admit, be called a subjunctive; but

infinitives of *debeo*, *possum*, etc.—as in *Platonem existimo potuisse dicere*. *Potuisse* represents an original *potuit*, as ET themselves recognize; there is no need for a 'conditional' in the case of either the finite or the infinitive form in Latin, though French does say 'aurait pu parler'. Moreover, even in French usage varies: note the indicative translations, 125, of *videas* and *scires*, which might have been rendered by conditionals.

⁷⁸ I believe, as do many, that in origin they were secondary indicatives; and I try to account for their acquisition of modality in SO 45-9.

⁷⁹ I know of no indication that *sim* or any other Latin subjunctive in *-im* was used as an optative rather than as a subjunctive. But we do find some traces in Plautus of an original differentiation in use between the metrical equivalents *siem* and *fuam* (see SO 48 fn. 88).

⁸⁰ Ever since Brugmann, retracting his earlier statements of 1892 (*Gdr.*¹ 2.952, 1285, 1291), argued in 1916 (*Gdr.*² 2.3.539-40, 835) for the non-existence of an Indo-European *-a-* subjunctive, his view has prevailed among scholars; the only specific exception known to me is Holger Pedersen, *Tocharisch vom Gesichtspunkt der indoeuropäischen Sprachvergleichung* 192 and 199 (Copenhagen, 1941). It is true that Meillet and Vendryes, in their *Traité de grammaire comparée des langues classiques*, do suggest in one place (167) that the *-a-* subjunctive of Italo-Celtic may go back to Indo-European, but they reverse themselves elsewhere in the same volume (176). ET in referring to the existence of an *-a-* subjunctive in early Latin (e.g. 207) seem to imply that there was also an *-a-* subjunctive in Indo-European; so too in their separate works, *Morphologie*³ 160 and *Recherches* 140. But if that is their intention, they certainly owe their readers both a formulation and a justification of so controversial an hypothesis.

⁸¹ I also question the view (207) that *fazo* and *faxim* normally have perfective force (*As.* 503 *haud negassim* seems to me no different from *Men.* 504 *non negem*), and the statement (208) that *ne feceris* began as an adaptation of *ne faxis*. Nor do I believe that *ne feceris* (an old expression) was eventually influenced by Greek *mē poiēsēis* (208); rather both Greek and Latin inherited their tense as a matter of aspect. (Cf. fn. 84, and contrast fn. 152.)

then what can *fuam* be called but an unknown quantity? — Again in regard to the futures (211), oddly said to be 'en partie' old subjunctives (what else are they?),⁸² we find more confusion of Indo-European and Latin. In the third and fourth conjugations, the first person is said to have 'demeuré commun' in the future and the subjunctive. Since the future and the subjunctive have not a common origin, the form in *-am* cannot have 'remained common'; since it belongs to the Latin subjunctive but not to the Indo-European subjunctive, it must have been borrowed from the Latin subjunctive by the Latin future.⁸³ — To say that the future perfect indicative and the perfect subjunctive 'hardly' differed except in the first person singular (211) is an odd way to put it: originally they differed in quantity, which is more than 'hardly' differing; when confusion in quantity took place, they did not differ at all.

The use of the future to express strong probability (a usage common to many languages), which is termed 'un véritable potentiel de probabilité' (211), seems to me closer to a present indicative than to a subjunctive, and therefore to merit comment on grounds of tense rather than of mood. (See *SO* 123 fn. 305.)

ET list a number of perfect infinitives used in an 'atemporel' sense (219). Here again we have aspect: the old legal formula *ne quis fecisse velit* (used e.g. in the *SC de Bacch.* and humorously echoed by Horace, *Serm.* 2.3.187) corresponds closely, as ET themselves note (*ib.*), to *ne feceris* in direct address.⁸⁴ — The gerund is explained as the neuter of the gerundive (221); I believe rather that a gerund misunderstood as a masculine or neuter adjective generated the gerundive.⁸⁵ — The reference to Umbrian and Oscan (226) as providing a parallel for the genitive of the gerundive construction is well illustrated by the first of the two passages cited, *Tab. Eug.* VI A 8, but not by the second, *Tab. Bant.* 24, since the genitive there, *eizazunc egmazum* 'earum rerum', is not a gerundive. — The old construction seen in *cupidus videndi urbis* and *nominandi istorum copia* consists of two genitives independently juxtaposed, as in *Bell. Gall.* 1.30.2 *Helvetiorum iniuriis populi Romani* (226). The parallelism is not exact; *Helvetiorum* and *populi Romani* really are completely independent of each other, one being a subjective and the other an objective genitive; but *videndi* and *urbis*, *nominandi* and *istorum*, are alike objective genitives, and must have been in a quasi-appositional relation to each other.⁸⁶ — The subject of the exclamatory infinitive is an

⁸² Cf. Thomas, *Recherches* xiv, 140, 155. An answer to my question is found in Ernout, *Morphologie*² 256, where the second element of the *-b-* future is traced to an indicative of **bhewa- bhū-*; but in *Morphologie*² no attempt is made to identify this element, apart from the statement (161) that the formation of *amabo* and *monebo* might have been favored by *ero*, which is explained elsewhere (177) as a short-vowel subjunctive. This is of course the explanation regularly given for both *ero* and the *-b-* future, e.g. by Ernout's coadjutor Meillet (Meillet-Vendryes 274); cf. the sweeping statement (*ib.* 273), 'le futur latin repose sur le subjonctif indo-européen'.

⁸³ Cf. *SO* 59 fn. 112. Even Ernout in *Morphologie*², though he errs in my opinion (cf. fn. 80) in calling *leges* and *legas* alike prehistoric subjunctives (160), correctly states (*ib.*) that the first person corresponding to *leges* (**lego*) was replaced by the first person corresponding to *legas* (*legam*).

⁸⁴ The aspectual force of the tense here has already been referred to (fn. 81).

⁸⁵ See *TAPA* 74.277-98.

⁸⁶ Cf. *Lg.* 29.249 fn. 29, 250; *TAPA* 84.114-5.

exclamatory accusative (229). Rather a transfer from the much commoner type in *oratio obliqua*?

There is not always complete simultaneity between the present participle and the main verb (232). *Phorm.* 615-6 *id agitans inveni remedium* is certainly an example of partial not complete coextension (to use terminology usually applied to *dum* clauses), but I do not agree that the meaning is 'après y avoir réfléchi'. Geta found the remedy while he was reflecting, not after he had reflected. — 'Un participe soit en accord, soit à l'ablatif absolu' (239) is badly put. The participle in the ablative absolute (except in the very few cases of an impersonal participle standing alone) is 'en accord' with the substantive accompanying it.

The dative with the gerundive is classed, as usual, as a dative of interest (241). Such constructions as *agrum colendum habebat* and *pugnandum habebam* (both cited 242) suggest that in the equivalent phrases *ei agrum colendum erat* and *mihi pugnandum erat*, the dative is of that particular type of dative of interest known as the dative of possession.⁸⁷ — The gerundive denotes possibility (242). Rather fitness.

The division of clauses into propositions complétives, relatives, and circonstancielles (248) seems to me to involve rather unfortunate nomenclature: nominal (or substantival), adjectival, and adverbial clauses would be clearer so far as names go. But there is a serious difficulty in any classification of Latin clauses: it is convenient to group relative clauses together, yet there may be nominal relative clauses⁸⁸ (as *Bell. Gall.* 4.2.1 *ut quibus vendant habeant*) and adverbial ones⁸⁹ (as those introduced by *quo* or *quominus*). In short, in view of all considerations and in spite of all difficulties, I would reaffirm here my earlier statement that the best way to classify subordinate clauses in Latin is by their introductory word; once this basis is established, we can in each case set up subordinate classes on the basis of the part played by the clause in the sentence, substantival, adjectival, or adverbial.

In origin *quod* is relative, *quia* and *quin* (from *qui* + *ne*) are interrogative (248). This is a false dichotomy. The *quo*- and *qui*- forms always freely interchanged (note interrogative *quos* and relative *quibus*), and the archaic ablative *qui* was quite as much relative as interrogative. ET themselves recognize (253) that *quod* and *quia* alternated in early Latin. — *Quod* 'as to the fact that' is an accusative of specification (251). This explanation has never satisfied me: is it not rather the whole *quod* clause that serves in this capacity?

In *timeo ut veniant* the *ut* is interrogative (261). Very strange! The subjunctive after *verba timendi* is surely optative, whether introduced by *ut* or *ne*. — *Ut* where we would expect *ne* in Horace, *Serm.* 1.3.120-1 (ib.) is doubtless due to the

⁸⁷ I made this suggestion in an article published in 1944 (*TAPA* 74.291-2 and fnn. 105 and 106), and was delighted to find my judgment confirmed by an independent, and more elaborate, treatment of the same point by Benveniste in a paper delivered before the LSA in Ann Arbor on 28 July 1950.

⁸⁸ Indeed, all relative clauses must have been originally nominal; when a demonstrative pronoun or other substitute appeared in the main clause to sum them up, this was in apposition with the clause rather than modified by it. But with the shift of the antecedent from the relative clause to the main clause, most relative clauses became adjectival.

⁸⁹ Actually in Latin nearly all subordinating conjunctions were relative.

use of *non vereor* as equivalent to *non credo fore*; *ut* + optative subjunctive 'may this happen' for the thing one fears will not happen is meaningless when the main verb means 'I do not fear' (i.e. 'I do not believe') that it will happen.

The indicative in indirect questions certainly persisted into the classical period, but in the example from Cicero, *Tusc.* 1.10 (267), the question may be rather direct (parataxis). — The double interrogative emphasizes the dependence of the indirect question (268). But it occurs also in direct questions, e.g. Horace, *Carm.* 3.27.37 *unde quo veni?* — *Si* in Cicero, *Rosc. Am.* 56 *ut significant si fures venerint* is interrogative (271). Surely conditional: the dogs are not expected to signify WHETHER thieves are approaching, which implies indicating either that they are coming or (like the watchman's 'All's well') that they are not; they are merely expected to give warning IF thieves approach.

The explanation of the *accusativus cum infinitivo* as originally a double accusative (271-2) seems to me untenable.⁹⁰ Latin *sentio eum* and *sentio venire* are much more unnatural than French *je l'aperçois* and *j'aperçois venir*, nor do the separate forms account for the element of predication in the combination *eum venire*. The parallelism between the infinitive construction and the participial construction with *verba sentiendi* (274) is more helpful;⁹¹ under certain circumstances there may be little or no difference between *eum audiui dicentem* and *eum audiui dicere*, the parallels cited by ET, but the word order is loaded. *Eum audiui dicentem* can mean nothing but 'I heard him saying', whereas *audiui eum dicere* is much more likely to mean 'I heard that he was saying'. So too *audiui eum tacere* 'I heard that he was keeping quiet', whereas *audiui eum tacentem* would make no sense at all (except as a jest). Furthermore, the passive construction which ET themselves rightly cite (276) as the ancient one, *dicatur Gallos transisse*, disproves their point: if, as they would have us believe (271), *Gallos in dico Gallos transisse* corresponds to *pueros in doceo pueros grammaticam*, then the passive would have had to be originally (as it did indeed become later,⁹² though always with exceptions) *Galli transisse dicuntur*, corresponding to *pueri grammaticam docentur*. And above all the infinitive was not originally an accusative comparable to *grammaticam*; it was a dative!⁹³

The infinitive lacks an imperfect and a pluperfect, these being replaced by the present and the perfect (274). Of course. These tenses in subordinate clauses denote respectively contemporaneous and completed action in secondary sequence, and the infinitive does not differentiate primary and secondary sequence.

The relative clause enjoyed a certain 'autonomy', as revealed by its occasional

⁹⁰ For a more detailed statement of my objections, see *Lg.* 29.251; cf. *TAPA* 81.119-20 and 84.121-2.

⁹¹ I myself believe the infinitive construction began with a participle, but this must have been a past passive or future active, misinterpreted as an infinitive without *esse*, whence the usage spread to non-ambiguous forms: see *TAPA* 81.117-29.

⁹² Probably this construction had spread from its use with verbs of ordering, permitting, etc. The fact that, as ET note (279), with these we have only the personal passive, is the natural result of the altogether different origin of the infinitive with verbs of this type: in *iubeo* (or *sino*) *eum venire*, the accusative really did begin as object of the main verb, while the infinitive had its original force as a dative (of purpose).

⁹³ Cf. fn. 92.

use in parentheses and anacolutha (282). Such uses, also the loose construction in which a second relative is replaced by a demonstrative (283), I would trace back to its origin as an indefinite.

'On voit même se créer *quo nē* ... d'après *ut nē*' (291). The two seem to me equally normal. — The result clause was formed on the model of the purpose clause, with the same conjunction (*ut*) and the same mood (291).⁹⁴ As well say that an indirect question with *ut* and the subjunctive was formed on the model of the purpose clause. The subjunctive in the purpose clause is volitive or optative, hence the negative *nē*; in the result clause it is potential,⁹⁵ hence the negative *non*.⁹⁶ The different tense usage in secondary sequence between the two clauses stems directly from the fundamental difference in meaning:⁹⁷ the purpose clause always points toward a subsequent period, hence the imperfect; the result clause may report a past punctual action, hence the perfect.⁹⁸ — *Quin* as a substitute for *ut non* (292) is, I believe, in result clauses as elsewhere, restricted to clauses following a negative. — Of the clauses classified as 'constructions particulières' (293–4), stipulative clauses belong with purpose, and *quam ut* clauses after a comparative with result.

On concessive *etsi* and *etiamsi* (297–8), I miss a note of the fact that *etsi* tends to accompany the indicative and *etiamsi* the subjunctive. — *Quamvis* is an adverb in origin, like *quamquam* (298); rather an adverbial clause. The subjunctive with it is potential (299); probably rather concessive, which I consider a kind of volitive. Despite ET (299), the conjunction seems to me to keep its force in Cicero's eloquent passage, *Verr.* 5.168 *quamvis civis Romanus esset* 'no matter

⁹⁴ The 'point de départ' was provided by clauses involving 'une idée d'intention ou d'éventualité', such as *Pseud.* 579–83 *ita paravi copias facile ut vincam* and *Tusc.* 3.71 *quis tam demens (est) ut sua voluntate maereat?* (291–2). Certainly 'intention' might form a connecting link between purpose and result, but just as certainly 'éventualité' cannot. Presumably the first example is meant to illustrate 'intention', and indeed it may, but it need not; it may exemplify potentiality pure and simple, 'I have so arrayed my forces that I can easily win out' or 'that I shall be able easily to win out'. The second example can illustrate nothing but 'éventualité', i.e. contingency, potentiality, as ET show by their translation 'qui est assez fou pour qu'il puisse s'affliger volontairement?'. No element of purpose there! With odd inconsistency ET trace the subjunctive in relative clauses of result not to its use in relative clauses of purpose but (rightly in my opinion) to 'l'idée de possibilité' (286); yet these are made of precisely the same stuff as the *ut* clauses of result, even showing at times the same correlatives in the main clause, as ET themselves point out.

⁹⁵ The fundamental meaning of a result clause must have been (as in *Pseud.* 583, quoted in fn. 94) that, as a result of some other circumstance, something was made capable of happening (and, incidentally, it actually did happen); but in common practice the stress is on the actuality rather than on the potentiality. As ET themselves remark (291), the Greek *hōste* clause differentiates the two uses, employing the infinitive for a potential result, and the indicative for an actual one.

⁹⁶ *De Or.* 3.171, cited by ET (292) as illustrating the use of *nē* in a result clause 's'il y a une idée d'intention assez forte', has indeed so strong an element of 'intention' that it can be classified as a purpose clause. Note that ET in translating use the subjunctive, in French normal in purpose but not in result.

⁹⁷ And of course not from a deliberate attempt to differentiate the two types. Cf. fn. 2.

⁹⁸ See fn. 23.

how much he might be a Roman citizen' (cf. Hamlet's *were she ten times our mother*). — (299) *Quamlibet* merits a more extensive treatment than three words at the end of a line, seemingly an afterthought. — *Licet*, translated 'il est possible' (ib.), is rather 'il est permis'. — To the type of correlatives *quo magis ... eo magis, quanto magis ... tanto magis* (302), I would add *ut maxime ... ita maxime, quam maxime ... tam maxime*.

Simul atque cognitum est, venit meant originally 'en même temps (*simul*) la chose fut connue et (*ac*) il vint' (306). This ignores the word order:⁹⁹ *atque* belongs to the first clause, not the second, and so must be intensive,¹⁰⁰ not copulative. There is no connective; the asyndeton makes for vividness. — '*Simul* en arriva parfois à s'employer seul' (ib.). *Atque* adds emphasis, but is not necessary; the use of *simul* alone as a conjunction (like English *once* or French *une fois*) must be original. — (306-7) Does *complerat* in Cicero, *Clu.* 93 indicate repeated action? — In the perfect tense we have the indicative with *ubi, postquam*, etc.; in the imperfect or pluperfect we have the subjunctive with *cum* (310). This approaches the matter backward. It is not the tense that determines the mood or the conjunction; it is the situation that determines the mood and the conjunction, and it is the situation indirectly, the mood directly, that determines the tense. (a) For purely temporal relations we use the indicative: here *ubi, ut*, and *postquam* are commoner, but *cum* is possible. An indicative usually expresses time independently and absolutely; therefore if an indicative verb in a subordinate clause denotes a past punctual action, even though this action may be prior in time to that denoted by the verb in the main clause, it is regularly in the perfect tense¹⁰¹ (usually combined with *ubi, ut*, or *postquam*, occasionally with *cum*, especially *cum primum*). However, if the priority of the action is stressed, as by the use with *postquam* of an expression denoting the interval of time lapsed (e.g. *post tertium annum quam, tribus post annis quam*), we have the pluperfect. The perfect is suitable for punctual action only; naturally, iterative or durative action demands a different tense, imperfect if the action is contemporaneous with the main verb (especially common in *cum* dating clauses, possible also with *ubi* and *ut* but of course not with *postquam*), pluperfect if it is antecedent (es-

⁹⁹ The same sort of reversal reappears later in the treatment of conditions and of proviso clauses. The original meaning of *meam rem non cures, si recte facias* is given as 'tu ne devrais pas t'occuper de mes affaires, ainsi tu ferais bien' (317); and of *oderint dum metuant* as 'qu'ils haïssent, et pendant ce temps (*dum*) qu'ils craignent' (331). In view of the fact that the clause which eventually became subordinated, justly named the protasis, must have come first in early Latin, I would recast the original meanings as respectively 'let us suppose (*si*) you should act well; (in that case) you would not worry about my affairs', which became 'if (*si*) you should act well, you would not worry about my affairs', and 'for a while (*dum*) they fear; (during that period) let them hate', which became 'so long as (*dum*) they fear, let them hate', or, in the commoner order for this particular construction, 'let them hate, so long as (*dum*) they fear'. (The original *dum* temporal clause in developing into a proviso clause changed its mood and usually its position; on this see below, fn. 112.)

¹⁰⁰ Cf. fn. 35.

¹⁰¹ I disagree with the statement (334) that the perfect indicates 'antériorité' in *ubi dixit, abiit*. It does so in *ubi dixit, abiit*, where it is a present perfect; but in *ubi dixit, abiit*, where it is a preterit, the tense is used absolutely, not relatively to the main verb.

pecially with *ubi* and *ut*).¹⁰² (b) For circumstantial relations we use the subjunctive; here *cum* is the regular conjunction. With the subjunctive, sequence must be followed, hence the imperfect or the pluperfect tense.

In Dido's apostrophe of *pudor* (*Aen.* 4.27) the use of the indicatives *violo* and *resolvo* with *ante ... quam* is altogether contrary to usage and logic (313). On the contrary, it may be a subtle way of revealing Dido's realization that she is going to break her vow, as indeed she almost at once decides to do (cf. 55 *solvitque pudorem*).¹⁰³

Dum the conjunction is derived from the particle used with the imperative, e.g. *manedum; scribit* (313). This is the usual explanation, but seems to me unnecessary. Since *dum scribit* must have meant 'he writes for a while', it could have become 'while he is writing'; to assume a previous imperative and a change in the place of the pause seems both needless and dubious.¹⁰⁴ — (313-4) A simpler way to explain the tense usage with *dum* is that in complete coextension, where the two actions coincide absolutely, the subordinate clause naturally has the same tense as the main clause,¹⁰⁵ while in partial coextension the present is always used no matter what the sequence; this usage, pronounced 'ancien' (314), is doubtless a vestige of the earlier aspectual system. — The subjunctive with *dum* in *Georg.* 4.457 *dum te fugeret* is due to the analogy of *cum* (ib.); I think it rather reflects Eurydice's intent,¹⁰⁶ as the subjunctive in *dum conderet urbem* (*Aen.* 1.5) reflects Aeneas's.

In a simple condition, 'la condition est supposée remplie' (317). I think there is no indication whether or not the condition is fulfilled; the chances of realization are one-half, whereas in an ideal condition they are about one-fourth, in an unreal condition zero. — In mixed conditions the 'rapport de dépendance' is less close (318); I do not understand this. — In unreal conditions, the present subjunctive alternates with the imperfect even in the classical period, as in Cicero, *Caecil.* 19 *Sicilia si una voce loqueretur* and *Cat.* 1.19 *haec si tecum, ut dixi, patria loquatur* (321). The two are not parallel. In *Caecil.* Cicero admits that the situation is impossible; after the supposed speech by Sicily, he goes on, *si universa, ut dixi, provincia loqui posset, hac voce uteretur; quoniam id non poterat, harum rerum actorem ipsa delegit*. But in *Cat.* 1, one of the most eloquent and impassioned outpourings of his whole career, he does not merely suggest that a foreign province would speak if she could; he pretends, or imagines, that his own country actually does speak. Before the great outburst that he ascribes to her, he says (18) that she speaks: *quodam modo tacita loquitur*. And after that an ideal condition is certainly in order, not an unreal one. — The examples of mixed

¹⁰² ET emphasize the restriction of these tenses to iterative-durative uses (306-7 on *ubi*, *ut*, and *postquam*, 307-9 on *cum*); to me it appears self-evident.

¹⁰³ For a wholly different explanation, see Conington ad loc.

¹⁰⁴ English *just* and German *nur* (as in Goethe's *Warte nur, balde ruhest du auch*) show no signs of generating conjunctions.

¹⁰⁵ The pluperfect did not occur before Livy (314). Its rarity is doubtless due to the fact that there was little occasion for it.

¹⁰⁶ Cf. Page's translation in his edition, 'so but she might escape thee'.

conditions, 'discordance modale' (322-4), are perfectly logical.¹⁰⁷ It is normal but not necessary that protasis and apodosis belong to the same realm.

The second element in *sin* 'but if' is the interrogative *ne* (326). I question this.¹⁰⁸ The element of interrogation in *Pers.* 227 *sin te amo* is provided not by *-n(e)* as ET think, but by *si*, used (as is French *si* also) in the sense of 'suppose ... ?' I would rather follow Hofmann (779) in taking *-ne* as an affirmative particle from the demonstrative stem *ne- no-*,¹⁰⁹ from which, to be sure, the interrogative *-ne* is also derived (ib. 648). — The imperative is met in the protasis (326); sometimes in both protasis and apodosis, as *Ecl.* 3.106-7.¹¹⁰ — Vergil, *Aen.* 1.180-2 and 6.78-9, and Cicero, *Att.* 13.22.5, which are grouped together (327), seem to me quite different: the first two mean 'to see if he can see', 'to see if she can'; the third, *epistulam Caesaris misi, si minus legisses*, belongs to the illogical colloquial type,¹¹¹ 'I've sent Caesar's letter, if (i.e. in case) you might not have read it'.

The subjunctive in the *dum* proviso clause, such as the often quoted *oderint dum metuant*, is volitive (331). The main clause in sentences of this type frequently is volitive (concessive); because of the close connection in thought and the common parallelism in form, I believe attraction induced the subjunctive in the *dum* clause also, and set the pattern for the type in general.¹¹² — 'La forme mixte de futur II et de subjonctif parfait' (334) must in my opinion in every instance be one or the other.¹¹³ In the example cited, Scribonius Largus 54.7 *ubi bene incaluerit iniciuntur*, I am sure we have a perfect subjunctive, not a future perfect indicative; the mood can be defended, since the subjunctive appears in iterative expressions from Livy on (cf. Hofmann 767), but not the tense. — The form in *-urus fuerit* appears in secondary sequence because it is 'effectivement un passé' (338). Why then is it confined to just two types, result clauses and *quin* clauses? (I understand its use in the former¹¹⁴ but not in the latter.)

The treatment of the subjunctive in subordinate clauses seems to me vitiated by the sporadic envisagement of the subjunctive as the mood of subordination. I say sporadic, for another basis of distinction between indicative and subjunctive is also recognized, that of reality vs. nonreality. The indicative in indefinite rela-

¹⁰⁷ For one thing, certain auxiliaries and periphrases are in themselves modal, and thus in the indicative parallel to other verbs in the subjunctive. Cf. fn. 77.

¹⁰⁸ I also question the view of Ernout-Meillet (s.v. *nē*) that it is the negative particle: *sin* is 'but if'; 'if not' is *si minus* (*sin* only very late).

¹⁰⁹ Also seen in *nam*, *denique*, *egone*, etc.; cf. Walde-Pokorny 2.337. That this may ultimately be connected with the negative particle too (cf. fn. 108) is not impossible; cf. *Lg.* 18.102 fn. 107.

¹¹⁰ Cf. English *spare the rod and spoil the child* and the often misunderstood *feed a cold and starve a fever*.

¹¹¹ Like Vergil, *Ecl.* 3.23 (quoted in fn. 65).

¹¹² See *TAPA* 66.199-207. The common position of the *dum* proviso clause after the main clause probably facilitated the attraction (ib. 201 and fn. 7); cf. fn. 99.

¹¹³ Cf. the earlier reference to an ambiguous dative or ablative as a 'forme mixte' (on this see fn. 54).

¹¹⁴ Because of the common use of the perfect in such clauses. Cf. fnn. 2 and 23.

tive clauses¹¹⁵ 'à sa fonction de mode de la réalité' (283); substantive *quod* clauses indicate 'une chose ou une circonstance effectivement réalisée' and consequently are in the indicative (251); the indicative plays a large part in temporal clauses because they 'servent le plus souvent à désigner des faits' (305). Agreed. But what then becomes of the subjunctive as a mood of subordination? — The very name of the subjunctive mood is attributed to its use as mood of subordination (195 and 248). This erroneous view stems from a misapplication to Latin by Roman grammarians, such as Priscian, of statements concerning Greek by Greek grammarians, such as Dionysius of Thrace and Apollonius Dyscolus.¹¹⁶ That a term perfectly acceptable from the viewpoint of the Greeks who devised and applied it has been automatically carried over in Latin, French, and English nomenclature to apply to a mood by no means restricted to subordinate uses is regrettable, particularly as it has led to serious misconceptions as to the true nature of the mood to which it has been applied. — If the Latin subjunctive is, or becomes, the mood of subordination, as ET maintain, how are we, or they, to answer the following questions? Why are some conjunctions regularly followed by the indicative and some by the subjunctive? Why are some conjunctions followed now by the indicative and now by the subjunctive, in different senses? Why do we not find the subjunctive everywhere in subordinate clauses? Why, in view of the fact that we do not, do we not find indicative and subjunctive interchanging in complete promiscuity?¹¹⁷ Why do ET themselves formulate rules explaining the occurrence of subjunctives in special types of subordinate clauses, and clarifying the distinction between indicative and subjunctive in other special types? In short, the refutation of this view of ET on the subjunctive as a mood of subordination is found in their own pages. I do not question the validity of their statements concerning actual modal uses in Latin, but I maintain that their explanations become in part otiose and in part inexplicable, if the subjunctive is simply the mood of subordination.

To begin with temporal clauses, as I think discussions of subordinate clauses should begin, I would say that the difference between the indicative with *cum* purely temporal, and the subjunctive with *cum* circumstantial (of which *cum* causal and *cum* concessive are simply variations), is precisely the same as that

¹¹⁵ ET contrast the Greek use of the subjunctive and optative (ib.). They might have noted the more widespread tendency in Greek to employ these moods in any type of generalization, e.g. the general condition.

¹¹⁶ The Latin subjunctive expressed futurity, will, potentiality, and wish. In Greek the subjunctive exercised the first two functions only, the last two being fulfilled by the optative. The subjunctive of futurity survived in subordinate clauses (e.g. protasis of more vivid future condition), but disappeared after Homer in principal clauses. This left in main clauses only the subjunctive of will, seen mainly in hortatory and jussive expressions. Apollonius classes these as a separate mood, the *hypothetikê* or adhortative, serving as a suppletive to the imperative (361.7–11 and 364.8–365.4 Uhlig); and consequently he finds no examples outside of subordinate clauses for the mood which he thus with justice calls *hypotaktikê* or subjunctive.

¹¹⁷ I am not denying the existence of occasional variations in mood which we would be hard put to account for. An excellent example cited by ET (308) is Cicero, *Leg. Agr.* 2.64 *tum cum haberet ... et tum cum erant*. In *cum* clauses of this type either the indicative or the subjunctive is possible; but why did Cicero shift from one to the other in two juxtaposed and coordinated clauses, seemingly with no change in nuance?

between the indicative and the subjunctive with *is qui*¹¹⁸ — a distinction which ET make very neatly indeed (286) with no question of a subjunctive of subordination. But in regard to temporal clauses they say (305) that the subjunctive occurs there 'soit comme subjonctif à valeur propre,¹¹⁹ soit comme subjonctif de subordination'. They of course recognize the use of *cum* temporal with the indicative (307-9), and they justly explain the use of *cum historicum*¹²⁰ with the subjunctive as containing 'une idée adventice de cause, d'opposition, etc.' (309). But when we turn to the earlier discussion of *cum* causal and concessive, we find the subjunctive with them called the subjunctive of subordination (294 and 299), and so too the corresponding relative clauses¹²¹ of cause and concession (285).¹²² Yet it is admitted that the indicative is used in causal clauses introduced by *quod* and *quia* (295), and in concessive clauses introduced by *etsi* and *quamquam*, these last resembling respectively conditional clauses introduced by *si* (297) and indefinite relative clauses introduced by *quisquis* (298). The occasional use at a late period of the subjunctive with *quod* or *quia*¹²³ and with *etsi* is attributed to the influence of *cum* (296 and 298) and not to subordination. And we also have the general statement (248) that eventually, as a result of the progress of the subjunctive of subordination, the use becomes automatic after certain conjunctions (*cum*, *etsi*, *priusquam*, *quamquam*) but not after others (*quando*, *quia*, *ubi*).

Comparative clauses have the indicative because they are 'faiblement subordonnées' (300). *Ut* correlative with *ita* in the main clause is found not only in comparative clauses but also in concessive, final, and consecutive clauses; and I question whether we should try to measure degrees of subordination in any of them. In all of them the two clauses must have been originally coordinate, and the clause that later became subordinate retains the mood that it had originally. In their completely developed stage I would say that the final clause seems more completely subordinated than the consecutive one;¹²⁴ yet here degrees of sub-

¹¹⁸ The parallelism between *qui* clauses and *cum* clauses is in general close, as ET themselves note (308).

¹¹⁹ The subjunctive in temporal clauses other than those introduced by *cum* is said (307) to be due to 'style indirect', 'attraction modale', or 'répétition'. But all these are causes for the subjunctive in general, with no relation to any particular category such as temporal clauses.

¹²⁰ Or *cum* circumstantial, as I prefer to call it. The restriction of this construction to historical tenses I have tried to account for in *SO* 136 fn. 358.

¹²¹ Cf. fn. 118.

¹²² So too relative clauses of result. On these see fn. 127.

¹²³ Except in the 'style indirect' (253 and 295); cf. fn. 119. The subjunctive with *quia* in *Cist.* 101-2 *mea mater iratast mihi, quia non redierim domum ad se* (cited 254 as an example of a rare use) undoubtedly falls into this category, as is indicated by the employment of the reflexive *se* (rather than *eam*, which the girl would use if speaking for herself); she perhaps disrespectfully mimics her mother's reproachful tone. ET end their quotation with *redierim*; perhaps had they included *se*, they would have recognized the occurrence of what they themselves elsewhere call 'style indirect libre'.

¹²⁴ Note the emancipation of the consecutive clause from the normal laws of sequence (cf. fnn. 2, 23, and 114). Also, *tam acriter pugnauerunt ut vicerint* is equivalent in meaning to *acriter pugnauerunt, itaque vicerunt*; but in *acriter pugnauerunt ut vincerent* the two clauses, main and subordinate, could in classical Latin hardly be separated.

ordination are not considered. As has already been pointed out,¹²⁵ the subjunctive in the purpose clause is (rightly) said to be 'employé à son sens propre d'intention ou de volonté' (290), and that in the result clause is (wrongly) attributed to the same origin (291).

The treatment of relative clauses of purpose and result is inconsistent with, and superior to, that of *ut* clauses. Here the two are kept completely separate, and the force of the subjunctive is in my opinion correctly given in each case, as respectively 'volition' (284) and 'possibilité' (286),¹²⁶ with an addendum in the latter case that the subjunctive here, as in the corresponding *ut* clause, was extended to embrace facts.¹²⁷ Yet elsewhere (285) the subjunctive in relative clauses of result is, as already noted (fn. 122), attributed to subordination. In brief, the practice of ET seems to be to account for a subjunctive in its own right when this is obvious, but to call it a subjunctive of subordination when difficulties arise.

I now turn to a group of subjunctives harder to account for, because they cannot be explained as showing volitive, optative, or potential force.

For the subjunctive in indirect questions ET give two conflicting, or at least overlapping, explanations: (1) it is due to subordination, and (2) it spread from questions in which the subjunctive was necessary, as a deliberative, a potential, or a 'subjonctif du style indirect' (266). But why did this spread from deliberative or potential questions to questions of fact take place only in indirect and not in direct questions? As for 'style indirect', even apart from the fact that one can hardly distinguish the indirect question from the question in indirect discourse, this is a mere begging of the question; the use of the subjunctive in quotations needs explanation quite as much as its use in questions. Oddly enough, the subjunctive in subordinate clauses in indirect discourse is not explained by ET, though it has been by others (e.g. Hofmann 701), as a subjunctive of subordination. Here they use their other basis of classification, reality vs. unreality: 'le subjonctif rapporte l'affirmation à un tiers, au lieu de la donner comme une réalité objective' (359).¹²⁸ Why, then, do we not find the subjunctive in main statements in indirect discourse as well as in subordinate statements and main questions? Why, moreover, do we meet the indicative both in indirect questions and in subordinate clauses in indirect discourse¹²⁹ not only in early but also in classical Latin? Hence I feel in regard to both types that the explanation pro-

¹²⁵ See fn. 94.

¹²⁶ Again see fn. 94.

¹²⁷ It is quite possible that in the relative clause as in the *ut* clause potentiality shifted to actuality (see fn. 95), but the example cited by ET (286), *Rosc. Am. 52 nunc dicis aliquid quod ad rem pertineat*, does not illustrate this. Here *pertineat* is a true potential: 'something that might have a bearing on the case (if it could be proved)'. For Cicero goes on with all his lawyer's skill and drive to demonstrate that the point in question (*istum exheredare in animo habebat*) cannot possibly be proved: cf. in particular his question *quaero qui scias* in 53, and his imaginary cross examination leading to a complete negative in 54.

¹²⁸ Cf. their explanation of the subjunctive 'du style indirect' as specifically met in indirect questions: 'le narrateur rapportant les propos d'autrui, ou les siens propres' (266). There seems, however, much less reason for the subjunctive if one is reporting one's own words.

¹²⁹ Even subordinate clauses that are an integral part of the indirect discourse.

vided by ET is inadequate, and that a more satisfactory substitute should be sought and may be found.¹³⁰

The iterative subjunctive is due to the close relation between repetition and contingency (339). There is justice in this observation,¹³¹ but the explanation does not quite go to the heart of the matter. In the whole discussion of the problem (338-40) the restriction of this subjunctive to secondary sequence is not accounted for or even explicitly commented on. The reason is, I believe, that the future rather than the present indicative was often used in a gnomic sense: early Latin freely interchanged the future indicative and present subjunctive in primary sequence, but in secondary sequence had only the imperfect subjunctive as a substitute for either.¹³²

For the subjunctive by attraction ET assign two possible reasons (341-2). One is 'éventualité' again, or 'indétermination'. This I doubt. Indefiniteness alone is not a reason for the subjunctive in Latin.¹³³ Potentiality of course is, but in that case we need not talk about attraction; however, I see no potentiality in any of the examples cited by ET (342),¹³⁴ though most of them may involve indefiniteness. The other is the reason that I myself would assign for the subjunctive by attraction, as for the subjunctive of iteration: i.e. the close connection of subjunctive and future. ET suggest that modal attraction to a certain extent corresponds with sequence of tenses (340); I would go further and say it actually is in origin a form of sequence of tenses.¹³⁵ In early Latin attraction is a matter of tense rather than of mood;¹³⁶ in classical Latin, when the subjunctive is no longer equivalent to the future indicative, it is a matter of mood rather than of tense. Hence I do not agree with ET (341) that in classical prose attraction is more developed than in early Latin;¹³⁷ it is merely different.

¹³⁰ Such explanations I have offered elsewhere: for the former, *TAPA* 83.248-62; for the latter, *ib.* 262-6 and *SO* 133-6.

¹³¹ I have commented elsewhere (*Lg.* 20.98) on the close connection between the general condition and the future condition.

¹³² Cf. *SO* 136 fnn. 358 and 359.

¹³³ Cf. fn. 115.

¹³⁴ In one of them, *As.* 29 *dic quod te rogem*, ET interpret *quod te rogem* as 'à ce que je peux éventuellement te demander', on the ground that the context here 'ne prêtait pas à attraction'. All students of attraction (including ET themselves) point out that a factor strongly favoring it is reference to the future, and this requirement is fulfilled quite as well by an imperative (*dic*) as by a future indicative. Cf. fn. 136.

¹³⁵ Cf. *SO* 129-33.

¹³⁶ This is proved by the fact that the main verb need not be a subjunctive at all, so long as it refers to the future. It may be a future indicative (cf. *Pers.* 16), an imperative (cf. fn. 134), or an infinitive, even a present infinitive if it refers to the future, as many present infinitives do in early Latin (cf. fn. 22). Hence I do not agree with ET (342 note) that attraction by an infinitive is 'peu vraisemblable'; see further on this *SO* 132-3, especially 132 fn. 346.

¹³⁷ I think attraction is much commoner in early Latin than they admit. However, I question whether the passage that they cite from early Latin as a clear example, *Mil.* 149 *faciemus ut quod viderit ne viderit*, is as certain an instance as they think, for I do not agree with their statement that *viderit* (the first one) cannot be a future perfect indicative. To be sure, 370 *quin viderim id quod viderim* is an argument for the subjunctive; but 571-3 *linguam comprimes, illud quod scies nesciveris nec videris quod videris* is an argument for

In dealing with sequence of tenses ET protest against the view of the perfect subjunctive as 'un temps exclusivement "présent"' (346). It seems to me that the way to deal with the problem is to emphasize that dependent subjunctives (as also infinitives in indirect discourse) express time not absolutely but in relation to the verb on which they depend — which is not necessarily the main verb. For time prior to a present or future indicative, or to an independent present subjunctive, or to a dependent subjunctive denoting the same time as either of the foregoing (i.e. a present subjunctive), we use a perfect subjunctive; and for time prior to any mood or tense whatever, we use a perfect infinitive. When the prior time denoted by the perfect subjunctive or infinitive remains within the present-future period, subjunctives depending on it remain within 'primary' sequence; but when the reference to prior time switches us back into the past period, subjunctives depending on it are shifted to 'secondary' sequence. This comes to about the same thing as the statement of ET (346) that the perfect subjunctive has the same double value, perfect proper and preterit, as the perfect indicative; but it seems to me undesirable to deal with the tenses of the dependent subjunctive in terms of the indicative. Thus ET offer (ib.) as their first example *Verr. Act. Prima 3 cum multae mihi insidiae factae sint, numquam tamen ... pertimui*, and explain *factae sint* as representing *factae sunt*. But this is not an adequate explanation. They do not translate *pertimui*, or give the significant words that follow it, *ut nunc*. The meaning of *numquam tamen tanto opere pertimui ut nunc* is 'I have never been as much afraid as (I am) now'. Had we had *pertimui* in the sense of 'I WAS afraid', or had we had *pertimebam*, the dependent subjunctive would have been not *factae sint* but *factae essent*, and yet the indicative represented by it would still have been *factae sunt*. Their second example, *Phil. 3.27 qui haec fugiens fecerit, quid faceret insequens?*, also belongs to the present period; *faceret* in an unreal condition refers to the same period of time as *faciat* in an ideal condition, i.e. the present-future or 'primary' period.¹³⁸ The translation 'qu'aurait-il fait?' is misleading, for it would demand *fecisset*; it should rather be 'que ferait-il?'.¹³⁹

Of course sequence of tenses is not a woodenly mechanized phenomenon, and it is part of the great merit of ET that this is emphasized.¹⁴⁰ Perhaps they might have been more explicit over one special problem — the fact that, because of the paucity of subjunctive tenses, the demands of time and of aspect sometimes conflict. The dependent subjunctive, being used relatively to some other verb, is usually either durative (denoting contemporaneous action) or perfective (denoting prior action), but in the comparatively few instances involving punctual

the indicative, since *quod videris* is parallel to *quod scies*. (However, *comprimes* suggests that *nesciveris* and the first *videris* may also be indicatives, in which case this passage will have no bearing on 149.)

¹³⁸ ET themselves very properly note (347) that present unreal conditions 'escape concordance'.

¹³⁹ Possibly ET avoided this because of its ambiguity. French does not distinguish the present unreal from the ideal. (English often uses the same form for the apodosis of both, but can more easily vary the protasis.)

¹⁴⁰ But they do believe that modal attraction may be 'purement mécanique' (340). I question this.

aspect, complications may arise. It is particularly in the past that we need to distinguish the three aspects, and here the indicative provides three tenses, imperfect, perfect (or preterit), and pluperfect. The subjunctive too has these three tenses, but their distribution for sequence limits their availability for aspectual nuances, since the perfect is mainly confined to primary sequence, serving there not as punctual in past time but as perfective in present time, corresponding precisely to the pluperfect in secondary sequence. But in a result clause, as already noted (fn. 23), a past punctual action may be needed, and then the perfect subjunctive is often requisitioned regardless of sequence.¹⁴¹ Likewise occasionally an example in present sequence may involve a reference to durative action in past time, and then again sequence may be violated, as in the instance cited by ET (347), *Verr.* 2.191 *laudantur oratores veteres ... quod copiose defendere solerent*; the tense demanded by the rule of sequence, *soliti sint*, would imply that the action went on within the present period.¹⁴²

Naturally, when the period referred to actually shifts within a sentence, sequence shifts too. In the four examples of the imperfect subjunctive in primary sequence (347–8), *veteres* in *Verr.* 2.191 (just discussed) and *ille annus* in *Pis.* 26 have the same effect in carrying us from the present to the past as the perfect infinitives *pollicitos (esse)* in *Aul.* 470–1 and *pepercisse* in *Planc.* 71. On the other hand in the examples of the present subjunctive in secondary sequence (348) we have a shift from the past to the present: in *Fam.* 6.7.1 *filus pertimuit ... cum adhuc poenas dem*, the present *dem* shows that the writer is still paying the penalty (note *adhuc*), whereas *darem* might limit this to the past period, denoted by *pertimuit* and *noceret*; similarly in *Leg. Agr.* 2.92 the persons involved were called *duumvirs* and still are. In *Dom.* 99, where we have the perfect subjunctive, the main clause *bis servavi rem publicam* is, I think, in primary sequence, 'I have saved' not 'I saved'; Cicero's act in saving the state is too vivid in his own mind to be relegated to a past period as ET seem to think it is (348). In *Bell. Gall.* 1.11.3 the tense of *debuerit* might be explained in either one of two ways, though ET (349) give neither: since the main verb, *mittunt*, is primary (historical present), and the infinitive, *meritos esse*, is present perfect, not preterit, the perfect subjunctive might be quite in order as a primary tense; or, more probably, we might have here the common use of the perfect subjunctive for punctual (negative) action in a result clause. In *Balb.* 2 *quae fuerit hesterno die Pompei gravitas declarari videbatur*, the explanation given by ET for the use of *fuerit* rather than *fuisset* seems unnecessary; the explanation I need is why we do not have *esset*. Perhaps we have a sort of anacoluthon: when the speaker said *quae fuerit hesterno*

¹⁴¹ ET explain the use of the perfect as an attempt to distinguish consecutive clauses from final ones (353); the context does that (cf. fn. 2). Similarly, the imperfect when used in a result clause 'apporte une idée de finalité' (352). This might be true of *Livy* 23.24.7, but on the other hand the passage may simply illustrate 'possibilité', as ET realize is the case in *And.* 135–6 *illa, ut facile amorem cerneret, reiecit se in eum*. (Here the imperfect *cerneret* 'you could perceive' would be used even in a main clause, and therefore is naturally retained in the subordinate clause; cf. fnn. 138 and 144.)

¹⁴² Or we might say that the actual words quoted here by implication were *veteres oratores laudabiles erant quod solebant*, which in explicit quotation would be *dicuntur veteres oratores laudabiles fuisse quod solerent*.

die gravitas, he meant to go on with *constat* or *scitis*, but shifted instead to a reminiscence of past time, *declarari videbatur*. In *Verr.* 1.75 (ib.), *potuerit* troubles me greatly, as it does a number of editors.¹⁴³ In *Val.* 5 *quaero cur Cornelium non defenderem*, ET explain the imperfect subjunctive 'en face d'un verbe principal au présent' as used to provide 'une nuance modale' (349). That is one way of looking at it; the sentence means 'I ask why I was not to defend C', whereas with *defenderim* it would mean 'why I did not defend C'. But the real point is that even in the original direct question (which, as a matter of fact, is reproduced here word for word, so that we might take this as an instance of parataxis) we would have had the imperfect subjunctive to provide the 'nuance' in question, without any 'verbe principal' to complicate the situation; and the imperfect subjunctive in a principal clause is retained unchanged¹⁴⁴ when the clause becomes subordinate, no matter what the sequence. — In *Stich.* 679–80 *curavi cena cocta ut esset*, *cocta* is practically an adjective,¹⁴⁵ so *cocta esset* is an imperfect, not a pluperfect as ET call it (350).

The following comments apply to indirect discourse. *Ne dubitaret* represents original *ne dubitaris* (358); more probably *noli dubitare*. — Temporal clauses sometimes retain the future indicative of direct discourse (360),¹⁴⁶ probably because of the original interchangeability of future indicative and present subjunctive.¹⁴⁷ — The present subjunctive may be used in secondary sequence for a general truth (363–4). Naturally,¹⁴⁸ the author has free choice between the present, implying that the matter is true when he is writing, and the imperfect, implying that it was true when the speaker was speaking.¹⁴⁹ — Horace, *Serm.* 1.9.35–8 mixes indicative and infinitive in the 'free indirect style' (368). This is one of the extremely rare slips in ET. Baget in his article on the 'style indirect libre' (cited by ET), *Revue de Phil.* 5.338 (1931), does indeed quote this passage, citing *casu*, *tum*, and (on doubtful grounds, I think) *ades* as signs of the style (to which I would add the mood of *fecisset*); but there are no pertinent infinitives here, for *perdere* like *respondere* depends on *debebat*.

To the factors leading to asyndeton (370–1) might be added the detail that repetition often replaces a connective: e.g. Horace, *Ep.* 1.7.27 *reddes dulce loqui*,

¹⁴³ E.g. Kayser and Peterson, who emend to *statueret* and *oporteret* respectively. On the latter see *CR* 17.202.

¹⁴⁴ In a deliberative or potential question just as in an unreal condition; cf. fn. 138.

¹⁴⁵ Like *parata*; cf. fn. 28.

¹⁴⁶ However, in one of the examples cited, *Sen.* 79 *nolite arbitrari me, cum discessero, nusquam fore*, there may be confusion with a variant form in which the *cum* clause is outside the quotation: *nolite arbitrari, cum discessero, me nusquam fore*.

¹⁴⁷ In secondary sequence there is of course no alternative for the subjunctive. Cf. fn. 120.

¹⁴⁸ Nor is it surprising that after such a generalization the present may continue even in reference to specific past action. Thus in *Bell. Gall.* 1.14.5–6 (*respondit*) *consuesse deos, quo gravius homines doleant, impunitatem concedere; cum ea ita sint, si obsides dentur, sese pacem esse facturum*, ET explain the tense of *doleant* as expressing a general truth, but that of *dentur* as expressing futurity with reference to the main verb. Rather the tense of the general *doleant*, through the transitional phrase *cum ea ita sint*, carries over to the specific *dentur*. However, I do not deny that in such instances *repraesentatio* (already referred to in fnn. 2 and 32) may play a part.

¹⁴⁹ Similarly, English can say *he did not believe that the world is round or was round*.

reddes ridere decorum. — In *Off.* 3.41 *id quod utile videbatur neque erat*, we might have expected *et non* (375). Perhaps not if we interpret *neque* as joining the two verbs, not 'what seemed useful but was not (useful)' so much as 'what seemed, but was not, useful'. — The seemingly illogical use of *nec* in *Dom.* 64 *nec intellegabam fieri diutius posse* (375) is facilitated by the widespread tendency to place the negative with the main verb (cf. *nego* for *dico ... non*, *I don't think* for *I think ... not*). — Why are *-ve* and *-que* called archaisms (376)? They are doubtless old, but did not early become obsolete. — Has *aut* really weakened force in *caesi aut capti* (377)? The terms exclude each other. — *Nunc vero* expresses reality 'après un irréel' (380); so, too, *nunc* alone. — To the correlative *cum ... tum* (382) add *ut ... ita*; the two pairs are absolutely parallel in their combination of indefinite-relative and demonstrative.¹⁵⁰

The 'arbitrary' order in Horace, *Serm.* 2.3.139 *non Pyladen ferro violare aususve sororem* (383) is in accord with his practice of distributing between two successive clauses words common to both: e.g. *Serm.* 1.6.88 *laus illi debetur et a me gratia maior*, *Carm.* 1.11.4 *seu pluris hiemes seu tribuit Iuppiter ultimam* (*hiemes* in the first clause of course doing duty for *hiemem* in the second).

A reviewer naturally dwells on details of disagreement rather than of agreement; but I cannot close this review without noting a few of the many items that seem to me especially meritorious. Among them are the discussions of the following topics: nominative not merely as subject case but as general case or 'cas-zéro' (11-3);¹⁵¹ local ablatives, especially those without prepositions (82-6); 'prepositions' retaining their original use as particles (99); late prepositions and prepositional phrases, ignored by most grammars (103-4); verbs with indefinite subjects (124-5), especially the distinction between the first plural and other forms (125); the reflexive (155-8), especially some particularly striking illustrations of the statement (157) that the reflexive need not refer to the subject; aspect, a topic normally neglected in Latin grammars (184-7);¹⁵² the 'infinitif substantivé' (215-6); sequence of tenses (344-55), especially the emphasis on the influence exerted by different types of clauses (347).

Many of the individual points are illuminating and significant; as specimens I offer the following. The distinction between transitive and intransitive is not fundamental (16).¹⁵³ Compound verbs take datives not because of the preverb

¹⁵⁰ So too in my opinion Greek *mén ... dé*. See *Lg.* 18.106-7.

¹⁵¹ Among instances of this ET (11) include 'les phrases nominales' and cite as an example *Sest.* 74 *clamor senatus, querelae, preces, socer ad pedes abiectus*. Here, where we have simply a list, I agree that the nouns cannot be classed as subjects, but I question whether this is a true 'nominal sentence'; to me the latter implies predication quite as much as a verbal sentence, the difference being that the predicate is a substantive or adjective instead of a verb (as in *Aen.* 6.126 *facilis descensus Averno*); and in such a clause the noun is the subject just as much as in the ordinary type.

¹⁵² Some individual items also show the importance of aspect in Latin, e.g. the treatment of the role of *ne* with the imperative (197), much better than Hofmann's (575), and showing clearly that Latin (like Greek) used the imperative for durative aspect; and the comparison of the present participle with the present and imperfect indicative as expressing 'la tentative et l'effort' (232).

¹⁵³ I would add in Latin as we know it; this distinction may have been more important at a very early stage, particularly in the use of nonfinite forms (cf. *TAPA* 74.269-74).

but because of their meaning, which, to be sure, may be due to the presence of the preverb (59).¹⁵⁴ With local prepositions there is a tendency to use the accusative with the 'infected' and the ablative with the 'perfectum' (96). A good distinction is made between *pro* and *ante* (98); also between *omnis* and *totus* (170). ET give the only satisfactory explanation I have ever found of the case of the pronominal element in *posthac* and *postea*, *antehac* and *antea* (99);¹⁵⁵ presumably it would apply also to *praeterea* and *propterea*. A sensitive and perceptive feeling for literary style is revealed by the comment (101-2) that the intercalation of *inter* between the two substantives that it governs establishes 'une liaison expressive' between meaning and order, as in Horace's *timores inter et iras*.¹⁵⁶ The rule that a verb agrees in person with its subject if the latter is a personal pronoun (108) is unusually well-put; the usual formulation, that the verb agrees with its subject in person as well as in number, ignores the indubitable fact that nouns and most pronouns lack the category of person.¹⁵⁷ The comment that the absence of an article reduced the number of 'nominal sentences' in Latin (126) is interesting, as are the examples given. So too is the comment that the possession of an imperfect subjunctive made possible a 'past jussive', as in Cicero's *moretur*, *inquies* (199).¹⁵⁸ The preservation of the impersonal *dictum est* despite the introduction of the personal construction in simple tenses is aptly attributed to the analogy of such neuter locutions as *manifestum est* (277). The term 'relative consécutive' (286), though not completely satisfactory, is surely preferable to the common 'relative clause of characteristic'.

Numerous other passages of great value, either as precept or as example, might be cited; but perhaps I have said enough to show, despite my earlier strictures, that I am deeply impressed by both the excellence and the importance of *Syntaxe latine*.

¹⁵⁴ ET point out (ib.) that the simple verbs may also govern the dative, but certainly this is comparatively rare; with simple *mitto*, cited by ET, a dative of the sort is surely less common than an *ad* phrase, and with simple *sum* is non-existent.

¹⁵⁵ However, they do not touch on the gender, so the problem remains obscure. I have sometimes wondered whether the *-eā* forms might be not ablatives but accusatives, retaining the original Italic quantity, like *trigintā* etc. But that will still not account for the ablative form of the adverbs *quo* 'whither' (also *quoad*) and *eo* 'thither' (also *adeo*), which I find baffling; ET compare *quoad* with *quam ad* and *quos ad* (101), which are relevant so far as order goes, but not in regard to the case of *quo*.

¹⁵⁶ Somewhat similar is the image created in Vergil's lovely line *Aen.* 8.608 *at Venus aetherios inter dea candida nimbos*, where the word order suggests the goddess' radiance gleaming in the midst of dark clouds.

¹⁵⁷ Even Hofmann (631) slips up in this respect. But in Nero's *qualis artifex pereo* (Suetonius, *Nero* 49.1) *artifex* is surely subject of *pereo* as much as it would be of *perit* in *qualis artifex perit*; similarly in Nepos, *Them.* 2.9.2 *Themistocles veni*, and its obvious model Thucydides 1.137.4 *Themistoklēs hēkō*. Examples of this sort are rare in Greek and Latin but abound in Hittite.

¹⁵⁸ An excellent parallel in French that might well have been added occurs in Act 3 scene 6 of Corneille's *Horace* (line 1021): *Que vouliez-vous qu'il fit contre trois?— Qu'il mourût!*

Grammaire homérique: Tome 2. Syntaxe. By PIERRE CHANTRAINE. Pp. viii, 379. Paris: Librairie C. Klincksieck, 1953.

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I began a review of the first volume (*Lg.* 22.144-52 [1946]) by saying: 'This is one book that must for years be constantly at the elbow of every Homericist, where it will earn for its author admiration and gratitude'; and ended: 'I shall always keep it on hand, and hope to profit from the possession of such a tool.' I wish to repeat this most emphatically, and can add but little.

The need for information about Homeric syntax seems to me more acute than about morphology or phonology. The author singles out (viii) six works to which he feels especially indebted. One is inaccessible, but the others are wisely chosen. They are Leumann's *Homerische Wörter*, my admiration of which is expressed in *Lg.* 27.68-80 (1951); the brilliant and profound *Vorlesungen über Syntax* by Wackernagel; the sane and learned commentary of Leaf; the monumental *Thesaurus* of Schwyzler-Debrunner's *Griechische Grammatik*; and the wise systematic treatment of the whole subject in Monro's *Grammar of the Homeric dialect* 127-338. It will be observed, however, that in four of these works Homeric syntax is only part of a much larger theme; while the other dates from 1891, that is two generations ago. It is a tribute to the genius of Monro to note the way in which the usefulness of his work has stood up in spite of this lapse of time, and no occasion for anything but joy that we are at last presented with a new treatment of the subject which requires (roughly) double the space.

The method employed is familiar. It is the one that was born in Europe shortly after the battle of Waterloo from the impact of the discovery of Indo-European upon the Greco-Roman tradition. It has since been developed and refined continually, on both sides of the Atlantic. Through this long history Chantraine pilots his readers skilfully. In the first place he refers them to the wealth of Schwyzler-Debrunner; but then he supplements this information by adding items that have been overlooked, or have appeared since 1949. More valuable still is the way in which he emphasizes those treatments of the subject which are of the greatest help. Chantraine's command of this bulky material is most enviable, and his work is to be regarded, in my opinion, as the peak of what can be attained by this method.

That, of course, does not mean that the answers to all questions have been found. On the contrary, one of the features that attracts me most in Chantraine's book is the frankness with which he points to the difficulties that remain. In part they spring from the nature of the material with which we must work. We are reminded again and again of the uncertainty of the text. For instance we are told (245, 356) that in Σ 209 the tradition divides between κλίνονται and κλίνονται, and (250) that in Γ 10, (T 386 is a different problem) there is a variation between εἶτε and ἦτε. The significance of the facts is limited; the earlier text was ambiguous (KPINONTAI, ETTE), and our mss. show merely the varying interpretations that have been put on it.

In the tradition of the 'Vulgate', larger variations—zero alternating with

one or more lines—rarely call for mention. They are found when lines have been added to the 'Vulgate' between Aristarchus and Wolf. Of nearly 80 examples in the Iliad, not one is listed among the 'principaux passages' that are indexed. I have noted mention of three and can see a reason for their preferred treatment: two led some sort of existence before Aristarchus was, and there is some probability that the same was true of the third.

The Ajax line of the Catalog, B [558] is described by Aristonicus at Γ 230 contemptuously as the line written by some people, the phrase going back perhaps to Aristarchus. Chantraine (268) cites it as an example of *ἵνα* 'where' without an antecedent substantive, and compares two very late passages of the Odyssey: θ 313 Song of Demodocus, ρ 68 in a passage added by B, cf. Merkelbach, *Unters. z. Od.* 74. If this is all, the fact is interesting, but a description of the tradition of B [558] would have been in place.

Θ [183] *'Αργείους παρὰ νηυσὶν ἀνυζόμενους ὑπὸ καπνοῦ* is an interesting instance of the sort of thing noticed recently by Jachmann, *Class. Phil.* 47.87-90 (1952). The line is present in P 7 (3a). Aristarchus did not include it in his edition, for it is not found in P 198 (2p.), P 197 (2/3p.), nor in the earliest mss. It was known to Eustathius, who had at his disposal 'eine bessere und vollständigere Form der T-Scholien'; the phrase is from p. 3 of Hartmut Erbse, *Zur handschriftlichen Überlieferung der Iliasscholien*, *Mnem.* SIV 6.1-38 (1953), an article of fundamental importance. It would lead too far afield to give what seem many good reasons for believing that this forerunner of T was here Eustathius' source. It then appears in the text or margins of some mss., and so gets into Wolf's edition. In his comment Eustathius speaks of *περὶ καπνῷ ἤγουν ὑπὸ καπνοῦ*. Chantraine (127) mentions Eustathius and concerns himself with the variations of the mss., *ὑπὸ καπνοῦ*, *ὑπὸ καπνῷ*, once *περὶ καπνῷ*. The treatment does not seem well balanced.

For X [121] the situation was probably similar, tho it is not so fully attested. However, the line is transplanted from Σ [512], cf. Leaf's note—a bit of concordance interpolation that could have been made independently by any scribe. Chantraine (362-3) includes it in his quotation of X 111-25, noting that we have 'une succession de propositions coordonnées, toutes au subjonctif, jusqu'au vers 121'. That verse has an indicative; and there was something to say of its defective attestation.

Large variations—zero vs. one or more lines—that antedate Aristarchus are known to us almost entirely through the scholia. These are rarely mentioned by Chantraine. Thumbing through his work has brought me only notices of atheteses: (29) of Θ [73-4], by Aristarchus; (146) of Π [669 = 679] by Zenodotus; (268-9) of Η [353] by Aristarchus; and a completely unimportant slip of memory. The procedure is not open to criticism, for it is, at bottom, a question of definition. If 'Homer' means Aristarchus, or the text that started c. 150 B.C. as the lines of Aristarchus contaminated (in the technical sense) with variant wordings of unknown origin, and has reached us through later papyri, manuscripts, Wolf, Leaf, and Ludwich, statements about these large variations have no place in a Homeric syntax.

However, they may suggest at times the possibility of getting past a 2nd-

century Homer. Take, for instance, T [94] βλάπτουσ' ἀνθρώπους· κατὰ δ' οὖν ἕτερόν γ' ἐπίδησεν. Wackernagel, *Synt.* 2.173-4, mentions as frequent in Herodotus a sequence preposition + ὦν + an aorist, noting that the ὦν is apparently meaningless, and that, with a single exception, the aorist is gnomic 'und zwar meistens so, dass er einen Brauch bezeichnet'. He claims (cf. Schwyzler-Debrunner 283) T [94] as the oldest example, tho he notes the formal differences: οὖν does not follow the preposition, and is not followed by the verb. I may add that a sequence preposition + οὖν + verb is not found elsewhere in the Iliad, while Denniston, *The Greek particles* 460 ff., declares that δ'οὖν, common in Attic, is not found elsewhere in Homer. The line must have been made by a speaker of Attic; it is evidently not a part of the organism to which it is found more or less loosely attached. No direct evidence about the existence of the line before Aristarchus has reached us. Everybody infers that he had one or more manuscripts with the line from the fact of its presence in his edition; a number have inferred from the fact that he placed an obelos in front of it that he also had one or more manuscripts without it. One can go on from there. Chantraine says (185): 'La proposition à l'aoriste peut être soulignée par δ'οὖν: T 93 sqq.' This is in keeping with the traditional method, but I should have preferred either more or less.

Difficulties of another sort, the ambiguity of certain forms—for instance, short-vowel subjunctive or future indicative?—are dealt with in a way that calls for no comment (cf. 209, 212, 225, etc.). The same is true of the passages in which a disagreement of other scholars is noted. Finally, as is well said (297), 'la syntaxe homérique ne se prête pas à des classements rigides'.

The urgent question is whether the method can be improved, perhaps even to such an extent that one might speak of a new method. Since the formation of the Linguistic Society in 1924 there has been developing in this country a new way of dealing with the observable facts of language. The history of the movement has been told: modestly by Bloomfield, *Twenty-one years of the Linguistic Society*, *Lg.* 22.1-3 (1946); with more detail by Hall, *American linguistics 1925-1950*, *Archivum linguisticum* 3.101-25 (1952), 4.1-16 (1953). The new way was the result of a lucky accident: the combination of Bloomfield's work in Tagalog and Algonquian with his knowledge of Pāṇini and of the development of Indo-European grammar. The basic description of the method is to be found in Bloomfield's *Language* (New York, 1933). Of this Hall says: 'In America [it] is generally considered the greatest single book on linguistics published in our century on either side of the Atlantic.' The method has been applied energetically in work on languages that can be heard from native informants. Its practitioners learn such languages rapidly, write descriptions of them that are clearer and better than our traditional textbooks, and teach them with amazing efficiency. Can the experience thus gained be applied to languages known only from written records? Efforts have been made, but on a much smaller scale. Penzl well says, *Lg.* 29.97 (1953): 'the application of rigorous structural techniques to historical material is, even in this country, still in its infancy.' Nevertheless it seems our best hope. I have been experimenting, and am convinced that the task is, if possible, enormous and extremely difficult.

One will have to start by making descriptions of a new kind. These will be dull reading and fail to attract some whose cooperation is desirable. Men beginning their studies now will probably never see a better Homeric syntax than Chantraine's.

Under these circumstances, I shall discuss but a single passage; partly because I am interested in it, partly because it may lead on to other matters of interest.

Sthenelos, speaking to Diomedes, says:

ἀλλ' ἄγε δὴ χαζώμεθ' ἐφ' ἵππων, μηδὲ μοι οὕτω
θῦνε διὰ προμάχων, μή πως φίλον ἦτορ ὀλέσσης E 250

Leaf compares with this the speech of Idaios to Priam:

ἀλλ' ἄγε δὴ φεύγωμεν ἐφ' ἵππων, ἣ μιν ἔπειτα
γούνων ἀψάμενοι λιτανεύσομεν, αἱ κ' ἐλεήση Ω 357

and concludes that the construction must be the same. Chantraine (107 n. 34) follows him.

To judge by the context, performers of the act termed *φεύγειν* are in the Iliad sometimes on foot, sometimes in a chariot. The context of Ω 356 shows clearly that Idaios' suggestion amounts to: 'Let's make a get-away on chariot'. In sharp contrast mortal performers of (*ἀνα*)*χάζεσθαι* act in the Iliad always on foot. Clear examples are Γ 32, Δ 53, E 440, 443, 600, 626, 702, 822, H 264, Λ 461, 504, 585, M 172, 262, 407, N 148, 153, 165, 193, 533, 566, 596, 648, 740, Ξ 408, O 426, 728, Π 122, 707, 710, 817, P 13, 47, 108, 129, Σ 160, Φ 403. In E 34 it is not clear just what act Athena suggests to Ares by *νῶι δὲ χαζώμεσθα*, but they are gods. Δ 539 has been interpreted differently, but see below. In Π 736 *ἄξετο* (cf. Leaf) is clearly to be preferred, though *χάζετο* would not trouble what is here said. Nor would *κεκάδοντο* Δ 497, O 574, if with Leaf and Schwyzer 748 we believe it pertinent. Sthenelos' suggestion then must be: 'Let us [with foot on the ground] fall back on the chariot'. What is designated by the prepositional phrase—the space relation between the actors and the chariots—is clearly different.

The only difficulty is that this, if taken literally, is not what Sthenelos is saying. Unless one wishes to make the very improbable assumption that he has left his chariot and walked over to talk with Diomedes, 'Fall back on the chariot!' is what he means. However, as pointed out by Wackernagel, *Synt.* 1.42–3, it has been recognized since Jacob Grimm (1819) that forms of the 1st person plural are often found where the 2d person singular might be expected. Grimm tells of a teacher who said 'Wir sind ein Esel' to a schoolboy; Persius of another who exclaimed *stertimus* on finding a lazy pupil asleep at sunrise. Another famous example is the question *ἦ ρ' ἐχομέν τι*; put by 'Homer' to the fishermen. Wackernagel quotes Sophocles, Plato, and from the Iliad the formula *ὡς ἂν ἐγὼ εἴπω πειθώμεθα πάντες* and Θ 234, Λ 313, O 553, Σ 273. It is language of this type that Sthenelos is using.

My interpretation is in agreement with the opinion of Aristarchus, reported quite fully in the scholia. According to Aristonicus in sA, the line was marked

(there is a diple, not an obelos, in cod. A) *ὅτι Ἀττικῶς ἐξηγήνοχεν ἀντὶ τοῦ ὡς ἐπὶ τοὺς ἵππους*. Didymus in a preceding note gives examples that point to a belief in Attic coloring: *οὕτως Ἀρίσταρχος ἐφ' ἵππων, ὡς εἰ ἔλεγεν ἐπ' Ἀθηναίων ἀντὶ τοῦ ἐπ' Ἀθήνας*. This might suggest that we are here confronted by an Atticism of the poet. Decisively against this is the occurrence of the construction in Γ 5, E 700, Λ 546, Ψ 374.

Twice the scholia show the same feeling towards the construction.

πέτονται ἐπ' Ὀκεανοῖο ῥοάων (Γ 5),

ἀντὶ τοῦ εἰς ῥοάς Ἀττικῶς sB—perhaps from Didymus.

οὔτε ποτε προτρέποντο μελαινάων ἐπὶ νηῶν E 700

Aristonicus (sA) says: *ἡ δὲ διπλὴ πρὸς τὸ σημαινόμενον, ὅτι Ἀττικῶς ἐξηγήνοχεν, οὐκ ἔφευγον προτροπάδην ἐπὶ τὰς ναῦς*. This is imbedded in a note by Didymus: *οὕτως Ἀρίσταρχος ἀμφοτέρω διὰ τοῦ ἔ, προτρέποντο καὶ ἐπὶ νηῶν. λέγει γὰρ οὔτε προτροπάδην ἔφευγον ἐπὶ τὰς ναῦς. ἔνιοι δὲ ἀγνοοῦντες γράφουσιν ἀπὸ νηῶν. γίνεται δὲ ἀδιανόητον οὐ γὰρ ἀπὸ τῶν νεῶν φεύγειν ἔμελλον*.

This combination of the Ἀττικῶς description with the record of variant readings suggests a shortening of the scholia to the other passages.

τρέσσε δὲ παπτήνας ἐφ' ὀμίλου Λ 546

Ἀριστοφάνης δι' ὀμίλου sA^tT, Didymus.

πύματον τέλεον δρομον ὠκέες ἵπποι
ἂψ ἐφ' ἁλὸς πολιῆς Ψ 374

οὕτως Ἀρίσταρχος· αἱ δὲ πλείους ἀφ' ἁλὸς· καὶ μήποτε λόγον ἔχει sA^tT Didymus.

The construction—contrast T [94] treated above—is rooted in the *Iliad* so deeply that it is impossible to ascribe its occurrence to a rehandling of the poem by some speaker of Attic. We must simply recognize that in this feature epic and Attic usage coincide. The fact roused the interest of Aristarchus; probably because of his belief, cf. Cauer³ 111, that Homer was an Athenian by birth.

In conclusion it may be noted that sA at E 249 continues after the above quotation from Aristonicus: *δοκεῖ δὲ Ζηνόδοτος τοῦτον καὶ τὸν ἐξῆς ἡθετηκέναι*. This is clearly a guess, to which not much importance can be assigned; cf. *Ath. Lines* 95.

In fulfillment of my promise (above) to examine Λ 539, I must take up the section 531–41, in the composition of which lies the key to the interpretation of *μίνυνθα δὲ χάζετο δουρός*. It begins after a speech of Kebriones with splendid verses in language of the earliest type:

ὥς ἄρα φωνήσας ἵμασεν καλλίτριχας ἵππους
μάστιγι λιγυρῇ· τοὶ δὲ πληγῆς αἰόντες
ρίμφ' ἔφερον θοὸν ἄρμα μετὰ Τρώας καὶ Ἀχαιοῖς,
στείβοντες νέκυνς τε καὶ ἀσπίδας· αἵματι δ' ἄξων
535 *νέρθεν ἄπας πεπάλακτο καὶ ἄντυγες*

The end of this was taken over by a later poet (dated by the dissyllabic ὀπλέων) for the building of a simile (T 495-503) from oxen treading out the grain. Still later (cf. Robert, *Studien zur Ilias* 106) Π or a slightly earlier predecessor used the simile to continue this passage:

αἶ περι δίφρον,
 ἄς ἄρ' ἀφ' ἱππέων ὀπλέων ῥαθάμιγγες ἔβαλλον
 αἶ τ' ἀπ' ἐπισσώτρων. ὁ δὲ ἔετο ...

The simile ends with κῶδος ἀρέσθαι, but it has furnished the shift from Kebriones to Hector, commented on by Von der Mühl, *Kritisches Hypomnema zur Ilias* 197 (1952). Thrown on his own resources, but perhaps remembering Π 729-30, the improver of Δ brings Hector down to earth, and continues:

δύναι δμῖλον
 ἀνδρόμεον (!) ῥῆξαι τε μετάλμενος· ἐν δὲ κυδοιμόν
 ἦκε κακὸν Δαναοῖσι, μίνυνθα δὲ χάζετο δουρός.

Then he winds up with Δ 264-5:

540 αὐτὰρ ὁ τῶν ἄλλων ἐπεπωλείτο στίχας ἀνδρῶν
 ἔγχετ' ἑορι τε μεγάλοισι τε χερμαδίοισιν.

'Von dem plötzlich als abgesprungen gedachten Hektor' says Von der Mühl a little belatedly.

Leaf's note shows that he was thinking of Hector as driving along the line of battle, and reaches no satisfactory explanation of μίνυνθα δὲ χάζετο δουρός. But with Hector on foot (note μετάλμενος) the meaning seems to be: 'he threw panic into the Danaans, for little would he draw back from any spear.'

Les grecs de Cargèse (Corse): Recherches sur leur langue et sur leur histoire.

By GÉRARD BLANKEN Vol. 1, Partie linguistique, pp. xix, 332. Leyde: A. W. Sijthoff's Uitgeversmaatschappij N.V., 1951.

Reviewed by GORDON M. MESSING, *Washington, D. C.*

The Greek dialect of Cargèse in Corsica, though in articulo mortis, still lingers on after a history of almost 280 years. This account of it is certainly one of the most solid and profitable studies yet to appear in the modern Greek field. Blanken, a disciple of Hesseling and Pernot, is thoroughly at home in the Greek dialects and in medieval Greek. His material, so far as it is based on the spoken language, utilizes the results of three field trips in 1932, 1933, and 1934; it would be instructive to know whether twenty eventful years and a second world war have administered the coup de grâce to this already moribund speech. In addition to taking down all he could collect from the lips of the 'quelques dizaines de grecophones' who have remained true to their native tongue, Blanken has carefully extracted and evaluated all previous descriptions. His three main predecessors are: Stefano Stefanopoli, a priest of Cargèse, who in 1860 translated the parable of the Prodigal Son into this dialect; N. B. Phardys, a physician and professor of Greek in Cargèse who described the dialect in a publica-

tion of 1888; and the well-known British scholar, Richard M. Dawkins, who visited Cargèse in 1926 and contributed a brief but meaty article the following year to *Byzantinisch-neugriechische Jahrbücher* (5.371-9). Besides exploiting the strictly linguistic materials, Blanken has gone through copious sources dealing with the original establishment by the Genoese Republic of a Maniot Greek colony at Paomia (Corsica) in 1676 and its subsequent vicissitudes. Much of the earlier material derives from unpublished documents in the state archives in Genoa. In a forthcoming second volume, Blanken will deal with the variegated history of the colony from its inception to the present day. (It is unfortunate from a linguistic standpoint that the first volume has no index verborum; this is to be included. much less appropriately as it seems, with the second volume.)

While the dialect of Cargèse is intriguing in itself, Blanken has also touched on the general questions arising out of his subject, and his results are pertinent to many types of linguistic situations. For example, the Greek speakers who came to Corsica to escape Turkish occupation were predominantly from Mani in the southern Peloponnesus; luckily, thanks to the work of André Mirambel, Maniot is a relatively well-known Greek dialect. Yet on examination it turns out that many of the most characteristic features of Maniot are not continued in the Cargèse dialect, which in some respects is closer to standard Greek and in other respects much further evolved. The explanation for the first of these two conflicting phenomena seems to be that in such a small and relatively isolated linguistic community even a few new arrivals from other parts of Greece exerted disproportionate influence; the teaching of standard Greek, particularly the literary *katharevusa*, had a normative effect on the dialect even within the time span covered by linguistic texts. For example, in Maniot the groups *-ρθ-* and *-ρχ-* yield *-ρτ-* and *-ρκ-*: Stefanopoli lists *ἦρτε* 'came' for standard *ἦρθε*, and the 17th-century documents of Cargèse contain such forms as *ἐρχόμενοι* 'coming' for standard *ἐρχόμενοι*; yet Blanken's informants employed *ἦρθε* exclusively (28-9). On the other hand, contradictory as it may appear, some of the lexical oddities and curious innovations of the dialect are also due to the restricted size of the community. As Blanken explains (30), the dialect has become a sort of private family language; hence joke-words, errors, and infantilisms manage to survive by escaping correction. The general linguistic situation in Cargèse must be kept in mind. All the Greek speakers are trilingual. En famille, they speak but do not write their variety of Greek. They speak but do not write the Corsican dialect of Italian in intercourse with their non-Greek-speaking neighbors. Finally, like all Corsicans, they speak and write French, the official language learned in school and the only real written language of Corsica. The influence of French on the Greek dialect of Cargèse is minimal, and is exerted through the medium of Corsican. Corsican itself has left indelible traces on the vocabulary of the Greek speakers and some day may displace the dialect completely. The process of dissolution has gone on very gradually; numerous visitors to the colony during the 19th century were already predicting the inevitable outcome. Another century has found it still threatened but still defying inexorable fatum. Its surprising persistence can be attributed (35) to an 'earnest desire to preserve the ancestral language'.

Blanken has not chosen to analyze the dialect of Cargèse in isolation; his study presumes a knowledge of standard Greek and its other dialects, with which features of this dialect are constantly compared either explicitly or by inference. In other words, Blanken's study, though grounded in adequate descriptive data, is primarily comparative and to some degree historical. The reviewer heartily endorses Blanken's method. Many of the most significant features of the dialect can thereby be seen in perspective, as they should be seen. It is true that he has perforce slurred over such descriptive details as stresses, intonational patterns, and juncture.

From a phonetic standpoint, the dialect of Cargèse shows the following main divergences from standard Greek: widespread palatalization, instability of final *-s*, loss of nasal in certain combinations of nasal and stop, and closing of accented *e* and *o* vowels.

1. The palatalization, occurring before a front vowel, may be schematized as follows: palatalized $\gamma > \check{z}$ (Blanken's *j*), $\kappa > t\check{s}$ (*č*), $\chi > \check{s}$ (*ch*), $\gamma\kappa > d\check{z}$ (*ǵ*), $\sigma\kappa, \sigma\chi > \check{s}\check{s}$ (*sch*) with *s* palatalized before *š*. Examples (with standard Greek in parentheses): *ža* (*γιά*) 'for', *tšɛ* (*καί*) 'and', *éši* (*έχει*) 'has', *stɔ džiɾo* (*στον κήπο*) 'in the garden', *áššimɔs* (*άσχημος*) 'ugly'. Note that *ν* and *λ* are also palatalized in this dialect. Some of these traits are paralleled in other Greek dialects, notably Maniot; but this dialect is apparently unique in the scale and consistency of its palatalizations.

2. Final *-s* is unstable, as in Maniot and other Greek dialects. Examples: *éne aéra* (*είναι άέρας*) 'there is a breeze', *ɔ φίλɔ* (*ὁ φίλος*) 'the friend' beside *ɔ φίλɔs*.

3. A nasal is lost before an occlusive, both within a word and in liaison between words, with voicing of the occlusive. Examples: *dhedrɔ* (*δένδρο*) 'tree' with loss of the final nasal as in standard spoken Greek, *dhe brépi* (*δέν πρέπει*) 'is not necessary'.

4. Under the accent, the vowels *ɛ* and *ɔ* are closed to *e* and *o*. It is Blanken's merit to have been the first to isolate this capital and unusual innovation, which even Dawkins had overlooked.

In its morphology, the dialect of Cargèse shows a number of interesting features, far too many to list here. The loss of *-s* has strengthened a tendency toward indeclinability in the noun while at the same time fostering a confusion of masculine and neuter genders. There are several novel forms among the demonstratives; the medio-passive endings show divergences from standard Greek; the future tense is formed with *éxw ná* (or simply *ná*) rather than *θά*.

Blanken considers that the dialect of Cargèse offers little novelty in its syntax. Its vocabulary, however, is another matter. He noted about 1500 words in current use (181), of which about 650 are standard Greek with the predictable phonetic modifications; a list of these 650 words is given (182-5). For illustrative purposes Blanken adds a selective list (186-7) of some 125 words of standard Greek which fail to occur in the dialect. These include such everyday items as *éðw* 'here' (Carg. *padá*); *patépas* 'father' (Carg. *afédi* < standard *ápértis* 'master'); *ɳ* 'or' (Carg. *ɔ*, from Corsican).

Another more detailed list (189-207) covers standard Greek words attested in the dialect of Cargèse but in a different sense. There are many varieties of

semantic change to be seen here, including both specialization of meaning (standard Gr. *γενήματα* 'cereals' > Carg. *ženima* 'barley') and generalization of meaning (standard Gr. *λωβός* 'leprous' > Carg. *lvó* 'bad'). A typical example is Gk. *χαρτί* 'paper', which retains this meaning in the dialect but has also come to mean 'book' (Gk. *βιβλίω* with the semantic shift seen in the development of Rum. *carte* 'book' from Lat. *charta* 'paper'). (Blanken suggests that the new signification may reflect a scarcity of books among Greek speakers in Cargèse. That this is not an inevitable conclusion, however, appears from American English, where 'paper' often means 'newspaper', and 'book' on some levels usually means 'magazine'.)

There follows (208–53) a fascinating list of 'mots remarquables', swarming with odd specimens. Some have no known antecedents: *lemēdera* 'earthworm', *kápidō* 'extreme cold', *smurđu* vague term of reproach used in scolding a child. In classifying these exotics and seeking out parallels for them in other Greek dialects, Blanken has acquitted himself splendidly. One might single out a very few examples for amplification: *sepsi* 'pipe' (Gk. *πίπα*) was explained by some of Blanken's informants as a North African Arabic loanword; Blanken presents this hypothesis with reservations, but this is in fact the case. For *skorína* 'crust' Blanken finds dialect words *κοριά*, *κορέα* in the same meaning (explained as yielding **koría*, whence with prothetic *s* a basic form **skoría*). Is it possible that this word is a blend of the forms cited by Blanken with *σκωρία*, *σκουριά* 'rust' (with a transition to the meaning 'outside layer')? The word *strinó* 'I run' is of unknown origin according to Blanken; one might hazard a guess that the verb may be related to Albanian (Tosk) *shtrinj* 'run', listed in Angelo Leotti, *Dizionario albanese-italiano*. Apparently, the word is not to be found in Stuart E. Mann, *An historical Albanian-English dictionary*; perhaps it is identical with *shtrinj* 'stretch, extend', used with colloquial force.

A section on words borrowed from the Corsican dialect of Italian follows (254–63). We learn that *abastandza* has replaced Gk. *ἀπερά* 'enough'; *speráro* rivals Gk. *ἐλπίζω* in the sense 'I hope'; and such hybrids as *ruvindró* 'I ruin', *ariváro* 'I arrive', and *múzika* 'music' suggest the manner in which the dialect will ultimately be displaced.

A selection of texts, linguistic and historical, rounds off the book (277–317). Only one of them (277–80) is an actual transcript of current speech, the rest are literary. Considering the moribund state of the Cargèse dialect, one would have been grateful for more extensive samplings of the speech of Blanken's informants. Nevertheless, the texts chosen pursue the dialect as far back as it can be traced. Not unnaturally, the literary texts have been extensively normalized while still retaining features of interest for an alert investigator. It is rewarding, for example, to light upon *γραφή* used in the sense of *γράμμα* just as in the current dialect of Cargèse, although it appears in a letter of 1665 from a member of the original Maniot community.

Blanken's book is a most significant contribution to the study of the modern Greek dialects. It is a pleasure to pay tribute to his thorough and scholarly treatment.

Studien zur Geschichte des Infinitivs im Griechischen. By PENTTI AALTO. (Annales Academiae Scientiarum Fennicae, Ser. B, Vol. 80.2.) Pp. 116. Helsinki: [Suomalainen Tiedekatemia], 1953.

Reviewed by JOSHUA WHATMOUGH, *Harvard University*

The only attempt to revise traditional grammar in logical terms, in a comprehensive way, is, so far as I know, that of Reichenbach. His discussion has nothing to say about morphology. But there is nothing new to be said about the morphology of the infinitive in Greek from any point of view. We know about the great diversity of forms, especially when documents other than Attic are considered, how they defeat systematization, historical explanation, comparison with the forms of other Indo-European languages except in detail, and any attempt to find Indo-European sources. Statistics (for Greek and Latin) have been available since 1934, in two articles not cited by Aalto. Syntax, especially descriptive, is handled fully in Schwyzler's grammar (Vol. 3, 1950). One would like to see anything further published on a subject already so abundantly discussed take a new direction.

Why did the infinitive in Greek develop into a neuter noun with $\tau\acute{o}$, capable of declension and of construction with a preposition? How did it come about that distinctions of voice and tense were then permitted? Why did the genitive of the articular infinitive predominate? Why just when it did? And in the authors that it did?

Aalto has added to our previous knowledge, descriptively speaking, in later Greek, conspicuously of the Septuagint and papyri; and he has drawn more freely on other Indo-European and non-Indo-European languages than his predecessors. He has also traced the replacement of the substantival infinitive by abstract nouns in modern Greek. This is a curious peripeteia, that makes its enormous prevalence in the classical period all the more strange in a language as rich in abstract noun formations as Greek.

Perhaps the merit of what Reichenbach calls event-splitting, or of the advantage of using correlates (active : passive) had something to do with it. Whatever the explanation, Aalto has not sought it, but has contented himself with description and some rough enumerations, not true statistics. Some of Reichenbach's examples (*Symbolic logic* 1947) and their solutions (§48, §49) help to show the function of the articular infinitive more clearly than much of the grammarians' discussions.

Le livre des deux mille phrases: I. La méthode des dictionnaires de phrases; II. Questionnaire de deux mille phrases selon le parler d'un Parisien. By HENRI FREI. (Société de Publications romanes et françaises, No. 40.) Genève: Librairie Droz, 1953.

Reviewed by JOSHUA WHATMOUGH, *Harvard University*

Dictionaries of words, dictionaries of phrases, dictionaries of concepts—they swell like the waves of ocean, their ingredients as multitudinous as the sand, and varied without measure or number. It was Bloomfield's great argument

against meaning that we cannot know it. Yet it is not so. Frei reminds us, and it was high time, that 'la langue constitue une "forme," c'est à dire un équilibre d'éléments non-substantiels', and therefore that 'la connaissance de chaque pièce implique celle de tout le reste et il suffira donc de matériaux en nombre limité.' The system is to be found in each particle of sand, each drop of water. Moreover at some point the system 'constitue un tout fermé'. It is difficult, at any given moment, to determine this point. But the whole elaborate structure of linguistic analysis rests upon this very assumption that a language is a status, a closed system. Without this assumption the description of a language, as practiced by structuralists, would be unobtainable; it would not be possible, without first waiting to hear it, to state any utterance ('state' is the correct term, not 'predict', since contemporary description is the undertaking, not foretelling the course of linguistic events). Moreover, a linguistic system is metastable. Descriptive linguistics invokes yet another fiction, namely that it is a stable system, but this seems unnecessary as well as beyond reason.

Frei has seen the absurdity of compiling, after twenty-nine years of effort, an analysis of French based on an inventory of over 34,000 utterances made by over 850 persons, the very walk of life of over 140 of whom is unknown—the seven volumes of Damourette and Pichon (Paris, 1927-). The remedy, he rightly insists, is to establish a limited ensemble covering the normal linguistic activity of one and the same person, and thus to obtain homogeneous materials, limited but complete in themselves. This gives the language (*la langue*).

Frei has compiled such an ensemble of 2000 items, each illustrated by a current phrase. Responses to this questionnaire have been obtained from a young Parisian, and also (but not yet published) from an English woman (London), a Chinese (Peiping), a Japanese (Tokyo), and a speaker of German (Allemanic, Basel). They are spontaneous, and in many ways more satisfying than responses consisting of single words evoked by a questionnaire that is a list of single words. They are not translations.

For example, in section 6 (*Boire et manger*) item 45 is *Appétit* (*symptôme de maladie*), *Il a peu d'appétit*. *Appetite* is a word of rare occurrence, and the English response turns out to be *He's off his food*; in 5 (*Bouche*) at item 39 *Souffler*, *Je vais souffler sur le thé pour le refroidir*, it was *Bad manners*.

Frei calls attention to the advantage of using homogeneous materials for statistical purposes. We get a grammar in which each important factor in the system is cited with its coefficient of usage. He seems to be in error, however, in supposing that subject matter affects the total vocabulary at risk. This principle has been rejected by Yule with good reason.

Frei's list of 2000 items has been carefully prepared. It is likely to be very useful to others; and his succinct introduction is well worth the thirty minutes or so in which it can be read.

Gestirnnamen bei den indogermanischen Völkern. By ANTON SCHERER. (Indogermanische Bibliothek: 3. Reihe, Untersuchungen: Forschungen zum Wortschatz der indogermanischen Sprachen, No. 1.) Pp. 276, plates I-VIII. Heidelberg: Carl Winter, Universitätsverlag, 1953.

Reviewed by ERNST PULGRAM, *University of Michigan*

This work on the Indo-European names of celestial bodies was originally presented in 1947 as the author's Habilitationsschrift at the University of Munich, but its publication was delayed. Scherer has assembled here an enormous amount of detailed lexical, onomastic, mythological, astronomical, astrological, and historical information. It lies in the nature of the subject that in gathering the evidence he had to go outside the area of the Indo-European languages and cultures, especially to Egypt and Babylonia, where astronomical studies flourished, though primarily with a religious and astrological intent, and whence they influenced the world of Indo-European speech. In view of the areas of knowledge and the geographic regions with which the book concerns itself, it will be obvious that its usefulness also transcends the boundaries of linguistics and philology, to say nothing of onomastics, though the title indicates the kernel and the goal of work.

As I read through the book, I was reminded of Buck's dictionary of Indo-European synonyms, for Scherer's treatment of the names of stars is constructed on the same plan: it is a lexical study, with great emphasis on etymology, but arranged and ordered according to semantic criteria and a classification of realia. It could, in fact, be either a detailed elaboration of Buck's headings dealing with heavenly bodies, or the basic research underlying a condensation like Buck's. The most important difference is, of course, that Buck is interested in common nouns and Scherer in proper names, although it is evident from Scherer, if the reader did not know it before, that all proper names originate from common nouns. This is true of the names of the stars, planets, and constellations as much as of personal names.

Scherer finds, however, that the names he treats are less stable than others, and he gives good reasons for such onomastic vacillation. At the base of it probably lies the fact that names of stars of all kinds are subject to revision and reconstruction according to the culture and lore of each society, and to translations, more or less felicitous, from one language to another—processes to which personal names are generally not exposed, or only very rarely. It also becomes evident from Scherer's treatment that the naming depends a great deal on the social or professional groups that perform it. Some names are due to hunters, farmers, or seafarers, others to astronomers, astrologers, priests, or poets. It is therefore not surprising that etymological equations based on numerous occurrences and pointing unambiguously to a common Indo-European root are relatively hard to set up. There is too much extra-linguistic interference, it seems, from all kinds of pressure centers, from astronomy and religion down to popular etymology. Moreover, one can not know just how far back each name goes—that is, how many of the celestial phenomena which had names in the various languages and cultures discussed, actually existed as named entities in

the consciousness of the speakers of Proto-Indo-European—whoever they were, wherever they lived, and whatever their number. The distinction that always needs to be made between COMMON Indo-European and PROTO-Indo-European here becomes strikingly clear.

At the end of the book there are sufficient indexes (240–76) to make it a useful reference work. This, primarily, is what it is, rather than a book for reading.

And this brings me to mention a desideratum. It lies outside the scope of the present volume; but Scherer is fully aware of it, and will, I hope, fill the gap himself, since no one is better equipped to do it. Any collection and classification of materials, lexical or onomastic or other, can fulfill an ulterior and, if I may say so, higher purpose, if it is made to illuminate the history and the ways of man. Having gathered laboriously and diligently the ingredients in all their exuberance, and having presented them to his readers systematically and admirably, Scherer should now cook from them a dish both nourishing and savory. This further labor is not only desirable for reasons of gastronomy; it also facilitates the digestion of the ingredients.

Reflections on the numerals 'one' and 'two' in the ancient Indo-European languages. By J. GONDA. Pp. 80, offset. Utrecht: A. Oosthoek, 1953.

Reviewed by J. ALEXANDER KERNS, *New York University*

The distinguished Sanskritist who has given us this monograph concerns himself primarily with the pre-semantics of the lower numerals and other words associated with or suggested by them. Since much of what he has to say falls in the fields of general linguistics and social anthropology, he does not too narrowly limit himself to the IE languages, though his collections of non-IE material are admittedly derivative. Considerations of IE morphology take second place, though specialists in that field will find some matters of considerable interest. Etymology is well represented; in particular, where semantic considerations (as hitherto understood) have stood in the way of a phonologically acceptable etymology, Gonda's pre-semantics often obviates the difficulty rather neatly.

Of the four chapters, the first (1–28) is concerned with the implications of complementariness and collectivity which Gonda finds in the lower numerals, particularly 'two', and also in the dual as a grammatical category, which 'has tended to disappear ... when a definite mental or cultural structure was given up' (11). Much of this chapter is devoted to a study of occurrences of the dual in various syntactic and phrasal structures (such as the absence or presence of the actual numeral 'two') in Greek and Sanskrit, carefully documented and of course chronologically presented. In some 'additional remarks' in the closing pages he notes that the word order, 'both' plus demonstrative plus noun (Skt. *ubhā ... ētē savanē*, Gk. *amphōin ... toin diathékain*, Goth. *ba þō skipa*) is rather favored, and differs from that of a group containing 'two' (26). He concludes with the modest claim that in the evidence surveyed there is nothing to contradict the assumption 'that a duality was ... considered a two-sided entity or unity' (28).

Chapter 2 (29-48) deals with words for 'half': 'when ... an entirety constitutes a duality, when it has ... two components that belong together, then one ... half of it is not identical with our modern ... integer "one" Only a certain familiarity with this mode of thought makes it intelligible that "one" and "half" should have been expressed by the same term, the single object being half of the "dualité unité" ' (29). On such grounds as these Gonda supports Persson's etymological connection of **sēmi*- 'half' (though strangely troubled by its ē, 36 fn. 32) with **sem*- 'one' (p. 35), as also Johannes Schmidt's of Goth. *sundrō* with Gk. *háteros* (34), both contrary to Walde-Pokorny and others.

The first half of Chapter 3 (49-55) amasses linguistic evidence from many families in support of the social anthropological theorem that 'natural man, so far as rationalistic tendencies and intellectual or "modern" onesidedness do not dominate, thinks and acts collectively ...; such concepts as "a single individual" are not the same in primitive society as with us' (49); the typical 'primitive' does not see the trees for the wood. Consideration of this topic leads into the second half of the chapter (56-61), devoted to an analysis of the pre-semantics of words for 'other, another', which earlier would have meant 'naturally associated'. On such grounds Gonda can acquit of any contemporary lack of logic such expressions as Skt. *ētāir anyāis ca ... rājaputrāiḥ* (in which the first-mentioned 'these' are not kings' sons), Hom. Gk. *mētēr d' emé ou ti pépustai, oud' állai dmōat*, Lat. *circa moenia aliāsque portās*, or Early Mod. Engl. *both preestis and othere laymen*. An old-fashioned way of saying somewhat the same thing would be that the last noun in such structures is not modified by 'other', but is in apposition to it, although this would do less than justice to the earlier meaning of 'other'. Next comes a detailed study of Skt. *anya*-, showing that it never occurs in the R̥gveda in such structures as 'murderers and other criminals in which both categories are species of the same genus, the former being explicitly designated' (59), though this type of structure is common in the brāhmaṇas (60), and that the ablative commonly used with *anya*- (and various virtual synonyms) is properly one of separation, not comparison, thereby vindicating Pāṇini against Speyer and 'other contemporaries', we might say.

The first two sections of Chapter 4 (68-72) deal with **sem*- 'together' and its various derivatives. First the rather good point is made that the intensive and perfective force of Skt. *sam*- is not a later development 'after the original function had faded', but rather that all its functions were implicitly involved from prehistoric times. There follows a trenchant criticism of the current etymologies of Lat. *semper*: to interpret it as 'une fois pour toutes > toujours, in einem fort > immer' is to read into its first element an abstract and specific numeral meaning that had not yet arisen when the Latin word (or its underlying structure) was formed: it is 'completely, connectedly > without interruption > always' (phrasing partly mine); we may understand OGmc. *sin*-compounds similarly (71).

The last section of Chapter 4 (73-80) begins with a technical discussion of certain functional complexities in the Latin numeral system: *ternī* and *trīnī* are

not functionally distributed as are *duo* and *bīnī*; Gonda finds a partial explanation in Walde-Hofmann's suggestion that *ternī* has been reinterpreted as a distributive because of its assonance with *alternī* (74). The suffix *-no-* had collective force; even **oino-* is in some sense a collective: he who in his loneliness is at the same time in his entirety is 'one'. The specific numeral meaning 'one' has arisen out of an older meaning 'lonesome, alone', not the other way round (80). The book closes with the customary disclaimer of 'glottogonic speculation'.

Downright errors, even purely mechanical, are few. Chukchee is not 'an American Indian language' (53), though Bogoras' grammar was included in Boas' *Handbook of American Indian languages, Part 2* (1922). In the citation of Euripides, *Bacch.* 918, *drân* must be corrected to *horân*. Fn. 52 (16), valuably correcting a 'slip of the pen' on the part of Schwyzer-Debrunner, seems to involve one on the author's part in quoting the German original with Dutch *met* for *mīt*. Fn. 59 (40) has dropped out; it should probably refer to Walde-Hofmann, *Lat. etym. Wb.*³ 382. — Gonda's style on the whole is excellent; only rarely is the non-native-speaker revealed. At times he is over-explicit or over-deferential to possible opponents, even to the point of mildly exasperating the reader.

Probably most readers will concede that specific numeral concepts, at once abstract and sharply arithmetical, must be a relatively recent development in men's thinking, and that hence the words used for them in times linguistically recorded are likely to have had earlier meanings less sharply arithmetical; but it may be that even some recent etymological work on the numerals has not taken adequate cognizance of this fact. If so, Gonda's treatment may improve matters.

The Nalarāyadavadanticarita (Adventures of King Nala and Davadanti):

A work in Old Gujarātī. Edited and translated by ERNEST BENDER. (Transactions of the American Philosophical Society, NS Vol. 40, Part 4, [1950], pp. 265-372.) Pp. 108. Philadelphia: American Philosophical Society, 1951.

Reviewed by SAMUEL D. ATKINS, *Princeton University*

This monograph comprises an edition and translation, accompanied by a grammatical analysis and glossary, of an Old Gujarātī version (15th century A.D.) of the Jain parallel to the well-known story of Nala and Damayanti as related in the Mahābhārata. Old Gujarātī occurs on the approximate chronological borderline between Late Middle and New Indo-Aryan, and might be termed an Early Modern dialect. There has been a comparative paucity, in this field, of printed texts, not to mention soundly edited ones, and translations thereof. In addition to the material published by L. P. Tessitori¹, the chief aids widely available for study of the language have been those published by T. N.

¹ Notes on the grammar of the Old Western Rājasthānī, with special reference to Apabhraṃśa and to Gujarātī and Mārwāḍī, *Indian antiquary*, Vols. 43-5 (1914-16).

Dave,² by W. N. Brown,³ and by K. B. Vyas.⁴ Bender's study, a work of admirable scholarship, is a welcome addition to these, and is an important and valuable contribution, in terms both of method and content, to New Indo-Aryan philology and linguistics.

The book is divided into six main sections: I. Introduction (267-70); II. Grammar (271-307); III. Text and notes (308-29); IV. Translation (330-41); V. Glossary (342-70); VI. Bibliographical references (371-2). The introduction has three subdivisions. The first treats the source of the materials, identifying and describing concisely the four manuscripts which the author has employed to establish his text. The second, bearing the heading 'The story and its significance', compares and contrasts the narrative detail of the OG rendition with another Jain version and with the Brāhmanical 'Nala-episode' of the Mahābhārata. There is very little treatment of the 'significance' of the story, and that is cursory and on an elementary level. Perhaps 'significance' was too pretentious a word to use. The third subdivision contains a comparatively full account of eleven of the thirteen metrical stanzas used; these are quantitative (each line or half line having a fixed number of morae) and employ end-rhyme (the last two syllables of a line or half line rhyme with the last two of the next following line or half line). A caupai stanza (the most frequent type) will illustrate the verse (112):⁵

2 1 1 2 11 1 111 2 1	= 15 morae
nyāya-bhaṇī tau pahilau dūta	
2 2 21 1 2 11 2 1	= 15 morae
peṣī rāi jaṇāviu sūta	
1 1 1 1 2 11 2 2 2 1	= 15 morae
paṇi navi mānai te-ū saṃḍa	
1 1 11 2 11 1 1 1 1 2 1	= 15 morae
paḍahau vājai jima viṇa daṃḍa	

It is obvious that full understanding and control of the meters can be extremely important for phonological analysis. The linguist reader, therefore, will be well advised to note the author's properly cautious statements: 'The mechanisms of the meters have not been fully explored. It is felt that a final report must be postponed until other Old Gujarātī metrical texts of this period have been examined' (269b); 'This meter [drūpada], which occurs thirty-seven times, is not clear to me' (270a); and 'I have not been able to reach any conclusion on the remaining two meters, the ḍhāla and the ūlāḍḍhāla' (270b).

The second section, that on grammar, is subdivided into Phonology, Mor-

² *A study of the Gujarātī language in the 16th century* (V. S.) (James G. Forlong Fund, No. 14; London, 1935).

³ An Old Gujarātī text of the Kalaka story, *JAOS* 58.5-29 (1938).

⁴ *Vasanta Vilāsa, an Old Gujarātī phāgu* (Bombay, 1942); *Vasanta Vilāsa phāgu*—a further study, *Jour. Univ. Bombay* 15(2).119-52 (1946); *Vasanta Vilāsa*, the revised, collated text, 8(1-2).25-36 (1947). [The last two items are not available to the reviewer. They are cited as in Bender (372).]

⁵ 'To find out, the king first sent a messenger who made known [the state of affairs] properly, but that ox did not listen. How can one beat a drum without a stick?'

phology, Notes on syntax. Bender's descriptive treatment of the phonology is vastly superior to any previous endeavors, e.g. that of Dave's, which is historically oriented, confusingly organized, and of course without phonemic analysis. It must be realized, however, that Bender's statement is not a statement of OG phonology but an account of a single text, including Sanskrit loanwords and all manuscript variants, and that, therefore, as concerns Old Gujarātī, it is tentative and unfinished because the corpus is inadequate in size, at least in the reviewer's opinion. The list of phonemes contains only segmentals; there is nowhere a consideration of suprasegmental features. The author, choosing his words carefully, speaks (271a) of phonemes which 'are evident in the language of the manuscripts: of the vowels, *a ā i ī u ū r e o*; of the diphthongs, combinations of the vowels—with the exception of *e* and *r*—with the consonantal *ī* and of *a* with consonantal *u* ...⁶ and of the consonants, the gutturals, *k kh g gh ṅ*, the palatals, *c ch j jh ṇ*, the cerebrals, *ṭ ṭh ḍ ḍh ṇ*, the dentals, *t th d dh n*, the labials, *p ph b bh m*, the semivowels, *y r l v*, the sibilants, *ś ṣ s*, and the aspiration *h*.'⁷ The distributional patterns are treated briefly but as adequately as possible for environments whose accentual and junctural features remain undetermined. Orthographical variations are analyzed perceptively (not all of them definitively) with great care and detail. Some of the variations indicate that phonemic distinctions are not consistently represented in the manuscripts, e.g. anusvāra is used to represent the nasal component in the sequence nasalized vowel plus consonant (*mūṁdraḍī* 'ring') and also the nasal consonant in the sequence vowel plus nasal consonant plus consonant (*dava-daṁṭī* 'Davadantī'). Other variations reveal a representation of nonphonemic

⁶ As described, in most cases explicitly but sometimes implicitly, the vocalic nuclei show some interesting asymmetries. Vowels occur in all positions (except that *o* does not occur initially and *r* occurs medially only in Sanskrit loanwords), thus also next to themselves: *gaḍaḍai* 'reverberate', *hosii* 'become'; and in clusters of two or three: *neura* 'anklets', *huī* 'became', *nīpāī* 'prepare', *āīu* 'arrive'. The nuclei can be arranged as follows:

I. Short nuclei: A. non-nasal simplex, one mora (*a, i, u, r*); B. simplex plus nasal component, one mora (*aṁ, iṁ, uṁ*). II. Long nuclei: A. monophthongal (three of these with and without nasal component): *ā(ṁ) = a* plus level (i.e. non-raising, non-lowering) component of length, *ī(ṁ) = i* plus level component of length, *ū(ṁ) = u* plus level component of length, *e (= i* plus lowering component of length?), *o (= u* plus lowering component of length?); B. diphthongal: of two morae (*ai = a* plus *ī*, *ii = i* plus *ī*, *ui = u* plus *ī*, *au = a* plus *u*), of three morae (*āī = ā* plus *ī*, *īī = ī* plus *ī*, *ūī = ū* plus *ī*, *oi = o* plus *ī*).

Obviously the structure would be simpler if no diphthongs were assumed for the language of this text. The author finds the following evidence for them in such orthographical variations (272a): 'In the writing *ī*, in the medial and final position, represents both the vowel *ī* and the consonantal *ī* of diphthongs. The character *ya* represents the consonant *y* (with inherent vowel *a*), as well as the consonantal *ī* of diphthongs. This is shown by the rhymes of *āī* with *āya* (and *āyu*) and *oi* with *oya*, and by the alternation in the writing of all the manuscripts between *ai* and *aya*, *āī* and *āya*, *ii* and *iya*, *īī* and *īya*, *ūī* and *ūya*, and *oi* and *oya*. These indicate that in such vowel clusters the *ī* was being replaced in the spoken language by the consonantal *ī*. That is to say, these vowel clusters were being replaced by the diphthongs *aī āī iī īī ūī oī*. For metrical reasons, however, it appears that these were read as vowel plus vowel *ī*, since in the meters the clusters *āī īī oī* require a count of three rather than two morae.'

⁷ One wishes that the author had presented the evidence (minimal pairs, at least) for the establishment of the consonantal phonemes, particularly *ṅ* and *ṇ*.

features, e.g. *y* as a nonphonemic glide between otherwise contiguous vowels (*gāiu* 'went' in mss. B and H, *gāiyu* in P and S). Many others show dialectal differences, e.g. *a* for *u*: BP *guraṇīya* 'nun', H *guruṇīya*; and BS *tajha* 'you', HP *tujha*.

In the second subdivision of the grammatical section the author presents with extreme thoroughness the morphological data organized in conventional ways, purposely making little attempt at a structural analysis. He explains his procedure in footnote 34 (276b): 'It has been found convenient to treat the linguistic evidence in the traditional fashion—"convenient" in that, since previous works in the field (see bibliography) have been thus presented, any new data gathered may be more readily coordinated with those already collected and arranged. (A future article is planned, presenting a structural study of the language of these texts.) In a presentation of this type the domain covered by morphology includes much which otherwise would be assigned to syntax ...' Following the 'traditional fashion' words are classed as nouns, adjectives (including present, past, and future participles), pronouns, numerals, verbs, postpositives, adverbs, conjunctions, interjections, and emphatics. The morphology of nouns and adjectives is treated first. Genders, numbers, and cases are identified; determination of gender is described; types of prefixation and suffixation are listed;⁸ the case suffixes are discussed and summarized in a table;⁹ and finally (278a-91b), copious examples are given of nouns, adjectives, and participial forms in every gender, number, and case (existing in the text), for all nominal bases as identified by their nom.-acc. sg. suffixes—*a* bases, (*a*)*u* or extended bases, *o* or extended bases, *ā* bases, *ī* bases, and *ā* bases. Bender has done an impressive job of collection, labeling, and presentation. One may have some doubts as to whether conjunctive participles (absolutives) in *-ī* are to be classed as having the loc. sg. neuter suffix in *-ī* or whether present participial forms in *-ām* show an abl. sg. neuter suffix. Such matters are relatively unimportant. All the raw data that can be gleaned from this corpus are at hand in eminently usable form, ready to be combined with data from other sources for a structural description of OG noun morphology. The same full documentation is accorded the pronouns,¹⁰ and also the verbs,¹¹ where for every base (vocalic

⁸ Nominal prefix: *aṇa-* 'not'; nominal suffixes (added to nominal bases): *-(a)ḍ-* (*hīyaḍae* 'heart'), *-(a)l-* (*āmdhalau* 'blind'), *-il-* (*pahilau* 'first'), *-(a)laḍ-* (*ekalaḍau* 'alone'), *-im-* (*guhīrima* 'gravity'); nominalizing suffixes (added to verbal bases): zero of conjunctive participles (*āṇī* 'brought'), zero of past participles (*hūu* 'became'), *-ī(y)-* and *-y-* of past participles (*āvīyu* 'arrived'), *-(n)t-* of present participles (*hūmta* 'becoming'), *-v-* of infinitives (*thāva* 'be'), conjunctive participles (*aṇāvī* 'caused to bring'), gerundives (*joāvau* 'find out'), and the partial base variation of some past participial bases (*kiddha* 'made').

⁹ Singular: nominative-accusative: *a*, (*a*)*ā(m)*, *o*, *ā*, *u(m)*, *ū(m)*, *i*, *ī*; vocative: identical with nom.-acc. when occurring; instrumental: *ii(m)*, *e*, *ihī(m)*, *i(m)*, *ī*; locative: *ii(m)*, *e*, *i(m)*, *ī*, (*t*)*tha*; genitive: *ha*, *ā*; ablative: *o*, *ām*. Plural: nominative-accusative: *a*, *ā(m)*, *ā*, *ū*, *i*, *ī*; vocative: no occurrences; instrumental: *e*, *i*, *ihī(m)*; locative: *e*, *i*; genitive: *ā(m)*, *ha(m)*; ablative: no occurrences.

¹⁰ They are (cited in a nominative form when possible): *hūm* 'I'; *tūm* 'you'; *ā* 'he' (pronoun), 'this' (adj.); *so* 'he, that'; *sā* 'she, that'; *e* 'he, this'; *te* 'he, that'; *teṭā* 'so much'; *kavaṇa* 'who?', *koi* 'anyone, any'; *je* 'who, which'; *jeje* (acc. sg.) 'whatever'; *sahu* 'every, all'.

¹¹ The following personal endings appear in this text. Singular: 1st person *u(m)*; 2nd

and consonantal) examples are given of every available person and number of the so-called present indicative (e.g. *ho-i* 'becomes'), the future (*ho-si-i* 'will become'), the precative (*hu-jj-a* 'may be'), the passive (*das-i-i* 'is bitten'), the causative (*bhaṇ-āv-a-i* 'causes to be recited'), and the denominative (*bhoga-v-a-i* 'enjoys'), as well as examples of every stem formation of the present, past, gerundive, and conjunctive participles and of the infinitive.

The third grammatical subdivision contains precisely what its modest title says—notes on syntax, particularly concerning the sentence types. The basic elements of the sentence (subject and predicate) are identified, and the ways in which these elements may be expanded by attributives of various sorts are exemplified. The minor sentence, consisting of interjections or vocatives, is illustrated: *jaya jaya* 'Heil! Heil!'. Examples of the full sentence types and subtypes are given as follows: (1) equational type—a. (noun, noun) *tujha piya niṣadha hūm* 'your father Niṣadha I = I [am] your father Niṣadha'; b. (noun, adjective) *caṇḍana kaṭṭu* 'sandal [is] bitter'; c. (noun, participle) *hiva savi sājaṇa haraṣiṇyā* 'now all good folk [were] delighted'; (2) narrative type (actor-action, nominal-verbal) *bolaiṃ baṇḍiṇa biruda* 'sang bards poems of praise = bards sang poems of praise'; (2a) narrative subclass (verbs *ch-* 'be', *thā-* 'become', *ho-* 'become', and nominal form) *te nala hoi* 'he Nala is = he is Nala'. Next, the types of complex sentences are illustrated: (1) coordinate, *vaṃchai sahū-i punya phala, punya karai paṇi koi* 'desires everyone virtue-fruit, virtue practices but anyone? = Everyone desires the fruit of virtue, but does anyone practice virtue?'; (2) subordinate—a. noun function, *pūchaiṃ kihim jāsiu* 'they asked, "where are you going?"'; b. adjective function, *je tuha mana mohai vara te* 'who your mind captivates, choose him = Choose him who pleases you'; c. adverb function, *punya na hui java pādharauṃ tava sukha navi hoi* 'virtue not is when correct, then happiness not is = When virtue is amiss, then there is no happiness'. Finally, compound words are conventionally classified as *dyandva*, *tatpuruṣa*, etc., and illustrated. As the author has sensibly realized, an attempt at an exhaustive structural study of OG syntax would have been impossible at this time, since the phonemic analysis needs to be reworked and completed in greater detail and since the morphemic analysis has hardly been begun.

The third section is the text: 322 stanzas of varying sizes, meticulously edited, followed by notes which contain the mss. variants for every word in every line. These variants are invaluable in the study of the phonology and the morphology. The fourth section is an excellent translation, close, accurate, well phrased. The fifth is an exhaustive glossary which cites and identifies every form that occurs in the established text as well as the significant variants. It is estimated that there are 2850 main entries. The author disclaims having made an etymological study of each form, but gives cognates and possible cognates wherever

person *i* (28 examples), *e* (5), *a* (1), zero (2)—all of these (except 6 futures in *i*) occurring with the imperative; 3rd person *i* (64 examples), *e* (4), *a* (2). Plural: 1st person, no occurrences; 2nd person *u*, *ha* (the latter only in the imperative); 3rd person *i(m)*, *mti* (Skt. ending), *ṃtaiṃ*. In the 2nd and 3rd singular one suspects the presence of allographs. To identify these, when one's only knowledge of the language comes from written records, can be difficult.

he can (Sanskrit, Prākṛit, Modern Gujarātī). A list of references concludes the work.

Essentially this is a philological treatise—one of the first rank. The author's solid, patient scholarship deserves praise. His conservatism in refraining from premature structural codifications deserves even more. It is highly important to gather a sufficient corpus before attempting a definitive descriptive analysis; for a language that exists only in written records, the task of gathering the corpus can be tedious and exacting. More studies like this one are needed in the field of New Indo-Aryan; Bender's work stands as a model.

The structure and development of Russian. By W. K. MATTHEWS. Pp. ix, 225. Cambridge: At the University Press, 1953.

Reviewed by GORDON H. FAIRBANKS, *Cornell University*

To American linguists the title of this book is misleading. We might expect a systematic treatment of the structure of Russian in terms of the phonemes and morphemes, their variants and their distribution, and the history of the development of this structure presented in a rigorous and intensive style. Instead of this, the book is discursive and diffuse; it is very readable for a historical grammar, but it leaves the reader with only a sketchy idea of the structure and development of Russian. A further serious defect mars its usefulness to students: comparative linguistics is almost discredited as worthless, being characterized as 'uncertain ground' (vii), 'an unverifiable approximation of the truth' (vii), 'conjecture' (109), 'abstractions, which lack the validity of mathematical formulas' (110), and 'entirely dependent ... on chance discoveries' (110).

Part I treats the structure of Russian (Introduction, Sounds, Words and forms, Sentences, Dialects); Part II treats the development of Russian (Conjecture and record, The Kijev and Tartar periods, The Moscow period, The eighteenth century, The nineteenth century and after, The post-revolutionary period); Part III presents a selection of passages from various periods, arranged chronologically and provided with English translations.

The alphabet is quite properly treated as a representation of the language. Some of the principles of the relationship between the language and the writing system are mentioned, but not rigorously stated. We are told that 'In Russian, as in German, the morphological, or "etymological", principle, which is paramount, overlooks purely phonetic values in the interests of grammar; thus fem. nom. sing. *rozʹ* is not written *rosh*, as it would be if the phonetic principle prevailed, but retains both *zh* and (*'*), because the rest of the paradigm of declension to which it belongs has *zh* and illustrates a "soft-ending" type' (8). This means that the writing system is morphophonemic in the indication of voiced and voiceless consonants, although we are not told the conditions of the morphophonemic alternation, nor the extent to which this alternation is represented in the writing system. The writing system is also morphophonemic with respect to the vowels, but this is not mentioned at all.

In the section on the sounds of Russian, Matthews adopts an equivocal

attitude towards the phoneme, which is implicitly indicated by the fact that not a single form in the book is cited in phonemic transcription, and explicitly by his statement: 'By drawing a sharp distinction between phoneme and sound, the comprehensive and the particular, however, we expose ourselves to the possible error of ignoring the physical basis of the phoneme, which is "realised" in a sound, as language itself is "realised" in speech' (19). Matthews says there are five vowel phonemes in Russian, /i, e, a, o, u/. He seems disturbed, however, that [i] and [ɨ] should be considered allophones of the same phoneme, although his objections are irrelevant: 'Phonology, primed by the science of meaning, or semantics, regards i and ɨ as phonematic ("phonemic") variants, because they are not used to differentiate meaning, which is the test of a phoneme, but to the ear the two sounds are obviously different, i being a front, and ɨ a back, vowel, and unless the difference is observed in speech, the native Russian may not understand at all' (23). He is not disturbed by the fact that each of the other phonemes has two quite distinct allophones in stressed position. He gives a chart in the form of the Jones quadrangle of cardinal vowels which is supposed to illustrate the variants of the phonemes and their positions of articulation. He links them as follows: [i ɪ], [i ɨ ə], [e ɛ ɪ], [u ʊ ʊ], [o ɔ], [æ a ʌ] (24). These are charted as if there were six vowel phonemes; but if we take Matthews' statement that there are only five, we can group [i ɪ] and [i ɨ ə] in one phoneme /i/. There is no indication that [ə] should belong to any other phoneme, although this would mean that Matthews' transcription of *goroda* [gəɾə'da] would be /giradá/ phonemically, a highly unlikely phonemicization. He also includes [ɪ] both with what must represent /i/ and what must represent /e/, an overlapping that is nowhere discussed. Later Matthews states that the phonologist 'requires no more than six vowel-symbols, viz. i, e, a, o, u, ə, with an appropriate device, e.g. the postliteral hook, for discriminating between the palatalised and the non-palatalised sets of consonants' (29); but he does not explain why six symbols should be required if there are only five vowel phonemes. These six symbols are then used in a chart (29) which is supposed to show the distribution of the vowel phonemes in relation to the stress. This comes close to being a statement of the morphophonemic alternations of the vowels, but misses the essential point that the distinctive position depends on whether the preceding consonant is palatalized, not on whether the position is pretonic or posttonic. Word stress is discussed, but stress on units larger than a word is not mentioned. Intonation is treated briefly from the phonetic point of view, but with no attempt to discuss it phonemically.

In the sections on morphology and syntax the treatment is neither structural nor systematic. The criteria for setting up form classes is sometimes formal, but more commonly semantic. This is illustrated by the following statements: 'we recognise the existence of the two major types, viz. words of full meaning, which comprise the major parts of speech, and subsidiary words, which comprise the prepositions, conjunctions, and other particles' (42), and 'the nucleus of the nominal class is the noun (substantive), which is essentially appellative and names, or "stands for", either concrete objects or "objectified" ideas. In Russian it presents these in association with the grammatical categories of

gender, number, and case' (46). In Russian there is no inflection of the noun for gender; gender is a matter of syntax, but this would not be clear from the above quotation. Speaking of adjectives, Matthews says 'there are three types of declension, and they are based on the now familiar apophony of hard and soft stems' (51). Any subdivision of the adjectives into three types is based on the writing system and has nothing to do with the language; there is only one type of adjective. Verbal categories and sentence types are set up on the basis of semantic criteria. Any discussion of derivation is avoided by eliminating it from the morphology: 'the association of *gordyi* "proud" with *gordost* "pride" is derivational and belongs therefore not to grammar, but to the vocabulary' (44).

In the section on dialects a chart is reprinted from Avanesov¹ showing the five major types of phonemic structures that occur in the dialects. In reprinting the chart (93), a misprint has crept in: *o* should read *o* and vice versa in columns 1 and 2. The chart also repeats a misprint that occurs in Avanesov:² in column 4 *e* should occupy the same space as both *e* and *ɛ* in column 3, not the same space as *e* and *ɛ* in column 1. Referring to Avanesov's analysis, Matthews states that 'such vowel-systems are the outcome of relatively recent analysis and have not yet been applied as criteria to differentiate dialects' (94). Then, leaving aside all phonemic discussion, he presents the main phonetic and morphological characteristics of the dialects.

The second part of the book deals with the history of Russian. It is in this part that Matthews evinces his distrust of comparative linguistics:

Balto-Slavonic, in its turn, is regarded as having emerged from the "ultimate" source of the Indo-European languages—Common Indo-European, a language which is an even more recondite complex of linguistic formulas than Balto-Slavonic and Common Slavonic. It is obvious to a little thought on this subject that we are dealing here with abstractions, which lack the validity of mathematical formulas. ... E. H. Sturtevant goes so far as to postulate an even earlier unity than Indo-European, to which he has given the name "Indo-Hittite". The possibility of such radical reconstructions of the protoglossa shows how entirely dependent orthodox "comparative-historical" linguistics is on chance discoveries, and this and the involuntary choice of temporally disparate forms, representing the most varied stages of linguistic development, are sufficient in themselves to invalidate the protoglossa, or parent-language, theory as an adequate explanation of the unrecorded past (110).

This represents a complete misunderstanding of science, mathematics, and comparative linguistics: the last of these is a branch of science, not of mathematics. It is the purpose of science to observe data pertaining to some aspect of the universe, to order and classify the data, and on the basis of the data to set forth hypotheses which will best explain most of the data. The hypothesis that does this is for the present valid. It is characteristic of a scientific hypothesis that it is capable of disproof by the introduction of new data, but is not subject to proof in the absolute sense by any amount of data. The hypotheses of comparative linguistics are precisely of this type. To the extent that Sturtevant's

¹ R. I. Avanesov, *Očerki russkoj dialektologii* 1.50 (Moscow, 1949).

² The misprint in the chart is clear from the discussion in Avanesov, *Očerki* 44-50.

hypothesis of Indo-Hittite is the best hypothesis on the basis of the data we now have on this family of languages, it is just as valid as Einstein's specific and general theories of relativity or any other accepted hypothesis in any of the branches of science, and like Einstein's theory will be discarded by all scientists as soon as new facts disprove it or a new hypothesis can be shown to explain the data more simply or explain more of the data. A mathematical formula is entirely different; it is based on some set of assumptions that need not be referred to any specific data, and is developed on the basis of the assumptions alone. Contrary mathematical formulas may be developed from contrary assumptions, and each formula is valid if it is consistent within itself; as long as it is consistent within itself, a mathematical formula is not capable of disproof. Such a formula may be applied to the description of observed data in science if it is found to fit the data; but no matter how well the formula may apply, it neither proves nor disproves the data, although it may considerably simplify the description. Note also that the 'choice of temporally disparate forms' does not invalidate the conclusions of comparative linguistics, since in comparative studies this factor is taken into consideration. The quotation above implies that the study of existing records is in some way more accurate, more valid, than the conclusions of comparative linguistics, yet these records are themselves difficult of interpretation and sometimes comparative linguistics is necessary to their interpretation.³

Old Russian is said to have 33 consonant and either eight or nine vowel phonemes (114). The contrast between palatalized and non-palatalized consonants is considered phonemic; but if this is so, it is not necessary to have so many vowel phonemes. For each period after Old Russian the main phonetic changes are mentioned, but there is no statement of the resultant phonemic systems. The main morphological changes for each period are also treated, but again not structurally. Some of the chief types of borrowing for the various periods are mentioned, and for the later periods there is a brief statement of stylistic developments.

The selections with English translations in the third part of the book range from the 11th century to the present time (though the earliest selections are from late manuscripts), and adequately illustrate the development of the language.

Homenaje a Fritz Krüger. [Ed. by TORIBIO M. LUCERO and ALFREDO DORNHEIM.] (Universidad Nacional de Cuyo, Facultad de Filosofía y Letras.) Vol. 1, pp. xxx, 466, with a portrait. Mendoza, República Argentina, 1953.

Reviewed by LEO SPITZER, *Johns Hopkins University*

The irony of history has willed that a first wave of refugee scholars to Argentina from fascist countries (among them Amado Alonso and his pupils, Corominas, and Terracini) should be followed by a second wave of refugees from countries

³ One case of this type can be found in the Old Bulgarian manuscripts, where the writing *rŭ*, for example, is generally considered to represent two different phonemes or groups of phonemes; cf. N. van Wijk, *Geschichte der altkirchenslavischen Sprache* 44, 59-61 (Berlin and Leipzig, 1931). The evidence is from comparative linguistics.

that gave up fascism after the last war (Krüger, Moldenhauer, Găzdaru). In either case it was their scholarship rather than their political affiliation which won them a place in which to continue their work—which is obviously as it should be. It is to the credit of the University of Cuyo that it has extended the same help to Juan Corominas (who was able to found there a new scholarly journal, the *Anales del Instituto de lingüística*) as to his successor Fritz Krüger. It is to the latter that the University has dedicated the Festschrift of which the first volume is now before us, on the occasion of his 60th birthday. The publication of a testimonial volume was decided, as the Rector tells us in his foreword, after Krüger had been only one year in Cuyo—an unusually delicate gesture, greeting a new colleague by recognizing his earlier scholarly contributions. Here for once the sad experience of the émigré (*quanto sa di sale ...*, as Dante knew) was not realized.

Fritz Krüger deserves to be so honored. With his more than thirty years of teaching at the University of Hamburg he has to his credit an outstanding record of publications, which was interrupted, according to the bibliography in this volume, only once: in the crucial years 1945 and 1946. He had also founded and directed for many years the journal that replaced his predecessor Schädel's short-lived and more specialized *Revue* and *Bulletin de dialectologie romane*: 16 volumes of Krüger's *Volkstum und Kultur der Romanen* have appeared, with 37 annexes (*Hamburger Studien zu Volkstum und Kultur der Romanen*). Krüger was also a successful director of theses, and was able to gather around himself a group of pupils specializing in the fields dear to their master. The enormous number of substantial reviews written by Krüger shows his abiding sympathetic interest in the work of others, to which he is always able to contribute valuable suggestions prompted by his own learning.

His works center round *Volkstum und Kultur*—ethnography, folklore, rural arts and crafts, and popular speech—a 'naturalistic' approach to civilization which was peculiar to him among German scholars in Romance, being more widespread among German scholars in Germanic, and which may astonish us by its contrast with the urban atmosphere of Hamburg in which Krüger lived. He can be said to be the most authentic representative of the Wörter-und-Sachen trend in the generation that followed its founders Meringer and Schuchardt—or rather of the Sachen-und-Wörter trend, since his contemporaries Max Leopold Wagner and Gerhardt Rohlfs, equally at home in folklore and the world of things, are primarily concerned rather with the 'words' than with the 'things'. These three scholars have, so to speak, divided among themselves the Romance territory: Rohlfs works on Italian dialectology (including Magna Graecia) and on Gascon; Wagner works on Sardinian, Judeo-Spanish, and American Spanish; Krüger is mainly concerned with Western France, Languedoc, and Roussillon, the Pyrenaean dialects, and all the Iberian languages. Characteristic titles of Krüger's writings are: *Die Gegenstandskultur Sanabrias und seiner Nachbargebiete*; *Sach- und Wortkundliches vom Wasser in den Pyrenäen*; *Die Hochpyrenäen (Landschaften. Haus und Hof ... Ländliche Arbeit ...)*. His interest in linguistics proper is turned mainly to two fields, phonetics and etymology. The first is represented by discoveries, on his trips of exploration, of interesting dia-

lectal variants (Studien zur Lautgeschichte westspanischer Mundarten; El dialecto de San Ciprián de Sanabria); the second is connected with his inquiry into the local nomenclature of primitive techniques. Krüger has not completely avoided syntactical studies; No. 362 of his bibliography is to be a study on the Argentine idiom *es de lindo*. All interest, however, in literature as a value in itself (and not as a reflection of folklore, as in No. 241: Rural life as the theme of Galician folk-songs), all preoccupation with the history of ideas or with philosophic speculation, is conspicuously absent from his work; though it is true that in his journal he accepted articles on literature, and sometimes reviewed literary studies, especially in the old Spanish field. He is an outstanding representative of a type of German scholar that is rapidly dying out before our eyes: the utterly reliable, patient, indefatigable, lifelong worker in a special field of his own choosing, which he masters absolutely and in which he moves with ease, joy, and that piety that is defined by the untranslatable German word *Werkfrömmigkeit*. It is ironical to think that the speculative work of scholars of much greater genius, such as Gilliéron or Schuchardt, will probably soon be in-part superseded, while the masses of new material unearthed and interpreted by Krüger's patient investigation will remain as a perpetual source of information for scholars to come who interest themselves in his field.

The volume before us, to which 22 German, Spanish, Catalan, Portuguese, and Latin American authors have contributed, reflects by its proportions Krüger's spheres of interest: there are 300 pages on folklore and ethnography, 100 on linguistics, 55 on literature (of which 33 are taken up by a paper on Cervantes, by E. Lommatzsch). I am obviously unable to discuss all these articles in detail. It is my personal belief that among the linguistic articles, the most deeply searching is Gamillscheg's new addition to his earlier studies on the different strata of French borrowings from Germanic, because it reveals new possibilities of dating and etymological explanation. (As to the verb *estibourner* 'to prop up, to fortify earth by poles of wood', found at Amiens in the 15th century, which he explains as derived from an old Norse *sti(g)bord* and influenced by the word family *borne, borner* 'to bound', I may mention that, as I shall try to prove in a forthcoming article in *MLN*, that verb may also explain the origin of the English—obviously postverbal—adjective *stubborn*, 14th century *stiborne, styborne* in the meaning 'strong, resistant'.) On the other hand, the lengthy study by H. Flasche, of the conjunction *que* in the prose of the 17th-century Portuguese church orator Vieira, seems to me to suffer from a failure to distinguish between the normal syntax of the time (which can only be established by a study of contemporary prose in general) and the personal style of the author.

Among the contributions to folklore I found the collection by Joan Amades of 'termes sense significat' (senseless words of the type *laralalà, leri leri*) in Catalan folk poetry as valuable as the theory offered by the author that such words represent residues of, or a regression to, a primitive wordless stage of emotional ejaculation which originally accompanied popular tunes. One must of course object to the use of the term 'letters' (*lletres*) instead of 'sounds' in reference to the *l, r, a*'s that occur in those popular words. More serious seems to me the absence of any comparative linguistic treatment of such refrain words as those

mentioned above (and also *lantururura*, *tiroliroliro*, *derideta*), which should be studied in relation to parallel French coinages (on which Thureau's book on the French refrain offers ample information) and to the international stock of onomatopoeias (*to trill*, *trällern*, etc.). In the same manner the original etymological value of 'magic words' (*Santa Cucurulla*, *milaneu*, *quirié quirié*) should be established by comparative study. I have the impression that the author has a tendency to consider words as etymologically (that is, diachronically) 'senseless' which are only synchronically so: they may have no clear-cut meaning for the average Catalan speaker today, but only because the original meaning has been obliterated. I cannot explain otherwise the author's assumption of 'intentionally made-up words' (words created with the intention of being senseless?). For instance, *tiroliro* 'the fine membranes by which the interior of the walnut is partitioned' (occurring in a popular riddle) may not be understandable today, but the comparative linguist will connect it with the ultimately onomatopoeic refrain word *tiroliroliro* mentioned above: it shows a semantic transfer 'something light and elusive like the sound of a flute' > 'something fine and thin'. The originally acoustic term has been used for the rendering of a visual aspect of a 'thing' — a process not too far from the transfer of the French acoustic refrain word *tirelire* to another 'thing', the 'piggy-bank': here the sound of the falling coin is the semantic bridge, there the idea of 'fineness'. Similarly, in the jocose definition of an egg as a box *molt ben requinquilladeta* ('worked out with great care and precision') the participle is not made up artificially, but has an obvious connection with the family of Fr. *se requinquer* 'to deck oneself out with elegance' and ultimately with *quincaillerie* 'copper- and iron-ware' (cf. *Diccionari Aguiló* s.v. *requincalla*, tentatively translated 'retòrica': onomatopoeic stem **klink*); cf. Gamillscheg's treatment of these words, *Etymologisches Wörterbuch der französischen Sprache*.

Lulelappsk ordbok (Lulelappisches Wörterbuch). By HARALD GRUNDSTRÖM, on the basis of collections by K. B. Wiklund, Björn Collinder, and the author. (Skrifter utgivna genom Landsmåls- och Folksminnesarkivet i Uppsala, Ser. C.1.) Fasc. 1-11, pp. vi, 1711. Uppsala: A.-B. Lundequistska Bokhandeln; København: Einar Munksgaard, 1946-53.

Reviewed by ASBJÖRN NESHEIM, *Indiana University*

This dictionary is an important contribution to the scientific literature on the Lapps and their language. The author was for many years a priest in the parish of Jokkmokk in Lule Lappmark in the north of Sweden, a district with an old Lapp population. He soon became deeply interested in the language and culture of the people, and, following the best traditions of earlier priests among the Lapps, began to collect ethnological and linguistic material. As a result of this work, Grundström has published interesting papers on Lapp folklore and a most valuable record of one Lapp's narrative of his own life: *Jåhttee saamee viessoom* 'The life of a nomadic Lapp' (1937), with a Swedish translation (1933) and a Lapp-Swedish-German vocabulary (1939).

Besides this vocabulary there exist two earlier Lule-Lapp glossaries, edited by

the well-known Swedish scholar K. B. Wiklund: *Lule-lappisches wörterbuch* (Helsingfors, 1890) and a Lapp-Swedish glossary in his *Lärobok i lappiska språket* (2d ed., Uppsala, 1915). But these glossaries are not very extensive; Grundström's new dictionary—which will consist, when finished, of well over 1900 pages—is an essential addition to Lapp lexicography. The book is based on material collected by Wiklund, by Björn Collinder, and by the author himself. It was on Collinder's initiative that Grundström in 1942 undertook the preparation of the dictionary, and Collinder has helped the author in many ways with his difficult and troublesome work. Other persons and institutions also, in various ways, have given the author their support; but Collinder has been the prime mover for the work.

Let me say at once: the book does great honor to the author and to Swedish science, and will long remain an important aid in the study of Lapp and other Finno-Ugric languages. It is a fine example of able and persevering research. This should be kept in mind when I now point out certain weak points of the book:

My chief objection is to the spelling system of the dictionary. Apart from unessential alterations, the spelling is that of Wiklund, chosen because it has become to a certain degree traditional in Lule Lappmark. It seems to me, however, that this tradition should not have kept the author and his advisors from altering the spelling system in the direction of consistency and clearness. Lule-Lapp will certainly never have any real importance as a literary language; this dictionary therefore should serve first and foremost the scientific study of Lapp.

Even before Wiklund's spelling there was a standard spelling for Lule-Lapp. Compared with this, Wiklund's spelling meant an important improvement. Thus, the former orthography made no difference between 'etymologically long' *a* and 'etymologically short' *a*, so that *manna* could be read both *mannā* 'he goes' and *mānnā* 'child'. Nor was there any clear distinction between preaspirated fortis stops and unaspirated lenis stops, so that *laike* could mean *lai'hkē* (Konrad Nielsen for Finnmark-Lapp: *lai'ke*) 'lazy' as well as *lai'kē* (Nielsen's *lai'ge*) 'woolen yarn'.

A fundamental feature of the Central, North, and East Lapp dialects is the so-called stage shift. This must be clearly expressed in any standard spelling. The stage shift is a quantitative, partly also qualitative, alternation of the stem consonants, implying also a shift in stress and pitch. It is clearly expressed in the spelling of Wiklund; cf. *kahtō*, gen. *kāhtō* 'cat' and *kāhtōt*, 1st sg. pres. *kātōv* 'be away, disappear' (Finnish *katoa*-). The first of these words has a 'relatively long' stem consonant, the second a 'relatively short' one. In other cases, however, it may be impossible to determine the stage from the spelling; thus, nominative *piessē* with a relatively long stem consonant means 'birch bark', but with a relatively short one it means 'nest'. Nielsen solved this problem in a simple way by using an apostrophe in the strong stage: *bæs'se* 'birch bark' vs. *bæssē* 'nest'. The same device is adopted by Grundström, but primarily as a phonetic sign, to indicate a glide vowel in certain cases of relatively long stem consonants: *tjal'mē* (Wiklund's *tjal'mē*) 'eye', gen. *tjalmē*.

As mentioned above, Wiklund in his spelling introduced a distinction between

fortis and lenis stops, but only in stressed positions. But in unstressed syllables we also find a contrast in the dialects, e.g. causatives in *-htit* (Nielsen's *-tit*) vs. frequentative-continuatives in *-tit* (Nielsen's *-dit*): *porōhtit* 'let eat, give to eat', *porōtit* 'have a meal, eat'. Here Grundström follows Wiklund, writing *påråtit* for both derivatives. In this case the historically determined difference between the two suffixes (**-kta*, **-xta* vs. **-nte*) ought to have appeared from the spelling.

In the final position the distinction between fortis and lenis stops seems partly to have disappeared; thus, for Southern Gellivare Wiklund has written unaspirated stops in nearly all cases. In reflected forms, however, the original distinction in many cases seems to reappear, e.g. SG *allak* (Nielsen's *állåg*) 'high' ~ *allakis*, *ārrat* (Nielsen's *ārrād*) 'early in the morning' ~ *ārratisan* vs. *sapēk* (Nielsen's *sābek*) 'ski' ~ *sapēhka*; similarly *suola* 'thief', gen. *suollaka* (Nielsen's *suolā*, *suollāgā*), where the stop was lost in final position. In Northern Gellivare there is a clear contrast between fortis and lenis in final position also, e.g. in *allak* 'high' vs. *alēhk* 'blue' (Nielsen's *állåg*, *ālek*). For the former type I have found once *-hk*, in all other cases *-k*; for the latter type I have found everywhere *-hk*. The few adverbs that I have found of the type of *kuhkēt* 'far' (Nielsen's *gukket*), with *-t* < **-kta*, **-xta*, have *-ht* in Northern Gellivare; while the type of *kuhkkēt* 'from far away' (Finn. *kaukaa* < **kaukaša*), *rusjkat* 'brownish' (with *-t* < **-ta*, **-ša*; Nielsen's *ruš'kād*, Finn. *ruskea*), and *isēt* 'head of the household' (with *-t* < **-nta*; Finn. *isäntä*) all show *-t* without aspiration. It is interesting that the infinitive suffix *-t* (< *-šak*) in all dialects seems to be unaspirated, thus representing a lenis stop. Lule-Lapp seems in this case to have preserved a more original feature than Finnmark-Lapp, where the infinitive now ends in fortis *-t* (*-ht*).

There are other weak points in Wiklund's way of writing fortis and lenis stops and affricates. Fortis stops and affricates differ from the corresponding lenes (and from other consonants) in that the final part of a preceding vowel or consonant is unvoiced. But in certain cases, because of the stress, the last part of a voiced consonant is voiceless even before a lenis. The 'medial stage' in the stage shift (i.e. the weak stage of relatively long stem consonants and the strong stage of relatively short stem consonants) is characterized by an increase in the intensity of the stress towards the syllable border, cutting through the stem consonants, which often causes the last part of a voiced consonant before another consonant to be voiceless. For this 'devoicing' Wiklund as a rule uses the mark ', even when the following consonant is fortis. Accordingly, such a form as *par'kō* can be interpreted both as the genitive of *par'hkō* 'bark' (Nielsen's *bar'ko*) and as gen. of *par'kō* 'work' (Nielsen's *bār'go*). Wiklund's *h* before fortis consequently cannot be considered a phonemic sign for the fortis quality, but only a phonetic sign for the voiceless finish of a vowel. Evidently a more radical change in Wiklund's spelling system would have given a more consistent picture of the consonant system in Lule-Lapp; the simplest way to distinguish between fortis and lenis would have been to adopt the writing of Nielsen: *p*, *t*, *k*, *c*, *č*, vs. *b*, *d*, *g*, *z*, *ž*.

Wiklund used macrons to indicate long vowels, for example *āhkātj* 'old woman',

ahkkō gen. *āhkō* 'grandmother'. This indicates the pronunciation of the word, but it does not identify all 'etymologically long' vowels, since it is omitted when such a vowel is pronounced half-long. The etymologically long vowels are by Wiklund written *a* ~ *ā*, *ē*, *ō*, *ie* ~ *e* (before *i*, *u*), *ā* ~ *ie* (before *ē* in the second syllable) ~ *e* (before *i*, *u*), *uō*, *ā* ~ *ā̃*; etymologically short vowels are written *a* ~ *ā*, *i*, *u*, *ā* ~ *ā̃*. Of the first series, *ē* and *ō* never occur in the first syllable, while *ie* ~ *e*, *ā* ~ *ie* ~ *e*, *uō*, *ā* ~ *ā̃* occur nowhere else; the vowels of the second series all occur in both syllables. It follows that two vowel signs in each series, namely *a* and *ā*, are in some cases ambiguous, covering both etymologically long and etymologically short vowels. To avoid misunderstanding, Wiklund added (*ā*) or (*ā̃*) for *a*, (*āa*) or (*ā̃*) for *ā*, after the words. Grundström has adopted Wiklund's writing of the vowels, but has dropped the explanatory parentheses. It is therefore sometimes impossible to infer the phonemic value of the vowel from the index forms alone: *kahttjat* with half-long *a* in the first syllable means 'hurry somewhere', with short *a* it means 'fall'; *pārrē*, depending upon how the *ā*(*rr*) is read, means 'raft', 'eating', 'summit of a mountain', or 'edge'. (Grundström has no sign to distinguish the longest quantity of *rr*, Nielsen's *r'r*, from the shortest, Nielsen's *rr*.) For *a* Grundström has in some instances eliminated the ambiguity by writing *ā* for the relatively long phoneme—e.g. *kāl'kat* (Nielsen's *gal'gāt*) 'unravel' vs. *kal'kat* (Nielsen's *gāl'gāt*) 'shall, must, etc.'—or *ā̃* for the short one—e.g. *tjällē* (Nielsen's *čälle*) 'hardness' vs. *tjallē* (Nielsen's *čal'le*) 'writer'. As regards the vowels also, then, the spelling in the dictionary should have been made clearer and more consistent through certain departures from Wiklund's system.

Apart from the spelling, I have very few objections to make to the dictionary. Sometimes I miss the citation of inflected forms in the phonetic transcription, sometimes I find an inconsistency in the transcription; but such faults are few and insignificant. Grundström had a difficult task in trying to unify three different collections of material: Wiklund's, Collinder's, and his own; he has accomplished it in an eminently satisfactory way. The translations in particular are excellent, and demonstrate the intimate knowledge of Lapp culture possessed by the author himself and by his two other chief contributors.

Grundström's dictionary is provided with three supplements, of which the first two have already been issued: a list of personal names current in Lule Lappmark, with explanations; a list of those words in Nielsen's dictionary that are found also in this one; and a grammatical survey of the Lule-Lapp dialects—the last still in press. These supplements, added to the unusual wealth of illustrations with which the uses of Lapp words are made clear, are the last touches in an exceptionally comprehensive and detailed picture of a dialect group. This work is the more remarkable as the author is not a linguist by profession, and has for many years been seriously ill; much of the final revision of the dictionary was done from a sickbed. As an acknowledgment of his achievement, the University of Uppsala has conferred upon Grundström an honorary doctorate, an action that we can only applaud.

Languages of West Africa. By DIETRICH WESTERMANN and M. A. BRYAN. (Handbook of African languages, Part 2.) Pp. 215, with folding map in pocket. London, New York, Toronto: Oxford University Press (for the International African Institute), 1952.

Reviewed by JOSEPH H. GREENBERG, *Columbia University*

This is the most ambitious in the series of handbooks currently being issued by the International African Institute, which are designed eventually to cover the entire continent linguistically. The area treated in the present work is bounded by the Atlantic Ocean on the west and Lake Chad on the east; it extends from the Gulf of Guinea northward approximately as far as the confines of the Sahara. Like the other volumes in the series, this one presents much valuable information, either widely scattered or unavailable in written sources, concerning the size, location, and nomenclature of the numerous linguistic communities of the area; like the others, it is provided with a map and an excellent introductory bibliography. On all of these counts the present work gives evidence of careful and painstaking research, and adds at a number of points to our knowledge of linguistic distributions in the West African area. The information is arranged in terms of a language classification. In view of the previous publications of the joint authors, I assume that the work of mapping nomenclature and population statistics was done primarily by Bryan, while the classification is that of Westermann, who is well known as the author of a whole series of publications on this topic.

The method of classification is described in the introductory section. Instead of the conventional 'family', 'subfamily', etc., a series of terms is employed, most or all of which have already appeared in other publications of this series. On the lowest level, 'language' indicates either a form of speech without recognized dialect variation or a dominant dialect with which others of lesser importance are associated. A number of dialects of which none appears dominant is called a 'dialect cluster'. The next higher unit is the 'language group', with a membership of related 'languages' or 'dialect clusters'. Above the 'language group' is the 'larger unit', which apparently designates the widest genetic unit posited in the present work. In addition we have the 'single unit', 'a language or dialect cluster which belongs to a larger unit while not sufficiently related to any other to form part of a language group' (7-8). Thus far all seems clear; Indo-European would be a larger unit, Germanic a language group, and Albanian a single unit. But there are further terms. We have the 'isolated group', a language group which is not part of a larger unit. This would be an independent family not comparable in extent or ramification to a larger unit. An instance in Asia might be the 'Hyperborean family' of Chuckchee, Koryak, and Kamchadal, which form a group but are apparently unrelated to other languages. Finally, there are 'isolated units', which are either languages known to be unrelated to anything else (like Burushaski in Central Asia) or not classified for lack of evidence. These latter are distinguished by the addition of a question mark. An examination of the work reveals, however, that the question mark also occurs in 'isolated language group?' and even 'larger unit?'. These expressions

are nowhere explained, but it seems reasonable to assume that what is intended is that the present evidence, though sufficient to show that the languages in the isolated language group and larger unit are related to each other, is insufficient to determine if the group is related to anything else.

Thus armed with what we hope is the correct interpretation of these terms, we are in a position to determine the number and constitution of independent linguistic units in the area. Or perhaps we are not. For as we peruse the work, we come upon the following statement (54) in the chapter devoted to the Kru languages, which are labeled an isolated language group: 'Their nearest relations are the Kwa languages.' The Kwa languages are the subject of a later chapter; they are called a larger unit. In other words, first we are told that an isolated language group is independent (by definition); then we are told that Kru is an isolated language group; finally we are told that it is most closely related to Kwa, also supposedly independent. Presumably Kru is also related to other unspecified languages, since if there are 'nearest relations', there should also be more distant ones.

I believe that this phraseology is a lapse by Westermann into the beliefs of an earlier period, when, as we shall see, he believed in the genetic unity of most of the languages of West Africa. Indeed, at one period he even considered the Kru languages a subdivision of the Kwa subfamily of West-Sudanic. For the present purpose, in view of the preponderant evidence as to Westermann's meaning in the rest of the work, we disregard this lapse. If we took the statement seriously, it would be impossible to make sense of the present classification; for, in the absence of any other statements of this kind in the work, we would have to regard the various larger units, isolated groups, and isolated units as potentially related, without any guide as to which are related to which. Surely ALL cannot be meant, since Westermann, like everyone else, certainly considers Chad-Hamitic, a larger unit of the present work, to be unrelated to the main West-Sudanic group, and specifically states that Songhai is independent.

Disregarding this lapse, then, our procedure will be to add up the number of larger units, isolated groups, and isolated units which are not accompanied by question marks. This will give us the minimal number of independent linguistic stocks, since resolution of the problems presented by the queried groups can add to the number of stocks but can never decrease it. Applying this method, we conclude that Westermann is asserting the existence of at least ten independent families in the area, of which five are larger units, two are isolated language groups, and three are isolated units. In addition, well over thirty units are queried, suggesting that in Westermann's view there will eventually prove to be well over ten independent linguistic families in the area. My own opinion is that there are three such families in the area covered in this work: Songhai, which is an independent language; the Niger-Congo family, embracing eight of the ten independent units of Westermann's classification and almost all of the queried units; and the Chad branch of Afro-Asiatic (Hamito-Semitic). Of these both the Niger-Congo and Hamito-Semitic are widely represented outside of the area treated here.

I shall deal first with the problems raised by Westermann's treatment of

Niger-Congo languages. A brief review of the history of his opinions regarding the classification of these languages may be of assistance in putting the present work in its proper perspective.

In his first work dealing with the problems of language relationships in Africa (*Die Sudansprachen*; Berlin, 1911), Westermann asserted that all the non-Hamitic and non-Bantu languages of that vast Sudanic area were related. He cited forms from five languages of the Western Sudan: Yoruba, Ewe, Efik, Gã, and Twi; and from three languages much farther to the east: Dinka and Nuba in the Anglo-Egyptian Sudan, and Kunama in Ethiopia. By the convenient device of treating the final consonants of the predominant CVC root formations of these languages as later suffixes, it was possible to adduce a good number of etymologies. The method was based on the assumption that the CV root morphemes of languages like Ewe must represent a 'primitive' monosyllabic type from which the others evolved.

Westermann was evidently dissatisfied with these results, and subsequently abandoned the thesis of the genetic unity of all the languages of the Sudan. It was noticeable in his work of 1911 that the five western languages figured frequently in convincing etymologies from which the eastern languages were excluded. In the following years Westermann began to work out the membership and details of a true genetic unit, involving practically all the languages of the western part of the Sudan, and published a series of separate treatments of subgroups in this family. Finally, in his general work *Die westlichen Sudansprachen und ihre Beziehungen zum Bantu* (Berlin, 1927), he presented approximately 300 etymologies involving the entire system of pronouns, all of the lower numerals, and numerous common nouns, verbs, and adjectives. He also demonstrated the essential unity of the system of noun classes marked by pairs of affixes for singular and plural, and pointed out the resemblance of this system to that of Bantu, extending even to specific details. In 110 of his etymologies, the Proto-Bantu forms reconstructed by Meinhof are close to, often identical with, the Proto-West-Sudanic of Westermann. His conviction regarding Bantu and West-Sudanic soon matured to the point where—in his article on African languages in the 14th edition of the *Encyclopaedia britannica* (1929), he asserted their genetic connection without qualification.

In 1940, in the chapters on language which he contributed to *Völkerkunde von Afrika* (Essen, 1940, under the joint authorship of H. Baumann, R. Thurnwald, and D. Westermann), he has apparently returned in some respects to the earlier general Sudanic conception. The term 'Sudansprachen' is once again used, but we now have four divisions: Nigritic, with Kwa as one of its subdivisions; Mandingo; Semi-Bantu languages; and Inner-Sudanic. There are defined on typological grounds; for example, Nigritic consists of languages without class systems and with CV root morphemes, the semi-Bantu group has CV root morphemes and noun classes, the Inner-Sudanic languages have CVC morphemes but no noun classes, and so on. The Bantu languages are listed separately as lying outside the Sudanic complex, and so are the Nilotic languages. As might be expected from the criteria involved, each of the four subdivisions (except Mandingo, a true but minor genetic unit) is a hodgepodge of related and un-

related languages. It is quite as though one were to set up three groups on the basis of sex gender: a group with no gender, comprising Basque, Hungarian, and English; a two-gender group, comprising Norwegian and Italian; and a three-gender group, comprising Russian and German. On the other hand, we are warned (*Völkerkunde* 383) that the term Sudanic 'designates languages of a common type whose genetic unity is only partially demonstrable'. We are not told for which languages this unity is fully demonstrable. Bantu, which is listed separately from Sudanic, is stated to have many roots in common with the Nigritic, Mandingo, and Semi-Bantu divisions. 'This relationship is closest among Kwa, Semi-Bantu and Bantu' (384). It is clear then, from Westermann's own statements, that the language divisions given here are not genetic, and that this is not a language classification in the usual sense: not all the languages in the Sudanic 'family' are related, while on the other hand there are 'related roots' (whatever that may mean) among languages in the separate Sudanic and Bantu 'families'. In its ignoring of West-Sudanic as a unit (the languages formerly assigned to it being split among the Mandingo, Semi-Bantu, and Nigritic subdivisions of a Sudanic family which comprises almost all the languages of the Sudan), this essay foreshadows the classification reviewed here.

The independent units of the present work correspond in general to the subgroups of West-Sudanic in the 1927 work. Reference to the latter also serves to make more intelligible the odd distribution of linguistic knowledge and ignorance implied by the new classification. All the languages qualified as unclassifiable for lack of evidence are in the eastern part of the area, in Nigeria or the Cameroons. Conceivably this might reflect the differential diligence of English and French scholarship; but as a matter of fact the western area contains British, French, Portuguese, and former German territory, while the eastern area contains the same except for Portuguese. The situation becomes understandable, however, if we realize that in *Die westlichen Sudansprachen*, the task of identifying West-Sudanic languages was not continued into the northern and eastern parts of Nigeria or into the Cameroons—the precise area in which every language or language group followed by a question mark is to be found.

For the present drastic step no explanation is offered here or, to my knowledge, anywhere else in Westermann's writings. It is quite as though Franz Bopp in 1860, after completing his comparative Indo-European grammar, had published, without explanation, a work in which the Indo-European family was fragmented into many independent subgroups and unclassified languages. Perhaps this may be looked on as a praiseworthy exercise in caution; but what if the subgroups are incorrect? That the overall membership of a large linguistic family should be more certain than that of its constituent subfamilies may at first blush seem something of a paradox. But the question whether A, B, and C are all related in some manner is often less difficult than the more subtle problem involved in subgrouping: the problem whether A is more closely related to B than to C. Nobody at present doubts that Oscan, Latin, and Irish are all related; but after more than a century of intensive Indo-European research there is still controversy as to whether Oscan is more closely related to Latin or to Celtic. Everything depends on the length of time during which the ancestral language of the branch

had a separate development. If this period is short, and the subsequent period during which the innovations have been subject to analogical and other changes is long, the problem becomes difficult and requires a great deal of descriptive analysis and reconstruction.

Westermann's use of typological criteria in these matters leads to inevitable errors. Of the independent units listed in the present work, only Mandingo and the Gur group are in my opinion completely correct as they stand. Any culture historian who accepts the remainder as valid units will do so at his peril. To take one instance, Westermann attributes great significance to the presence or absence of noun gender classification (both sex and non-sex gender). This is evidently his motive for placing Adyukru, which has noun prefix-classes, not with the closely related Ari and Abe of the neighboring Ivory Coast lagoons, but with the West-Atlantic languages, which generally have prefix classes. Since these two groups are now considered independent by Westermann, the error is analogous to stating that Dutch is related to Russian but not to German. In a study of the West-Atlantic languages, *MSOS* 31.63-86 (1928), Westermann presented 153 etymologies among various languages, designed to establish the validity of this subgroup within what he then considered to be the West-Sudanic family. In these etymologies, Adyukru figures only twice; and of these two, one is the first person plural pronoun, which is almost universal in Niger-Congo and therefore not a West-Atlantic innovation.

If Westermann had been consistent, he would have put the northern Guang and southern Guang dialects in separate West-Sudanic subgroups and would now treat these as separate families, since the northern dialects have noun classes and the southern have none. But wherever the relationship is this close, Westermann prefers to follow common sense in classing them together. As the dialects move further and further apart, however, precisely that will occur which Westermann apparently does not consider possible: classless and classifying languages of more distant relationship will be members of the same linguistic grouping.

I do not wish to leave the topic of Niger-Congo languages with an appearance of total disagreement. By and large, the languages now classed as members of the same independent unit are members of the same genetic subgroup of Niger-Congo in my classification. In two details, the present work marks an important advance. Fulani is now placed in the West-Atlantic group and its Hamitic status denied; I do not believe that any responsible scholar still regards it as Hamitic—a view first advanced by Meinhof in his *Sprachen der Hamiten* (Hamburg, 1912). And Songhai, which I consider an independent family, is now unequivocally accorded separate status.

The problems raised by Westermann's treatment of the Chad languages are in principle similar to those just discussed. What I consider to be the Chad group of Hamito-Semitic is divided into a Chad-Hamitic larger unit, therefore presumably independent, and a Chadic larger unit which is queried because of its possible relation to Chad-Hamitic. I consider this an arbitrary division with no basis in fact. In setting up a Chadic division as distinct from Chad-Hamitic, Westermann is apparently following Lukas, who did so because these languages

lack grammatical gender. However, as Westermann points out, all these languages except Mandara are now known to have gender. In itself I do not consider this fact especially important; what is important is that the feminine is marked by the almost universal Hamito-Semitic *t* and that the masculine of Gider, one of the Chadic languages of Westermann, has *n*, as in Hausa and Masa, both languages included in Chad-Hamitic by Westermann. In a series of Chad etymologies which I have compiled, and in a series of etymologies to be published in which Chad forms are connected with those of other Hamito-Semitic branches, the languages of this Chadic group enter just as frequently as those of the Chad-Hamitic group.

Most surprising of all is the placing of Angas and closely affiliated languages, which I consider part of a western Chad subgroup comprising Hausa, Ngizim-Bade, and the Bolewa group, in an isolated language group. The appearance of Angas in the chapter on the classless languages of Nigeria along with languages formerly considered Sudanic suggests that in Westermann's opinion they have nothing to do with Hamito-Semitic. The resemblances to the 'Chadic' and 'Chad-Hamitic' languages are attributed to borrowings from Hausa. This explanation is inadequate; it leads to improbabilities and contradictions of the sort which arise whenever a language is excluded from a group to which it is affiliated and the resemblances are explained as borrowings from one of the languages of the group. In the Chad etymologies mentioned above, Angas figures approximately 200 times. Hausa appears in about 80 of these etymologies, but in 120 of them does not. A few of the latter may be incorrect, but hardly all of them or even, I think, a major portion. Most of them are semantically straightforward and involve such recurrent correspondences as Proto-Chad **b* > Angas *p*; **p* > *f*; **d* > *t*; **g* > *k*; **k* > *ɣ/y* depending on the following vowel. Included are 6 of the 7 possessive suffixes, and 4 of the 6 new subject pronouns. Of these, the third possessive singular *-nyi* is not found in Hausa but is found in scores of 'Chad-Hamitic' and 'Chadic' languages; it is cognate with *ni*, the third person masculine singular pronoun of the central Cushitic languages. The possessive pronouns of Angas may serve as a further illustration. The terms meaning 'mine', 'thine', are formed by the addition of the usual bound possessive suffixes to a base *mi-* ~ *mu-*. Bolewa, which is Chad-Hamitic in Westermann's classification, has *mi-* in the same function; Hausa does not have this formation. The possessive pronouns of Angas and Bolewa are cited here along with the possessive suffixes of Hausa, which correspond to the second elements of the Angas and Bolewa pronouns:

	ANGAS BOLEWA HAUSA				ANGAS BOLEWA HAUSA		
1st sing.	<i>mina</i>	<i>mino</i>	<i>-ā</i>	3rd fem.	<i>minyi</i>	<i>mito</i>	<i>-ta</i>
2nd masc.	<i>miya</i>	<i>miko</i>	<i>-ka</i>	1st plur.	<i>munu</i>	<i>mimu</i>	<i>-mu</i>
2nd fem.	<i>miyi</i>	<i>miko</i>	<i>-ki</i>	2nd plur.	<i>muwu</i>	<i>miku</i>	<i>-ku</i>
3rd masc.	<i>minyi</i>	<i>minyi</i>	<i>-sa</i>	3rd plur.	<i>muma</i>	<i>' misu</i>	<i>-su</i>

The Angas base *mi-*, and the suffix of the 1st and 3rd sing. cannot be explained by borrowing from Hausa; if there was borrowing in this case, it must have been

from Bolewa. For the 2nd plur. *wu-* we must go to Buduma and other languages of the Kotoko-Logone subgroup of Chad, which show a differentiation between a 2nd plur. possessive with *w-* like Angas *wu-* and a 2nd plur. verb subject pronoun with *k-* like the Angas subject pronoun *kun* = Hausa *kun*. The same distinction, between 2nd plur. pronouns in *w-* and *k-*, which has been erased in Hausa by the analogical spread of the *k-* form, reappears in Berber *-wən* vs. *-kən*.

It is not only the distribution of the resemblances to Angas among the Chad languages that creates difficulty for the view that borrowing from Hausa is an adequate explanation. The forms taken show every evidence of independent development from reconstructed Proto-Chad and Proto-Afro-Asiatic originals. There are the unique, Grimm-like sound shifts mentioned above. One unusual equivalence, *-in* for non-front vowel + *l* of the other Chad languages, occurs in a number of instances: Angas *kwīn* 'narrow' = Masa *gər*, Kotuko *γul*, Musgu *gulle* 'short'; *bwīn* = Musgu *bul* 'hip'; *čīn* = Logone *gala* 'drive away'; *tīn* = Bata *dule* 'cloud'; *pīn* = Bolewa *bolu* 'break'; *sīn* = Wandala *šallwa*, Musgu *salawɔŋ*, Hausa *saywa* (with regular loss of *l*) 'root'. If Angas borrowed *sum* 'name' from Hausa *sūnā*, the original Proto-Hamito-Semitic final *-m* was obligingly restored, cf. Proto-Semitic **šim*, Bedaue (Cushitic) *sim*, etc. In other words, to account for all these resemblances by borrowing we must resort not to borrowing from a single language but to a kind of itinerant borrowing. Indeed, for a small number of apparently good etymologies where only Angas among the Chad languages has retained a form found in other Hamito-Semitic branches, we must travel still further afield to North Africa and the Near East. And in a few cases, where cognates are found with Ancient Egyptian, we need a time machine.

The theory is thus highly improbable. Moreover, it is arbitrary: given all the facts about the languages concerned, it would be just as logical to exclude Hausa (or any other language) from the Chad group, include Angas, and then explain the resemblances between Hausa and the remaining Chad languages as the result of itinerant borrowing from Angas and the other languages. Implicit in this approach is the fallacy of tacitly taking certain languages as points of reference—because we learned them earlier or because the people who speak them are more interesting or more important, or for some other nonlinguistic reason. This fallacy was noted long ago by Codrington. Speaking of those who deny the Malayo-Polynesian status of the languages of Melanesia and see in them a 'Malay' element, he declared that if they had started out with the languages of the New Hebrides or some other Melanesian area as an assumed point of reference, they would have seen 'Melanesian elements' in Malay, placed Melanesian and Polynesian languages in the same family, and excluded from it Malay and other Indonesian languages.

There are at present only two linguists who attempt to give an overall picture of linguistic relationships in West Africa and in Africa generally—Westermann and myself. It is unfortunate that we differ on a number of points. Some linguists, not specialists in African languages, will wish to draw conclusions

about these areas, and it is unreasonable to expect that they should examine the many languages at first hand. Before they arrive at conclusions on the questions discussed in this review, I hope that they will examine the evidence presented by Westermann in his 1927 work, and also the evidence presented in my *Studies in African linguistic classification*, *Southwestern journal of anthropology* 5.79-100, 190-8, 309-17 (1949), 6.47-63, 143-60, 223-37 (1950).

A last analogy. Suppose that Europe were as much terra incognita to most linguists as West Africa still is, and that two classifications of the languages had been proposed. Linguist A sets up two extensive families, Indo-European and Finno-Ugric, and one isolated language, Basque. (By a happy chance these correspond to Niger-Congo, Afro-Asiatic, and Songhai.) Linguist B sets up ten families and numerous unclassified languages, beginning perhaps as follows: (1) English, German, Dutch, Swedish, Russian; (2) Polish, Czech, Bulgarian; (3) Macedonian?; (4) Greek, Albanian; (5) Hungarian, Slovene; and so on. It is clear that safety does not necessarily lie in numbers. A classification with many families is not ipso facto more cautious than one with fewer families, in the sense of avoiding errors more successfully. In a word, no satisfactory substitute has yet been discovered for examining the evidence.

Die tasmanischen Sprachen: Quellen, Gruppierungen, Grammatik, Wörterbücher. By WILHELM SCHMIDT. (Comité International de Linguistes: Publications de la Commission d'Enquête linguistique; mit Unterstützung der ... UNESCO.) Pp. 521. Utrecht-Anvers: Uitgeverij Het Spectrum, 1952.

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In this work, Father Schmidt has made a very significant contribution to the dialectology, grammar, and lexicon of the Tasmanian language, which has been an object of great interest for many years, not only because of the very limited material culture of its speakers, but also because of the brutal circumstances under which these people became extinct. From the year 1642, when the island was discovered by a Dutch sea captain named Tasman, until 1772, the region was not visited by Europeans; but in 1803 the English established a colony. From then on, for some twenty-five years, the whites employed the most merciless tactics to exterminate the indigenous population. In 1830 an attempt was made to round up the Tasmanians, but this was singularly unsuccessful. At last, in 1835, a certain George Robinson succeeded in winning the confidence of the people and arranged for the only survivors (a total of 203 persons) to be transported to the Island of Flinders. By this time some missionary-minded persons had become interested in the people and attempted to help them; but contact with white men's diseases, inadequate food supplies on the Island of Flinders, and general deterioration in morale led to the extinction of the people in 1876, when the last Tasmanian woman died.

Three factors have lent significance to the study of the Tasmanian culture and language: the extreme simplicity of the material culture, the presumed cultural isolation of the people, and the limited data which have been available.

Toward the end of the 19th century there was a surfeit of idle speculation about Tasmanians as the lowest form of humanity, a subhuman link with the animal world, or a child race. It is quite understandable that Schmidt, who has been so much interested in the historical processes of cultural development, should become vitally interested in the Tasmanian language, which as an essential part of the indigenous culture is basic to his theories about primitive societies. All linguists and anthropologists are deeply indebted to Schmidt (and his colleagues) for having undertaken such a thorough study of the available source materials and for having classified and presented them in such a usable form.

In the first six pages of Part I Schmidt deals with the anthropological significance of the Tasmanian speech (and indirectly of the culture), and then proceeds to demolish the theories of such men as E. M. Curr (*The Australian race*; Melbourne and London, 1887), Hermann B. Ritz (*The speech of the Tasmanian aborigines, Papers and proceedings of the Royal Society of Tasmania*, 1909), and Alfredo Trombetti (*I linguaggi estinti della Tasmania*; Bologna, 1926). Curr attempted to relate Tasmanian to some African languages, while Ritz contended that the Tasmanian language was 'but one step removed from the articulate cry of an infant'. Ritz declared that sounds such as *l*, *m*, *n*, *r*, and *ng* indicated motion, while *b*, *p*, *w*, and *m* showed motion before rest. Motion after rest was supposedly expressed by *g*, *k*, *w*, *r*, and *ng*, and rest by itself was symbolized by *d*, *t*, *l*, and *n*. In order to explain the numerous exceptions to these rules, Ritz insisted that 'members of each group are readily interchangeable'. Trombetti did not engage in idle speculation about 'childish speech', but he was no less selective in his use of the data, for he attempted to relate Tasmanian not only to languages in Australia, Papua, Southeast Asia, and the Andaman Islands, but also to languages in Africa and the Western hemisphere.

Schmidt's treatment of his sources (17-38) is typical of his careful compilations of widely scattered data. Unfortunately, the data provide no sure means of sorting the word lists and sentences by dialects. No doubt some of the sources represent mixed lists, but Schmidt has been quite ingenious in ferreting out the tangled and none too obvious relationships. Not the least of the difficulties in the analysis was the interpretation of the phonetic values of the sounds transcribed in a number of different ways by different persons, often inconsistently by the same person.

In discussing the grouping of dialects (38-61), Schmidt considers previous statements made by nonlinguists (including George Robinson). He concludes that there were five main dialects, grouped in two main divisions, an arrangement which is supported by the geographical distribution. The proof of this division of dialects is accompanied by a list of all the extant dialectal variants of 139 terms (62-103).

Part II (104-220) treats the phonetics, morphology, and syntax of Tasmanian. It is difficult to arrive at satisfactory conclusions regarding the sounds, especially the vowels, since most of the source material comes from linguistically untrained speakers of English. Schmidt concludes that there must have been ten vowels, of which some could occur either long or short. In the matter of consonants Schmidt is on somewhat safer ground, but failure to interpret the data in terms

of phonemic contrasts results in some significant oversights.¹ For example, Schmidt makes much of the inconsistent writing of voiced and voiceless stops; for him, this indicates that the Tasmanian sounds were halfway between voiced and voiceless. If, on the other hand, he had assumed that Tasmanian had no voiced-voiceless contrast, the inconsistent writing would have made more sense, since the sound may have been spoken and heard either as voiced or as voiceless. One sound, written variously as *gh*, *g*, *w*, or omitted entirely, is interpreted as being similar to the Arabic pharyngealized voiced spirant *ayin*. This 'strange' sound, however, occurs only in word-medial position. Another sound, generally written as *k* or *ck*, is described by Schmidt as also having an unusual distribution: contrary to the customary open-syllable structure, it terminates many syllables, especially in word-final position. The phonetic hints and the distributional data seem to indicate that what Schmidt regards as medial and final exceptions to the rule may be really one and the same phoneme, probably a glottal stop, which in medial position is relatively weak (hence frequently not written) but in final position is quite strong.

In order to determine any possible 'laws of sound change' (and hence relationships between dialects), Schmidt has painstakingly summarized the frequencies of sounds occurring in the words listed in the dictionary. On the whole, the results of this analysis corroborate the division into dialects described in the previous section.

Schmidt goes into great detail concerning any scrap of evidence that might indicate a complex morphological structure. Even at best, the results are meager, for the data consist primarily of lists of isolated words. It is also possible that the Tasmanians purposely used a very simple form of speech in dealing with foreigners (220). Nevertheless, the evidence is sufficient to indicate clearly that Tasmanian was a fully developed language, not the primitive gibberish that some had seen in it. Even the restricted data show that Tasmanian had several morphologically distinguishable classes of words, including personal pronouns (indicating subject, object, and possession), demonstrative pronouns, nouns (with several different suffixes, among them those indicating possession, location, and possibly direction), adjectives (again with several different suffixes, the meanings of which are not certain), numerals, postpositions, and verbs (indicating some modes, and having infinitive and participial forms).

Part III (221-468) consists of a Tasmanian-German dictionary with copious notes and long lists of the variants found in the different sources. Part IV (469-521) is a collection of Tasmanian words grouped under headings such as Religion, Animals, Body parts, and Verbs, and subdivided by dialects.

In the body of the book Schmidt carefully avoids commenting on the relationship of Tasmanian to other languages, but in the Preface he indicates clearly that he does not regard it as related to the languages of Australia or Oceania. The possibility of its being related to some languages of New Guinea is suggested, but rejected as unlikely. Schmidt states emphatically that the Tasmanians were

¹ Schmidt indicates in the Preface that this work was completed about 1920 and was only superficially revised for publication. It is unfortunate that the data were not reinterpreted on the basis of phonemic principles.

'doubtless the oldest people of the South Seas' (11), but he does not make it clear exactly what he means by 'oldest'. He hints at his meaning, however, by saying that they remained 'pure, unmixed, and uninfluenced by later events'. He obviously is not referring solely to the people's isolation over a long period of time, but to the supposed static quality of their culture, for it is upon this general premise that Schmidt has constructed so much of his hypothesis concerning historical relationships. Nevertheless, the linguistic evidence cited in his book neither affirms nor denies the age of the Tasmanian peoples (irrespective of what 'age' might mean), but it does provide proof that Tasmanian was as fully developed as any other language spoken by a 'primitive' or by a 'civilized' people. Schmidt has succeeded not only in amassing and classifying a considerable body of data, but in demonstrating conclusively that there is no relationship between poverty of material culture and the alleged simplicity of 'primitive tongues'.

Humanisme en taalkunde. By L. L. HAMMERICH. (*Scripta academica Groningana: Aula-voordrachten, No. 2.*) Pp. [vii], 46. Groningen, Jakarta: J. B. Wolters, 1952.

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In February 1952, L. L. Hammerich of the University of Copenhagen delivered a series of four guest lectures at the University of Groningen. They were addressed to a general university audience, and anyone who, like your reviewer, has had the privilege of listening to Hammerich give a semipopular lecture, in excellent Dutch, can well imagine that his hearers went away enthusiastic and delighted. The four lectures, with ample notes added, were later printed in the little volume here under review. For purposes of research, linguists will of course turn to Hammerich's more strictly professional writings. But for the insight which they give into Hammerich's broad range of interests, and into the wide sweep of his scholarly imagination, these four lectures will also be of interest to our profession. They show us Hammerich, respectively, as general linguist, as literary critic and analyst, as Germanist and Indo-Europeanist, and as scholar in the field of Eskimo dialects.

Lecture 1 (*Taal en mens*) takes up the fascinating topic of numbers, and of the differences between mathematical and linguistic counting systems. Mathematically, every number occupies a fixed position in a $1 + 1 + 1 \dots$ system; linguistically, such expressions as Finnish 'ten less one' for '9', Germanic 'ten remainder one' for '11', or Latin 'one from twenty' for '19', suggest that some quantity expressions first existed outside of any fixed counting systems, and were only later incorporated into them.

Lecture 2 (*Taal en stijl: Der Ackermann aus Böhmen*) gives a most appealing literary analysis of this powerful Early New High German text, which Hammerich has recently helped to edit. The work is skillfully placed in its literary setting, and its structure is interpreted in terms of what we know of the author's life.

Lecture 3 (*Over de taal van onze voorvaderen: Het Indogermaans*) uses the

Stammbaum theory to work back in time from Dutch to Southern West Germanic, to West Germanic, to Germanic, to Italo-Celtic-Germanic, to Centum, to Centum-Satem (Proto-Indo-European), and even beyond. Perhaps because it is much more technical than the others, linguists will also find in this lecture much more to criticize. Nearly all linguists will disagree with Hammerich's assumption of a single laryngeal for Pre-Indo-European; cf. Sturtevant's remarks in *Lg.* 25.290-1 (1949), Martinet's in *Word* 6.184-6 (1950). Many will also feel that he has vastly overworked the Stammbaum theory, with its unrealistic assumptions of unified proto-languages and repeated clean splits into daughter languages. The theory is of course indispensable for comparative work, but it might better be presented to semipopular audiences not as 'the truth', but simply as a way of stating the relationships among languages. Eighty years after Johannes Schmidt's postulation of the wave theory, and with a solid half century of dialect geography behind us, we can no longer accept the Stammbaum theory in the same way that our predecessors did. The recent work on Germanic (particularly West Germanic) by Maurer, Frings, Schwarz, and others shows a much more realistic approach.

The final lecture (Oertijd en nu: Eskimo, taal en volk) may well have been the most interesting for Hammerich's audience. Without implying at all (as the title might indicate) that Eskimo exemplifies a stage through which the languages of more complicated cultures must have passed, Hammerich simply gives as good a picture as he can, in his allotted time, of the language of the Eskimos, with remarks on phonology, morphology, form classes, syntax, etc., plus a sample text with translation.

Despite the title of the series, there is no common theme running through all four lectures. One can well believe, however, that Hammerich's personality gave them a unity which is now somewhat lost on the printed page.

L'époque réaliste; première partie: Fin du romantisme et Parnasse. Par CHARLES BRUNEAU. (Histoire de la langue française des origines à nos jours, [par FERDINAND BRUNOT], Tome 13.) Pp. xvi, 384. Paris: Librairie Armand Colin, 1953.

Reviewed by ALPHONSE G. JUILLAND, *University of Washington*

Les 10 premiers tomes de cette vaste *Histoire* ont été rédigés par Ferdinand Brunot, qui a mené son oeuvre jusqu'au début du XIX^e siècle. Le tome 11, *Le français au dehors sous la Révolution, le Consulat, l'Empire*, toujours de Brunot, paraîtra bientôt par les soins de son disciple Charles Bruneau, à qui l'on doit déjà le tome 12, *L'époque romantique (1815-1852)*, et la première partie du tome 13, *L'époque réaliste (1852-1885)* qui, sous le titre *Fin du romantisme et Parnasse*, fait l'objet du présent compte-rendu. Les derniers tomes, 14 et 15, actuellement en préparation, porteront respectivement sur la première (1885-1905) et sur la seconde (1905-40) périodes symbolistes: 'L'oeuvre conçue par Ferdinand Brunot à l'aube du XX^e siècle se trouvera ainsi achevée en ce qui concerne la langue littéraire' (viii).

Cette dernière précision est importante, surtout pour celui qui, n'ayant pas

pris connaissance du volume précédent, n'a pas suivi les discussions qu'il a soulevées. Le lecteur pressé, qui aura négligé de parcourir la Préface (v-viii), sera sans doute surpris par le caractère de cet ouvrage présenté sous le titre trompeur d'histoire de la langue lorsqu'il s'agit en réalité d'histoire de la langue littéraire. En effet, celui qui s'attend à retrouver, à plus vaste échelle, le type d'exposé que l'*Esquisse d'une histoire de la langue latine* ou l'*Aperçu d'une histoire de la langue grecque* de Meillet ont rendu classique, le germaniste qui envisage une oeuvre entreprise dans l'esprit de la *Geschichte der deutschen Sprache* de Hermann Hirt ou le romaniste qui escompte, en plus développé, quelque chose de l'ordre de la magistrale *Die rumänische Sprache* de Sextil Pușcariu, sera déconcerté par la conception de l'histoire de la langue illustrée par cet ouvrage. Car l'auteur ne s'occupe que de la langue des écrivains—et encore seulement des écrivains réputés, dont il sélectionne les oeuvres les plus achevées¹—et les seuls matériaux qu'il utilise sont les textes littéraires. Par rapport aux tomes rédigés par Brunot, le changement de perspective est évident. Il suffit, pour s'en rendre compte, de passer en revue les têtes de chapitres du volume 6, *Le XVIII^e siècle*, par exemple. Ils sont intitulés dans l'ordre: Philosophie, Économie politique, Agriculture, Commerce, Industrie, Politique, Finances (fascicule premier), La langue des Sciences, La langue des Arts (fascicule deuxième). Il apparaît clairement que l'histoire de la langue de Brunot est devenue histoire de la langue littéraire avec Bruneau. Celui-ci le signale d'ailleurs explicitement à la fin de sa Préface et, malgré un titre trompeur qui lui a été imposé par voie d'héritage, c'est en tant qu'histoire de la langue française littéraire que j'examinerai son ouvrage.

1. STYLISTIQUE ET LINGUISTIQUE

1.1. Importance de l'ouvrage. Cet ouvrage est doublement important: tout d'abord, en tant que contribution à l'histoire du français littéraire au XIX^e siècle; ensuite—et c'est peut-être ici que réside son principal intérêt—par la doctrine stylistique que se trouve à sa base. Comme l'un des recenseurs du volume précédent le pressentait déjà, 'sous le titre d'*Histoire de la Langue* tome XII, il [Bruneau] pourrait bien nous avoir donné l'illustration d'une nouvelle méthode "stylistique" appliquée à l'étude du mouvement littéraire' (Gérald Antoine, En marge d'un livre récent: Stylistique et histoire de la langue à l'époque romantique, *Revue d'histoire littéraire de la France* 53.175-87 [1953]). Ce qui, dans le tome 12, n'était que vaguement entrevu est devenu depuis une certitude: entre temps, Bruneau a pris conscience d'une doctrine qui n'était auparavant que latente et, dans une étude-manifeste qui a suscité beaucoup de commentaires et à laquelle je me reporterai dans les paragraphes suivants (La stylistique, *Romance philology* 5.1-14 [1941]), il l'a explicitement formulée en prenant position vis-à-vis des autres doctrines stylistiques et principalement contre la 'stylistique littéraire' (ou 'néostylistique', ou 'criticisme stylistique') représentée par Leo Spitzer, Damaso Alonso, et Helmut Hatzfeld.

Il n'est pas dans mon intention de discuter ici la théorie stylistique esquissée

¹ Un passage comme le suivant, qui ne manquera pas de surprendre le linguiste, est caractéristique de cette façon de faire: 'Banville, qui a vécu de sa plume, a publié plus de prose que de vers; mais le poète seul mérite d'être retenu par l'historien de la langue' (55)!

par Bruneau dans l'étude mentionnée, car je préfère maintenant la juger d'après les résultats obtenus en pratique, tels qu'il sont consignés dans le livre qui nous occupe. Mais comme c'est en premier lieu au nom de la rigueur scientifique que Bruneau rejette les 'stylistiques impressionnistes' qui, à son avis, ne présentent pas les garanties de certitude offertes par les sciences exactes,² et comme parmi ces dernières c'est de la science du langage qu'il se déclare,³ il n'est pas sans intérêt de confronter dans les termes posés par lui-même la stylistique avec la linguistique, la science des faits de langue littéraire avec la science des faits de langue tout court. D'autant plus que, ces derniers temps, des voix de plus en plus nombreuses se sont élevées pour réclamer l'application des techniques d'investigation linguistique aux faits littéraires, aux faits de style.⁴

² Les essais de concevoir l'étude de la littérature comme une science de type plus ou moins exact remontent assez loin dans le passé. On trouvera une bibliographie substantielle des essais de cet ordre dans le livre de René Wellek et Austin Warren, *Theory of literature* (New York, 1949); et aussi dans le livre de Helmut Hatzfeld, *A critical bibliography of the new stylistics, applied to the Romance literatures, 1900-1952* (Chapel Hill, 1953). Un effort récent: Guy Michaud, *Introduction à une science de la littérature* (Istanbul, 1950).

³ 'J'essayerai, en me plaçant du point de vue strictement linguistique, à définir exactement le champ des études de la stylistique' (*La stylistique* 1).

⁴ L'application des méthodes linguistiques dans l'analyse des faits littéraires a été tentée plus d'une fois par des savants travaillant dans l'esprit de la linguistique traditionnelle. En Europe, l'enseignement saussurien a directement engendré la stylistique genevoise, dont le fondateur et le principal animateur a été Charles Bally; voir en dernier lieu Robert Godel, *La stylistique est-elle une science linguistique?*, *Cahiers Ferdinand de Saussure* 11.4-5 (1953). La glossématique hjelmslevienne a fourni les principes de bases de plusieurs esquisses de théories structurales de la littérature; par exemple A. Stender-Petersen, *Esquisse d'une théorie structurale de la littérature*, *TCLC* 5.277-387 (1949); Svend Johansen, *La notion de signe dans la glossématique et dans l'esthétique*, *ibid.* 288-302; Johansen, *Le symbolisme, étude sur le style des symbolistes français* (Copenhague, 1945); Knud Togeby *L'oeuvre de Maupassant* (Copenhague et Paris, 1954).

L'école structurale dont l'enseignement s'est avéré le plus fructueux sur ce terrain a été cependant l'école de Prague. Combiné avec le formalisme littéraire russe, le fonctionnalisme pragois a donné naissance au structuralisme littéraire tchèque, avec à sa tête Roman Jakobson et Jan Mukarovsky, qui a décisivement influencé l'école littéraire polonaise dite 'intégraliste' fondée par Manfred Kridl et Franciszek Siedlecki; pour les principes fondamentaux de ces écoles, voir William E. Harkins, *Slavic formalist theories in literary scholarship*, *Word* 7.177-85 (1951).

Aux États Unis, la linguistique a inspiré plusieurs essais de reconsidération des faits littéraires dans l'esprit behavioriste. A retenir: M. Joos, *Verbal parallels and recurrent images*, dans le livre de M. L. Hanley, *Word index to Joyce's Ulysses* (Madison, Wisconsin, 1937 [1944]); A. A. Hill, *Toward a literary analysis*, in *English studies in honor of James Southall Wilson*, ed. by Fredson Bowers (Charlottesville, 1951); H. Whitehall, *From linguistics to criticism*, *Kenyon review* 1951.710-4; Hill, *A sample literary analysis*, *Monograph series on languages and linguistics* [Georgetown University] 4.87-93 (1953); W. F. Twaddell, *The Kerker lexicon and the Gretchen episode*, *Monatshefte* 45.354-70 (1953); Eugene Dorfman, *The Roland and the Cid: A comparative structural analysis* (Columbia diss. 1950, Microfilm publication No. 1843).

Pour des renseignements bibliographiques supplémentaires, voir A. Juilland, *Bibliographie de la linguistique fonctionnelle et structurale* (thèse complémentaire de Sorbonne 1951, MS). Pour une critique des méthodes structurales en matière d'analyse littéraire, voir D. Alonso, *Poesia española: Ensayo de métodos y límites estilísticos* (Madrid, 1950), qui rejette le fonctionnalisme troubetskoyen aussi bien que les autres doctrines linguistiques modernes.

1.2. La 'stylistique pure'. La stylistique proprement dite est, d'après Bruneau, la stylistique pure, dont il nous dit qu'elle 'appartient à la catégorie des sciences humaines' (La stylistique 2). Et, suivant Matoré, l'auteur 'l'encadre entre la Sociologie, la Psychologie sociale, la Psycho-physiologie, d'une part, et d'autre part, la Linguistique statique et la Lexicologie (historique, descriptive)'. Le linguiste, qui sait à quel point les efforts pour faire de la linguistique une science autonome—'immanente' ou 'pure'—et pour surprendre le fait linguistique dans sa structure propre se confondent justement avec les efforts pour débarrasser cette discipline des hypothèses logique, psychologique, sociologique, biologique, et physiologique, restera un peu rêveur devant l'assurance avec laquelle Bruneau place la stylistique 'pure' justement au carrefour des disciplines voisines ou postulées telles. Si une stylistique pure est possible—et ceci reste encore à prouver—ce n'est en tout cas pas du côté de ces disciplines hétérogènes qu'elle trouvera ses points d'appui et de repère. L'enseignement que nous offre la linguistique, qui guide Bruneau, est sur ce point formel: c'est plutôt dans une réaction anti-psychologique, -sociologique, -physiologique, etc., que la stylistique risque de trouver un jour son immanence et sa 'pureté'.

1.3. La stylistique comme science autonome. Les disciplines 'auxiliaires' une fois écartées—et en fait Bruneau les écarte—est-ce que la linguistique pourrait, elle, offrir à la stylistique des bases solides en vue de l'établissement d'une science autonome des faits littéraires? Bruneau en semble persuadé, et il est loin d'être le seul. Pour lui, comme pour beaucoup d'autres stylisticiens qui ne sont pas nécessairement des linguistes, la science du langage paraît seule susceptible de fournir à la stylistique les techniques d'investigation rigoureuses qui, en ces matières, puissent permettre 'de "savoir" et, dans une certaine mesure de "prévoir"' (ibid. 7). Dans ces conditions, pour qu'elle puisse éviter le double-emploi avec la linguistique et jouir d'un statut indépendant, la stylistique doit disposer avant tout d'un domaine propre, d'un objet spécifique: ce serait le fait de langue littéraire (ou fait de style), opposé au fait de langue tout court, qui forme l'objet propre de la linguistique. Mais comme il se trouve que fait de langue et fait de style coïncident matériellement, c'est bien de techniques d'investigation différentes que l'on attend la différenciation du même objet naturel en deux ou plusieurs objets de science. Or, sur ce terrain, la stylistique ne peut rien emprunter d'essentiel à la linguistique, sous peine de se voir entièrement assimiler par celle-ci: les deux objets de science étant 'naturellement' identiques, le rôle de la méthode devient décisif, car c'est bien elle qui, en dernier lieu, 'ratifie' l'objet naturel pour lui conférer la dignité d'objet de science. En opérant avec des méthodes linguistiques sur n'importe quel objet, on ne peut faire que de la linguistique: si un objet quelconque, postulé inconnu, 'répond' et 'se prête' aux méthodes linguistiques, c'est que l'objet en question est une langue, ce qui rend inutile une science indépendante comme la stylistique pure. Si le fait envisagé, littéraire en l'espèce, est autre chose que le fait de langue, il échappera de ce fait même aux méthodes d'investigation linguistique: pour les méthodes linguistiques, les faits de langue littéraire ne sont que des faits de langue purs et simples. Et si le fait littéraire est un certain fait de langue, c'est à dire un fait de langue pourvu d'un certain caractère différentiel, ce caractère échappera néces-

sairement aux méthodes linguistiques: la perspective linguistique réduira à deux dimensions un objet qui est supposé en avoir au moins trois.

1.4. Méthodes linguistiques et analyse stylistique. Il est aisé d'en fournir la preuve: ceux qui veulent utiliser strictement les méthodes linguistiques dans les investigations littéraires oublient trop souvent que, en soumettant l'oeuvre (ou un 'texte'), de Hugo par exemple, à une analyse linguistique rigoureuse, le résultat en sera—une grammaire: à peu de chose près, la grammaire de la langue française telle qu'elle se pratiquait dans les milieux littéraires à l'époque en question. Il est vrai qu'on pourrait espérer que cette grammaire sera une CERTAINE grammaire, la grammaire de Hugo en l'espèce, et qu'elle s'opposera à certaines autres grammaires, la grammaire de Gautier par exemple: les oppositions entre ces 'grammaires' rendront compte de certaines différences dont on désignera l'ensemble comme 'style'. Les méthodes linguistiques ayant ainsi isolé certains faits qu'on désignerait comme 'littéraires', le style d'un écrivain (ou sa 'langue') serait à définir comme l'ensemble des traits différentiels qui opposent et distinguent le style d'un certain écrivain du style d'un certain autre. Cette définition serait oppositive et relative, donc variable: dans cette conception, la définition du 'style' ou de la 'langue' d'un auteur (ou d'une 'école', ou d'une 'époque') variera par rapport à la langue de l'auteur (etc.) opposé. La définition de la 'langue' de Hugo, par exemple, ne sera donc pas une et invariable, ainsi qu'on la conçoit généralement dans les diverses stylistiques: tel trait de la prose hugolienne, pertinent lorsqu'on l'oppose à Gautier, deviendrait non-pertinent par rapport à Michelet et, vice-versa, tel trait irrélevant par rapport à Gautier deviendrait distinctif pour le 'style' de Hugo confronté avec Michelet.

Cette conception du 'style' aurait pour conséquence un renversement total de perspective dans l'analyse des faits littéraires: à la littérature comparée, telle qu'elle se pratique de nos jours, on substituerait une littérature opposée. Aux investigations littéraires courantes—basées essentiellement sur la recherche des 'sources', des thèmes, des motifs, etc., et visant surtout à l'établissement des 'filiations', donc des identités—où les éléments différentiels sont envisagés comme une masse amorphe et indifférente qui sert de support aux traits communs pourvus, eux, de la valeur définitoire, on opposerait une méthode où les éléments communs—de substance (thèmes, motifs) ou de forme (procédés, types, figures, tours)—s'estomperaient pour céder la place aux éléments différentiels qui, passés au premier plan, assumeraient la fonction identificative.

On renverserait ainsi les termes des équations établies par la littérature comparée, où ce sont les éléments communs qui sont les termes marqués s'opposant aux traits différentiels, conçus comme termes neutres, non-marqués: comme en linguistique fonctionnelle, les éléments différentiels deviendraient les termes marqués qui servent à définir.

Il s'agirait, théoriquement, de refaire le même chemin en sens inverse: au lieu de partir du particulier à la recherche du principe réducteur qui puisse conduire au général, au lieu de descendre des ramifications multiples vers la racine unique, on partirait des éléments communs comme d'une base neutre pour remonter de proche en proche, de différence en différence, jusqu'aux ramifications extrêmes et individuelles de l'arbre typologique littéraire.

1.5. Inconvénients des méthodes linguistiques en stylistique. Aussi séduisante qu'elle paraisse en théorie, cette procédure est frappée en pratique de certaines limitations qu'il convient de mettre en évidence. En effet, du point de vue différenciatif, la phonologie et la morphologie ne donneraient naturellement rien, car il faudrait opposer des langues d'écrivains fort éloignés dans le temps pour éviter la quasi-identité de l'analyse, qui serait d'ailleurs stylistiquement inconcluante. Quant aux différences de syntaxe et de vocabulaire (on devrait introduire aussi les 'figures stylistiques') dont on attend le verdict décisif, elles seraient, au moins dans l'inventaire, considérablement réduites: car presque chaque élément lexical, tour syntaxique ou figure stylistique caractéristique de la 'langue' de Hugo se retrouvera au moins une fois chez Gautier, et vice-versa. De sorte que, même sur ces terrains supposés favorables, on sera réduit, pour rendre compte des différences de style, à recourir à des données statistiques: telle construction, rare chez Gautier, apparaîtrait avec une fréquence x chez Hugo et x^2 chez Michelet. Sans nier la grande importance des faits de fréquence dans les investigations stylistiques et littéraires, il reste toujours que c'est justement la linguistique qui nous enseigne que les faits de cet ordre ne sont pas analytiquement décisifs. Et, qui plus est, ce n'est pas sur la statistique linguistique qu'on pourra bâtir une stylistique pure.

Il y a plus: une telle procédure renferme, d'autre part, certains dangers qu'un maniement prudent permettra néanmoins d'éviter jusqu'à un certain point. En effet, la grande majorité des différences est prévisible dans le domaine des 'innovations' (de vocabulaire, de syntaxe, de 'style'). Or, les innovations 'heureuses', celles qui ont une 'valeur' dans la mesure où elles correspondent à l'esprit de la langue (autrement dit à ses lois de structure), sont justement celles qui ont été ratifiées par la langue, c'est à dire adoptées par les autres écrivains pour pénétrer dans le patrimoine linguistique et littéraire commun. Il s'en suit que, du fait même de leur validation, les faits les plus significatifs risquent de devenir irrélévants du point de vue définitoire puisque se retrouvant ailleurs, ils auront perdu leur valeur différenciatrice. Resteront donc, comme faits caractéristiques et analytiquement décisifs, les 'innovations' malheureuses, celles que la langue (je veux dire les confrères et l'histoire) a condamnées: par une telle procédure, on risque d'être amené à juger un auteur dans ses exagérations et abus, donc dans ce que sa langue a de négatif, sinon de moins caractéristique.

1.6. La stylistique comme 'science des écarts'. Ceci nous conduit tout droit à la définition de la stylistique comme la 'science de écarts' (ibid. 6). Le fait stylistique étant considéré, dans cette conception, comme un écart par rapport à l'usage, comme un enfreignement de la norme linguistique,⁵ ceci equivaut à

⁵ Ceci découle du caractère paradoxal de la norme (linguistique, littéraire, esthétique ou autre): synchroniquement conçue, elle repose sur un jugement arbitraire; historiquement définie, elle s'appuie sur un jugement circulaire.

Pour ce qui est de la norme linguistique, Bernard Bloch a plus d'une fois souligné son caractère arbitraire et protesté contre l'emploi abusif qu'on en fait. En effet, la norme linguistique n'a pas de valeur absolue: en termes purement descriptifs, l'apparition de l' 'écart' annule la 'norme' du fait même de son apparition. La seule conception descriptive de la norme serait celle offerte par les données statistiques, qui implique néanmoins une solution arbitraire: ayant abandonné les critères d'existence et de non-existence en faveur

donner du 'stylistique' une définition négative: dans un 'texte', est stylistique ce qui n'est pas un fait de langue brut. Ceci nous indique par quelles voies on pourrait éventuellement utiliser les méthodes proprement linguistiques dans l'analyse des faits littéraires: en soumettant une oeuvre littéraire à une analyse strictement linguistique, tout ce qui, dans le 'texte', se prête aux critères de cet ordre est un fait de langue pur et, de ce fait même, ne saurait constituer un fait de style. Le stylisticien pourra ainsi, à l'aide des méthodes linguistiques, isoler son champ opératoire: l'application d'une telle procédure lui permettra d'ignorer dans l'analyse purement littéraire les faits identifiés au cours de l'analyse linguistique et de se concentrer uniquement sur les faits résiduels, ceux qui ont résisté ou échappé aux critères linguistiques. Dans cette phase préliminaire, le stylisticien sera amené à considérer les faits de style d'une manière transcendante—en les rapportant aux faits de langue—et à opérer provisoirement avec des jugements négatifs: le 'fait de style' sera le fait non-linguistique et cette 'métastylistique' sera conçue comme une sorte de contre-grammaire.

Ce n'est que dans une seconde phase, qui constituerait la stylistique proprement dite, que le stylisticien se libérerait de l'hypothèque linguistique pour s'attaquer aux éléments résiduels par une méthode propre, assortie à la spécificité de l'objet littéraire. Il pourra espérer de jeter ainsi les bases d'une discipline immanente et, éventuellement, de la stylistique pure entrevue par Bruneau.

1.7. Inconvénients de cette conception. Remarquons cependant que, même dans cette phase préliminaire, les méthodes linguistiques devront être utilisées avec prudence, car elles recèlent plus d'un danger et pourraient compromettre l'analyse stylistique proprement dite. Il se peut que l'écart, qui n'est pas un fait de langue dans le sens défini ci-dessus, ne soit pas un fait de style non plus, mais quelque chose d'autre—à définir par la suite—, une faute par exemple. Et ce n'est pas en définissant avec Bruneau, d'après Valéry, l'écart comme un certain genre de faute, comme 'une faute voulue' (ibid. 6) qu'on évitera les dangers. Car Valéry n'étant plus parmi nous, invitons Bruneau—qui semble d'ailleurs opposé à l'introspection (ibid. 7)—à nous fournir les critères objectifs susceptibles de distinguer en ces matières ce qui est voulu de ce qui ne l'est pas. N'oublions pas que ce qui était voulu—un écart—hier, est involontaire—la norme—aujourd'hui; que même en synchronie, ce qui est voulu, donc conscient, chez un sujet, est mécanique chez l'autre; enfin, que chez le même sujet, l'écart voulu dans certaines circonstances devient mécanique dans certaines autres.

Ce n'est pas tout: la conception de la stylistique comme 'science des écarts'—qui implique le rejet des faits 'normaux', abandonnés à la linguistique—peut brouiller irrémédiablement la perspective d'ensemble des faits littéraires. Car le même fait matériel, identifié par les critères linguistiques comme un fait de langue pur et simple, peut, lorsqu'il est placé dans un autre éclairage et pris dans

des critères de fréquence, il est impossible d'établir, en termes quantitatifs, une frontière précise entre la norme et l'écart.

Reste la solution dynamique, qui consisterait à concevoir la norme et l'écart en termes de procès. Cette solution ne nous avance malheureusement pas beaucoup, car elle repose sur un jugement circulaire: après avoir défini l'écart par rapport à la norme (descriptivement établie), on ne peut définir la norme que par rapport à l'écart.

un réseau de relations différent, être validé sur un plan autre, sur le plan littéraire en l'espèce. En ignorant dans l'analyse littéraire un fait linguistiquement 'banal' (c'est à dire correspondant à la 'norme' linguistique), on risque d'omettre un élément constitutif de la structure littéraire (à déterminer).

C'est pourquoi il me semble que l'utilisation des méthodes linguistiques dans l'analyse des faites littéraires doit être pratiquée avec beaucoup de prudence: de plus, leur portée semble limitée au travail préparatoire, au nettoyage du terrain, et elle restera peut-être sans répercussion directe sur l'analyse stylistique proprement dite.

1.8. La stylistique comme 'science exacte'. Une stylistique telle que Bruneau et tant d'autres la réclament, c'est à dire une discipline d'un type analogue aux sciences dites 'exactes', est-elle d'ailleurs possible? Au moins dans l'état actuel de nos connaissances et avec les méthodes d'investigation stylistique dont nous disposons à ce jour, il me semble qu'il faille répondre, contre Bruneau, par la négative. Car ceux qui veulent faire profiter à la stylistique des surprises conquêtes de la linguistique moderne pour en faire une discipline également rigoureuse n'oublient que trop souvent la différence essentielle qui oppose celle-ci à celle-là, comme d'ailleurs à toutes les autres sciences exactes: à savoir, que les sciences dites 'exactes'—la linguistique comprise—opèrent avec des jugements d'existence, lorsque la stylistique (ou la science des faites littéraires) opère essentiellement avec des jugements de valeur. Le linguiste se contente d'enregistrer, il doit parfois interpréter, il n'a jamais à évaluer. Par contre, ayant à faire à un objet indissolublement lié au problème du 'beau', le stylisticien ne saurait se contenter d'enregistrer et de classer: il doit en outre rendre compte de la valeur des faits enregistrés, ce qui bouleverse entièrement la perspective. Cette difficulté ne semble pas étrangère aux préoccupations de Bruneau, pour lequel 'la stylistique ne peut pas se désintéresser de cette question de valeur' (ibid. 7).

1.9. La stylistique comme 'science des valeurs'. Il s'en suit que la possibilité de faire de la stylistique une science rigoureuse, de type plus ou moins 'exact', dépend entièrement de la possibilité de transposer les jugements de valeur (littéraire) en jugements d'existence (littéraire). Aussi longtemps qu'on n'aura pas trouvé une méthode adéquate pour opérer cette transposition, les exigences de Bruneau sont prématurées: le recours à la linguistique sera frappé de nullité et la stylistique ne saura en aucune façon prétendre à la dignité de science véritable.⁶ Il n'est pas le lieu de discuter ici par quelles voies une telle transposi-

⁶ Il n'est pas sans intérêt de signaler ici, d'après Helmut Hatzfeld, la position d'avant-garde de l'un des pionniers de la 'science exacte' de la littérature qui, malgré ce qu'elle a de prématuré, voire d'utopique, ne manquera pas de séduire au moins en théorie, les chercheurs anti-mentalistes s'inspirant de l'enseignement bloomfieldien. Michel Dragomirescu, *La science de la littérature* (3 vols.; Paris, 1928), 'has the most radical viewpoint on stylistics which he calls literary technology. He believes that having selected and analyzed the masterworks of world literature from all angles with an aesthetic, strictly anti-historical approach, one must arrive at an aesthetic-critical synthesis which is self-evident to everybody, so that there will be "only one opinion as to value and character of a master work. Then only will happen in the Science of Literature what occurs quite naturally in the Natural Sciences" (I, 169), namely there would be no history of masterworks, as there is no history of minerals.' (Hatzfeld, *A critical bibliography of the new stylistics* 262).

tion pourrait offrir, sinon une solution, du moins quelques lumières, dans le renversement de perspective par lequel les écoles structurales ont réussi à transposer les jugements basés sur le fameux 'sentiment linguistique' en jugements d'existence linguistique. Peut-être que, de manière analogue, la stylistique arrivera un jour à faire éclater le 'goût littéraire'—qui est en quelque sorte le pendant du 'sentiment linguistique'—en explicitant l'ensemble des jugements littéraires latents qui l'engendrent.⁷

1.10. Linguistique et stylistique. Contrairement à la tendance générale et aux vues exprimées par Bruneau, il résulte de ces considérations d'ordre général que si la linguistique a de précieux enseignements à fournir à la stylistique, ce n'est certainement en lui prêtant ses méthodes. Ce que la linguistique offre—et non seulement à la stylistique—c'est une certaine façon d'approcher les faits, un certain esprit de soumission à la spécificité de l'objet, visant avant tout à le surprendre dans ce qu'il a de propre, de caractéristique, de sui generis. Le linguiste qui sait qu'il n'a réussi à rendre sa science autonome qu'en s'élevant contre les servitudes imposées par la logique, la psychologie, la sociologie, et la physiologie, déconseillera vivement au stylisticien à la recherche d'une doctrine 'pure' de placer son objet au carrefour de ces disciplines. Au contraire, il l'encouragera d'essayer par tous les moyens de rompre (au moins provisoirement) les ponts avec les disciplines voisines et, à partir d'un certain point, avec la linguistique elle-même. Quitte à les reconstruire plus tard, à partir de sa propre rive et à l'endroit choisi par elle-même. Autrement dit, le dialogue ne sera repris que plus tard, lorsqu'elle pourra le faire d'égal à égal et dans ses propres termes. Et c'est peut-être dans ce mouvement temporaire de libération que la science du style se retrouvera.

Sur ce point, je me sépare donc très nettement de Harold Whitehall, pour lequel 'no criticism can go beyond linguistics' (From linguistics to criticism 713). Pour ma part, je suis plutôt tenté de croire que l'analyse littéraire proprement dite commence justement là où la linguistique s'arrête (tout comme, dans le sens contraire, la linguistique commence là où s'arrête l'étude proprement littéraire). Il est vrai qu'il se peut que je me trompe. Mais dans ce cas, le 'criticism' perd de ce fait même tout raison d'être, englouti qu'il est par la linguistique. Il va de soi que la linguistique peut et doit fournir un point de départ sûr à l'étude littéraire, tout comme la physiologie articulaire, par exemple, sert de base à la phonématique. Vouloir cependant réduire l'étude littéraire à la linguistique équivaudrait à vouloir réduire la phonématique à la phonétique, voire à la physiologie ou à l'acoustique, autrement dit reconcer aux conquêtes les plus décisives de la linguistique moderne. Il est pourtant évident qu'à un certain niveau, le plus immédiat, ces deux ordres de faits coïncident: matériellement, le fait littéraire est un fait de langue. Ce qui est peut-être moins évident—

⁷ Considérée sous cet angle, il faut reconnaître que, sans viser d'une manière conséquente à la rigueur, la méthode spitzerienne s'avère autrement efficace et d'une souplesse admirablement adaptée à la complexité des faits littéraires. Même certaines formulations 'fleuries', contre lesquelles s'est acharné Bruneau, recèlent souvent, en dépit de leur caractère 'impressionniste', des principes valides: il faut simplement expliciter ces principes et les reformuler en termes objectifs pour leur conférer le statut opératoire.

mais non moins vrai—c'est que le fait littéraire est quelque chose de plus qu'un fait de langue pur et simple: un fait de langue pourvu d'une dimension supplémentaire, la dimension littéraire. Cette 'dimension littéraire' n'ayant pas encore été définie, ni même identifiée, certains esprits pourraient douter de sa réalité. Il suffira de leur rappeler que, matériellement, le fait de langue coïncide, lui aussi, avec le fait acoustique (ou avec le fait graphique), ce qui ne l'empêche pas d'être quelque chose de plus qu'un fait acoustique pur et simple: c'est un fait acoustique pourvu d'une dimension supplémentaire, la dimension linguistique.

Cette façon de concevoir les faits littéraires se rencontre au départ avec celle de Hill et Whitehall, pour lesquels 'a work of literature is therefore a language act, like other language acts, but differentiated from them by characteristics of its own' (*A report on the language-literature seminar* 1).⁸ Où j'hésiterais cependant à suivre les auteurs, c'est lorsqu'ils déduisent, 'As a further assumption, which in part necessarily follows from the first, we believe, that a work of literature can be behavioristically investigated as a language act'. Car ce qui, dans la définition proposée, différencie le fait littéraire du fait de langue, ce sont certaines 'characteristics of its own' (ma 'dimension supplémentaire'), dont on ne connaît pas la nature. Tout ce que l'on peut affirmer, c'est qu'elles ne sont pas de caractère linguistique, car autrement leur incorporation dans la définition deviendrait tautologique, contenues qu'elles seraient dans le 'language act'. Or, aussi longtemps qu'on n'aura pas surpris et défini ces caractères, rien ne nous autorise à préjuger sur leur nature et à affirmer qu'elle est telle qu'ils peuvent être 'behavioristically investigated'. En effet, l'identité matérielle du fait linguistique et du fait littéraire ne nous permet pas d'opérer cette réduction analytique. Une fois de plus, il convient d'avoir à l'esprit que le fait de langue coïncide matériellement avec le fait acoustique, ce qui n'empêche que les méthodes linguistiques diffèrent essentiellement des techniques d'investigation de l'acoustique ou de la physiologie articulaire, et que la linguistique, qui collabore avec ces disciplines, s'est acquis un statut indépendant: elle a cessé d'être une branche de l'acoustique ou de la physiologie. Il en va de même pour la science de la littérature: tout en tirant profit des données linguistiques, elle ne saurait, à mon avis, être conçue comme une simple branche de la linguistique.

Il en découle un désaccord essentiel, touchant aux besoins les plus pressants des études littéraires. En effet, je me sépare aussi nettement de Whitehall lorsqu'il affirme que 'the kind of linguistics needed by recent criticism for the solution

⁸ Dans le même esprit, voir la définition du vers par J. Lotz: 'All verse is a glossic phenomenon but all glossic phenomena are not verse; thus verse may be defined by adding to lingu[ist]ic phenomena, the special marks that make verse; to put it in terms of logics, a glossic phenomenon in general is the *genus proximum* to which, in order to define verse, you have to find a *differentia specifica*'—Notes on structural analysis in metrics, *Helicon* 4.125 (1940). Une fois de plus, le problème est de savoir si la *differentia specifica* se prête aux mêmes méthodes d'investigation que le *genus proximum* (le fait linguistique proprement dit), ce qui semble par définition exclu.

Voir aussi, tout récemment, la définition proposée par B. Bloch, *Linguistic structure and linguistic analysis, Monograph series on languages and linguistics* [Georgetown University] 4.42 (1953): 'the *STYLE* of a discourse is the message carried by the frequency-distributions and the transitional probabilities of its linguistic features, especially as they differ from those of the same features in the language as a whole.'

of its pressing problems ... is not semantics, either epistemological or communicative, but down-to-the-surface linguistics, microlinguistics and not metalinguistics' (From linguistics to literary criticism 713). Il est, bien sûr, tenant d'appliquer en matière littéraire le principe russelien 'Divide and conquer', et de faire subir aux faits de cet ordre l'opération de dissociation en ligne de l'expression et ligne du contenu qui a donné de si beaux résultats en linguistique structurale. On pourrait ainsi opérer l'analyse des deux chaînes séparément, comme le recommande la glossématique, ou essayer de réduire autant que possible le système du contenu à la chaîne de l'expression, comme le préconise Whitehall dans l'esprit de la linguistique bloomfieldienne. Le malheur est que, dans un cas comme dans l'autre, l'identification de l'analyse littéraire avec l'analyse linguistique devient fatale et inévitable. Il est parfois utile de couper le noeud gordien. Mais il faut alors ne plus avoir besoin de la corde et, dans notre cas, c'est justement celle-ci qu'il convient d'examiner. Car le fait littéraire est chose bien plus fragile que le fait linguistique brut, il se pourrait qu'il ne résistât point à ce dépeçage, dans la mesure où c'est justement dans l'intégration indissoluble de la forme et du contenu, pour employer la terminologie traditionnelle, de l'expression et du contenu (de la forme et de la substance), pour recourir à la terminologie plus rigoureuse de la linguistique moderne, que réside sa nature spécifique. Ici encore, je m'opposerai à Whitehall en affirmant que l'impasse où se trouvent actuellement prises les études littéraires est la conséquence directe de l'état précaire des études de contenu et du grand retard qu'elles ont pris par rapport aux études de l'expression. En prenant donc le contre-pied de Whitehall, je dirais que le genre de linguistique dont les études littéraires ont le besoin le plus urgent est justement la sémantique. C'est de l'établissement d'une sémantique saine mais souple, d'une étude du contenu rigoureuse mais compréhensive que dépend le plus essentiellement l'avenir des études littéraires.⁹

2. LA DOCTRINE STYLISTIQUE DE BRUNEAU

2.1. Économie de l'ouvrage. Le scepticisme exprimé au sujet des formulations théoriques de Bruneau ne nous dispense pas pour autant d'évaluer sa doctrine

* Un regain d'intérêt de date assez récente pour les recherches sémantiques nous permet d'espérer que les études littéraires pourront bientôt se mouvoir sur un terrain plus ferme en ce qui concerne les faits du contenu. Plusieurs traitements d'ensemble ont paru ces dernières années. A retenir: S. Ullmann, *The principles of semantics* (Glasgow, 1951); S. Öhmann, *Wortinhalt und Wortbild: Vergleichende und methodologische Studien zur Bedeutungslehre und Wortfeldtheorie* (Stockholm, 1951); R. Hallig et W. v. Wartburg, *Begriffssystem als Grundlage für die Lexikografie* (Berlin, 1952); Heinz Kronasser, *Handbuch der Semasiologie: Kurze Einführung in die Geschichte, Problematik und Terminologie der Bedeutungslehre* (Heidelberg, 1952).

Mais je tiens surtout à attirer l'attention sur quelques articles récents, entrepris au moins en partie dans l'esprit structural de la linguistique moderne: R. S. Wells, *The State and prospects of semantics* (mimeographed; 1950); B. v. Lindheim, *Neue Wege der Bedeutungsforschung*, *Neuphilologische Zeitschrift* 3.101-15 (1951); S. Öhmann, *Theories of the linguistic field*, *Word* 9.123-34 (1953); S. Ullmann, *Descriptive semantics and linguistic typology*, *Word* 9.225-40; et surtout l'étude révélatrice de Charles Bazell, *La sémantique structurale*, *Dialogues* 3.120-32 (1953).

selon les résultats qu'elle donne à l'usage, car il arrive très souvent que la pratique soit en avance sur la théorie. C'est pourquoi j'examinerai dans les paragraphes suivants l'économie interne de l'*Histoire* en considérant surtout les principes méthodologiques qui se trouvent à sa base et en insistant plus particulièrement sur le degré de rigueur et sur le caractère scientifique de la doctrine en cause.

La matière de l'ouvrage est répartie en trois LIVRES; ils sont consacrés, dans l'ordre, à la poésie romantique (1-78), à la prose romantique (79-199), et aux Parnassiens (201-374). Dans les deux premiers Livres, la langue des divers auteurs est traitée séparément (poésie: Hugo, Banville; Glattingy, Pommier; prose: Hugo, Gautier, d'Aurevilly, Villiers, Cladel, Vallès, Michelet); dans le troisième, Gautier mis à part (201-32), la langue des Parnassiens est traitée globalement (sont retenus, 'autour le Leconte de Lisle et Hérédia', Mme. Ackermann, Bouilhet, Bourget, Coppée, Dierx, France, de Guerne, Lahor, de Laprade, Lemaître, Lemoine, Menard, Mendés, Merat, Silvestre, Sully-Prudhomme, et Valade; Baudelaire, Verlaine, et Mallarmé seront étudiés à part).

2.2. Domaines du style: vocabulaire, grammaire, style, phrase. Aussi bien les exposés des auteurs examinés séparément (romantiques et Gautier parnassien) que le traitement d'ensemble des Parnassiens sont assis sur les mêmes bases, ce qui nous permet de saisir de manière plus concrète la façon dont Bruneau conçoit la stylistique et ses principaux secteurs. Dans la conception de l'auteur, les domaines propres de la stylistique sont au nombre de quatre: le vocabulaire, la grammaire, le style, et la phrase. Cette répartition quadripartite, qui représente l'ossature d'un traitement stylistique exhaustif, n'apparaît pas à première vue, car les articulations n'en sont pas suffisamment mises en relief. Elles sont brouillées par des imprécisions d'ordre technique (typographiquement, les titres des 4 sections sont imprimés avec les mêmes caractères que ceux de leurs subdivisions, les paragraphes; du point de vue de la mise en page, les sections apparaissent tantôt comme chapitres, tantôt comme sous-chapitres, le plus souvent comme paragraphes), aussi bien que par l'incohérence des titres de sections dont la formulation n'est pas homogène. Ainsi la section consacrée aux faits de 'grammaire' apparaît sous le titre *Hugo et la grammaire* (100) dans l'exposé réservé à Hugo poète romantique, sous le titre *Paix à la rhétorique et guerre à la syntaxe!* (145) chez Barbey d'Aurevilly, sous le titre *La grammaire* (185) chez Michelet 'essayiste', sous le titre *Gautier 'grammairien'* (217) dans l'exposé dédié à Gautier poète romantique, sous le titre *Formes et fonctions* (278) dans le traitement d'ensemble des Parnassiens, etc., ce qui n'est pas pour faciliter la compréhension du schéma de base. Pour saisir le principe de la répartition, il faudra surtout ne pas se laisser abuser par les titres des grands chapitres, qui sont parfois trompeurs. Ainsi dans le chapitre II du livre premier intitulé *Victor Hugo et la grammaire* (31-54), seules les 4 premières pages (31-5) traitent de ce que l'auteur désigne généralement comme 'grammaire', le gros du chapitre étant consacré aux faits de 'style' (35-47), aux faits de 'phrase' (47-50), et aux conclusions générales (51-4); par contre, le chapitre II du livre second consacré à Victor Hugo et intitulé *Le style* (98-109) comprend à côté des *Procédés de style* (101-5) la section *Hugo et la grammaire* (100-1); alors qu'en général la 'gram-

maire' et le 'style' sont traités séparément, comme des domaines plus ou moins indépendants.

2.3. Importance et hiérarchie des domaines. Si l'on évite cependant ces écueils en considérant, non les titres et la présentation matérielle, mais la matière traitée dans les sections, la répartition de base—vocabulaire, grammaire, phrase, style—apparaît avec netteté. En effet, on retrouve cette distribution chaque fois qu'un auteur bénéficie, conformément à l'importance de son oeuvre, d'un traitement ample, plus ou moins exhaustif. C'est le cas pour Hugo poète romantique (3-76), Hugo prosateur romantique (79-127), Gautier prosateur romantique (128-41), Barbey d'Aureville (142-62), Michelet l'historien (175-83), Michelet l'essayiste' (183-97), et de l'exposé d'ensemble de la langue des Parnassiens (201-374), dont le traitement 'stylistique' comprend ces quatre sections.

Lorsqu'un auteur de moindre importance demande un traitement plus sommaire, ceci se manifeste non seulement par la réduction de l'espace imparti à chaque secteur mais aussi par la suppression de la section consacrée à la grammaire en premier lieu, de la section consacrée à la phrase ensuite. Lorsqu'un exposé est complet, l'ordre de traitement est: (1) ou (2) vocabulaire ou grammaire, (3) style, (4) phrase. Une seule exception à ce schéma: dans le traitement d'ensemble des Parnassiens, le vocabulaire est traité en troisième lieu, après la grammaire et les procédés de style, mais avant la phrase. Ceci semble suggérer une certaine hiérarchie des divers secteurs de la stylistique échafaudée par Bruneau, où le vocabulaire et le style semblent l'emporter sur la grammaire et la phrase. Ceci paraît d'ailleurs confirmé par la statistique de l'espace imparti aux divers secteurs: les introductions et les conclusions mises à part, sur 287 pages consacrées aux traitements proprement dits, 100 reviennent au style, 89 au vocabulaire, 68 à la phrase, et 30 à la grammaire.

2.4. Vocabulaire. Il est difficile de se faire une idée très précise de ce que Bruneau entend par vocabulaire, grammaire, style, et phrase en tant qu'éléments constitutifs de son système stylistique. Grosso modo, on pourrait les assimiler à la morphologie traditionnelle axée sur le mot (grammaire), à la syntaxe axée sur la phrase (phrase), et au lexique (vocabulaire), auxquels on ajouterait le style (figures, procédés stylistiques). Puisque Bruneau ne les définit nulle part, essayons de mieux les saisir d'après les matières rangées sous ces titres. On peut très aisément se débarrasser du vocabulaire, qui ne fait pas de difficultés: on y trouve essentiellement des 'emprunts' contractés par la langue littéraire aussi bien auprès des langues étrangères qu'auprès de la langue commune ou courante, aussi bien auprès des dialectes et patois qu'auprès des argots, vocabulaires scientifiques ou de métier, terminologies techniques, etc. Seule exception notable, les innovations lexicales, que l'auteur désigne curieusement comme 'néologismes'. Sur ce terrain, les inconséquences sont insignifiantes et je me contenterai d'objecter à Bruneau d'avoir parfois introduit dans le vocabulaire les changements de sens (Hugo 'visionnaire', 25; Michelet l'essayiste', 184-5) ou les extensions de sens (Hugo prosateur, 93-94; Parnassiens, 342-3) qui, par opposition aux autres catégories de termes renfermés dans la même section, ne sont pas des

emprunts et qui auraient mieux convenu aux figures stylistiques. Il en va de même pour l'euphémisme (Parnassiens, 343), qu'il aurait mieux convenu de maintenir dans les procédés de style, comme dans le cas de Barbey (150), ou de l'accumulation (Gautier prosateur, 136-7), généralement traitée dans la phrase (Hugo prosateur, par exemple 116-88).

2.5. Grammaire, style, phrase. Des difficultés pratiquement insurmontables apparaissent cependant lorsqu'il s'agit de saisir ce que l'auteur entend, à l'intérieur de son système stylistique, par grammaire, style, et phrase. Ici les incongruences sont tellement nombreuses et d'ordres tellement divers que l'on n'arrive plus à entrevoir, ne serait-ce que très vaguement, les limites qui séparent ces domaines, ni le principe qui les intègre. Tout ce que nous pouvons dégager, c'est quelque chose comme un 'noyau' central offert par le style, qui s'interpose et sépare la grammaire de la phrase, la morphologie (axée sur le mot) de la syntaxe (axée sur la phrase). Il s'agit moins de l'ordre de traitement des secteurs (le style est toujours traité après la grammaire mais avant la phrase), que du fait que l'on ne trouve que très rarement des confusions entre la grammaire et la phrase (je veux dire qu'on ne trouve presque jamais les mêmes matières traitées tantôt dans la grammaire, tantôt dans la phrase), lorsque les confusions entre grammaire et style d'une part, entre style et phrase d'autre part, sont très nombreuses. Quelques exemples seulement: les mots composés sont traités sous grammaire à la page 100, mais sous style aux pages 157 et 167; les adverbes en *-ment* apparaissent sous grammaire 100, 137, 186, mais sous style 153, 304-5; l'adjectif substantivé est traité sous grammaire 100, mais sous style 153. Quelques exemples de confusions entre style et phrase: l'antithèse est traitée sous style aux pages 105 et 149, mais sous phrase aux pages 115, 118, 362, et 366; l'énumération, l'accumulation, et l'entassement sont traités sous style 44, mais sous phrase 116, 322, et, qui plus est, sous vocabulaire aussi (136); l'emploi d'un verbe impersonnel avec sujet est traité sous style (70), mais l'emploi d'un verbe intransitif avec objet est renvoyé à la grammaire (283). On a parfois l'impression que l'auteur renvoie au style tout ce qui n'appartient pas nettement à la grammaire ou à la phrase.

2.6. Phrase. Puisqu'il est difficile de serrer de plus près ces concepts en les confrontant entre eux, essayons d'y voir un peu plus clair en les examinant indépendamment. Lorsqu'il donne un traitement plus ou moins complet de la phrase, par exemple, Bruneau distingue essentiellement entre les procédés et les types. Cette distinction apparaît chez Hugo prosateur (types 115, procédés 115-25), chez Michelet l'essayiste (procédés 193-5, types 195-6), et chez les Parnassiens (types 352-62, procédés 362-8). Le caractère lacunaire des définitions nous empêche, une fois de plus, de préciser ce que l'auteur entend par procédé et par type. Essayons de nous en faire une idée en examinant les matières rangées sous ces rubriques. Les procédés comprennent: chez Hugo prosateur (115-25), la coupe, le détachement, l'énumération, la phrase antithétique, les liaisons de phrases, le couplet, le développement, la description, le portrait; chez Michelet l'essayiste (193-5), l'exclamation, le dialogue intérieur, la parenthèse, l'inversion, l'anticipation, l'ellipse, l'anacoluthie, l'interruption; enfin, chez les Parnassiens (362-8), l'inversion, la répétition, le pronom relatif séparé

de son antécédent, l'antithèse, l'alliance des mots. Comme on voit, sur 22 procédés, les seuls qui reviennent sont l'inversion (193 et 362) et l'antithèse (118 et 362), et encore s'agit-il, pour cette dernière, d'opposition des membres de la phrase dans un cas (118), d'opposition des mots dans la pensée dans l'autre (362).

Si l'on confronte maintenant les procédés avec les types, on s'aperçoit que le développement, par exemple, qui était un procédé chez Hugo (115), devient type chez Michelet (195-6); et que l'anacoluthie, traitée comme procédé chez Michelet (194), est considérée comme type chez les Parnassiens (357). Bien que l'on puisse justifier, dans un certain nombre de cas, ces inconséquences en invoquant des différences d'un ordre quelconque entre phénomènes à première vue analogues, il est pourtant difficile de faire la part et de donner un classement tant soit peu rigoureux en raison du caractère defectueux et incohérent des définitions qui, d'une manière générale, sont très représentatives pour le peu de rigueur de la doctrine stylistique que nous propose cet ouvrage.

2.7. Définitions. Il est pourtant vrai que l'auteur fait un effort notable pour éviter les pièges tendus par l'inconséquence de la terminologie traditionnelle: très souvent, un chapitre ou un paragraphe débute par la définition du terme ou de la formule qui le désigne ou le caractérise. Malheureusement, l'effort n'est pas suffisamment poussé et, comme nous l'avons déjà vu, ces définitions indispensables font souvent défaut. Par contre, certains termes désignant des figures, des procédés, des types, etc., sont définis à plusieurs reprises mais de façon, sinon contradictoire, du moins divergente. Un exemple: l'énumération est définie comme 'réunissant des noms inattendus' (44), comme 'la simple constatation d'une série de réalités' (116), et comme 'formée d'une série de membres, généralement très courts, séparés par des virgules, qui marquent des silences' (322)! En confrontant les définitions des procédés apparentés (ils sont toujours traités ensemble) qui sont l'énumération, l'accumulation, et l'entassement, nous apprenons à la même page que les énumérations 'réunissent des mots inattendus', l'accumulation n'est pas explicitement définie, mais l'entassement est défini, lui, comme un certain genre d'accumulation, comme 'une accumulation désordonnée' (44).

Le grand inconvénient des définitions sur lesquelles s'appuie le système stylistique de Bruneau est qu'elles se rapportent à des séries ou ordres de faits ou de relations hétérogènes, sans que l'on puisse déceler une quelconque hiérarchie des divers niveaux. On définit les concepts (figures, procédés, types, etc.) tantôt par des critères de contenu, tantôt par des critères d'expression; tantôt par des critères de forme, tantôt par des critères de substance; tantôt la définition est formulée en termes causals, tantôt en termes téléologiques. Tantôt le procédé de style est défini par l'intention de produire un certain effet (sur l'interlocuteur, lecteur), tantôt par l'effet supposé produit, etc. Devant une telle variété non-hiérarchisée de niveaux, aucun principe de réduction ne semble donner des résultats satisfaisants: certains principes réducteurs expliquent un nombre limité de faits; mais un nombre au moins égal reste en dehors, abandonné aux 'marginalités' ou aux 'exceptions'.

2.8. Fondements de la doctrine (expression/contenu, forme/substance). Ainsi que nous l'avons déjà vu, il semble que le seul moyen d'établir un rapport

plus ou moins constant entre les divers secteurs de la doctrine stylistique qu'on nous propose est de procéder empiriquement et d'assigner au style (sens restreint) une place intermédiaire entre la grammaire et la phrase, ce qui est confirmé jusqu'à un certain point par l'ordre de traitement. Si l'on veut cependant serrer les choses de plus près et voir ce qu'il y a derrière ce classement purement mécanique, on s'aperçoit que, dans bien des cas, les faits et phénomènes que Bruneau range sous style sont ou bien des faits de contenu, ou bien des faits qui se déroulent sur les deux plans mais que l'on définit plus aisément par des critères de contenu. Par contre, les faits de grammaire et les faits de phrase sont presque toujours de faits d'expression, ou définis par des critères d'expression.¹⁰

Si ceci est bien le principe latent qui règle le fonctionnement de la doctrine illustrée par l'*Histoire*—et il semble fonctionner dans un assez grand nombre de cas—on s'explique mieux les nombreuses confusions entre faits de grammaire (morphologie) et faits de style, entre faits de style et faits de phrase (syntaxe) signalées plus haut. Le même fait pourra donc être renvoyé au style lorsqu'on le classera d'après des critères de contenu, mais il reviendra, suivant le cas, à la grammaire ou à la phrase si on préfère l'identifier, le définir ou le classer suivant des critères d'expression.

2.9. Principe de la doctrine (mot/phrase). Quant à la distinction, dont le caractère ressortit plutôt à l'expression, entre grammaire et phrase, elle semble correspondre à la distinction traditionnelle morphologie/syntaxe, axée sur l'opposition mot/phrase. Tels qu'ils sont conçus par Bruneau, les faits de grammaire stylistique portent jusqu'à un certain point sur le mot, lorsque les faits de phrase stylistique portent sur la phrase grammaticale dans son ensemble. Mais ceci ne fonctionne que très imparfaitement. Plutôt que sur une dichotomie mot/phrase, la stylistique de Bruneau paraît échafaudée sur le mot comme notion centrale, la phrase n'étant conçue que de manière relative et négative par rapport au mot, comme quelque chose qui se trouve en dehors de celui-ci: dans le système de l'auteur, appartient à la phrase tout phénomène stylistique qui débordé les frontières du mot.

En axant sa doctrine sur le mot, Bruneau se fraye du même coup chemin jusqu'à la terminologie grammaticale traditionnelle dont il s'empare: substantif, adjectif, verbe, etc. Ceci présente l'avantage de la commodité et aussi d'une compréhension générale malheureusement peu concluante. Car, dans un système stylistique, les termes de la grammaire traditionnelle ne disent pas grand'chose, assortis qu'il sont à un tout autre système de faits et de relations. Stylistiquement parlant, un mot comme *adjectif* peut, en effet, désigner un fait de vocabulaire (si l'adjectif est un emprunt contracté par la langue littéraire) aussi bien qu'un fait de grammaire (s'il se trouve pourvu de l'article et employé comme substantif), un fait de style (s'il est employé métaphoriquement) aussi bien qu'un fait de phrase (s'il fait partie d'une série de mots qui construisent la phrase par entassement). C'est pourquoi, malgré les réserves explicites formulées par Bruneau (*La stylistique* 9-10), l'élaboration d'un système terminologique ap-

¹⁰ Les termes *expression* et *contenu*, *forme* et *substance*, sont employés ici avec l'acception qui leur a été attribuée par la glossématique hjelmslevienne.

proprié s'impose plus que jamais si l'on veut tirer la stylistique de son impasse actuelle.

2.10. Problème de la valeur. Une dernière question méthodologique reste ouverte: le problème de la valeur dans le système stylistique de Bruneau. Après avoir identifié, sélectionné et classé les matériaux à l'aide des concepts discutés (concepts stylistiques: procédés et figures; concepts grammaticaux: nom, verbe, adjectif, etc.; concepts syntaxiques: procédés et types de phrase), comment s'en sert-on pour évaluer les faites littéraires? Il est évident que, dans la doctrine illustrée par *l'Histoire*, aucun rapport ne semble exister entre les jugements d'existence établis à l'aide de concepts plus ou moins linguistiques et les jugements de valeur qui s'en suivent. Sur ce point, la doctrine me paraît hybride, car elle repose sur deux systèmes de références successifs et incompatibles: les concepts stylistiques, voire linguistiques, utilisés par Bruneau ont de toute évidence une valeur 'en soi', car on n'arrive pas à saisir le système de références en vertu duquel 'la *personification* est jolie' (140) alors que 'La répétition est un procédé banal' (364). En effet, la plupart des jugements de valeur portés dans ce livre ne se rapportent pas à l'usage que font les auteurs des procédés, mais aux procédés considérés en eux mêmes: la reprise du sujet est un 'procédé commode et peu élégant' (159); il en est de même de l'inversion ou du déplacement de mots qui est 'un procédé facile' (168); par contre, le procédé du renouvellement des mots 'peut être considéré des plus impressifs' (46); etc. Cette conception d'une valeur 'en soi' ne semble pas se limiter aux concepts stylistiques, elle entraîne parfois même les concepts grammaticaux: 'Les Prépositions, qui exprimant des rapports ont par la même quelque chose d'anti-poétique' (288); ou encore: 'Les Conjonctions de subordination, qui marquent elles aussi des rapports, ne seront pas plus poétiques que les prépositions' (290)! Si ceci est vrai, si l'expression d'un rapport est par définition anti-poétique, voilà qui est bien fâcheux pour la stylistique de Bruneau, qui tombe en morceaux: car, dans ce cas, toute figure stylistique et tout procédé grammatical est 'par là même' anti-poétique, puisque c'est le propre de toute figure stylistique et de tout concept grammatical d'établir et d'exprimer un rapport.

Je ne crois pas, pour ma part, que l'on puisse assigner une valeur en soi, constante et invariable, aux figures stylistiques, et encore moins aux concepts grammaticaux. La valeur d'une figure, par exemple, dépend essentiellement de l'usage qu'on en fait: elle peut varier suivant un certain nombre de conditions aussi bien immanentes que transcendantes et dont l'élucidation constitue l'une des tâches essentielles de la stylistique, sinon une certaine façon de concevoir la science du style. Ceci ressort d'ailleurs très nettement de certaines formulations d'aspect paradoxal avancées par Bruneau lui-même—'L'emploi que fait Barbey de ce procédé grossier [l'attelage] est remarquablement délicat' (150)—qui nous indiquent que c'est dans le sens de l'usage, c'est à dire des faits de relation, qu'il faut chercher l'essence de la valeur littéraire. Sur ce terrain le livre de Bruneau est manqué, la doctrine qui se trouve à sa base n'effleure même pas le problème: ce qui lui manque avant tout, c'est une théorie des valeurs.

3. ANALYSE STYLISTIQUE DE L'ÉPOQUE RÉALISTE

3.1. Conception de l'histoire de la langue littéraire (époque/période/école).

Du point de vue purement littéraire, que j'adopterai dans les paragraphes suivants, Bruneau, qui suit ici une longue tradition, conçoit l'histoire de la langue littéraire par époques, voire par périodes et par écoles: le volume précédent avait été consacré à l'époque romantique (1815-52), le présent volume est dédié à l'époque réaliste (1852-85) et subdivisé en deux périodes, fin du romantisme et Parnasse.

Sur quelles bases l'auteur asseoit-il ses concepts historiques, qui sont principalement les époques? Quels sont les critères qui lui permettent de poser une époque romantique et une époque réaliste, distinctes et successives? Précisons—et ceci constitue l'une des plus graves objections que l'on puisse faire à la conception historique de la langue littéraire illustrée par l'*Histoire*—que les époques de Bruneau ne sont pas établies sur des bases linguistiques: elles sont empruntées telles quelles, non pas à l'histoire de la langue (fut-elle littéraire) mais tout simplement à l'histoire de la littérature. Bons ou mauvais, les instruments d'investigation stylistico-linguistiques utilisés par l'auteur tout le long de l'ouvrage ne jouent aucun rôle dans l'établissement des étapes historiques du français littéraire. L'auteur ne se demande jamais s'il a vraiment existé un romantisme linguistique (ou un réalisme linguistique), c'est à dire une période du français littéraire linguistiquement cohérente, caractérisée par un certain nombre de traits de grammaire, de phrase, de style, ou de vocabulaire que l'on puisse désigner, serait-ce arbitrairement, comme romantique. En réalité, l'existence d'une époque romantique dans l'histoire de la langue française littéraire est considérée par Bruneau comme historiquement donnée: et elle l'est effectivement, non point cependant par l'histoire de la langue mais par l'histoire de la littérature. Bien que l'auteur prétende faire l'histoire de la langue, il opère constamment avec les concepts de l'histoire littéraire à laquelle il a emprunté ses schémas, dont il examine le contenu à l'aide des méthodes linguistiques. Mais les critères linguistiques n'interviennent qu'après coup, leur rôle étant limité au classement de certains faits à l'intérieur de frontières tracées d'avance et sur lesquelles ils n'ont point de prise.

En conclusion, la conception de l'histoire de la langue littéraire illustrée dans ce livre est elle aussi hybride: elle consiste à examiner avec des méthodes plus ou moins linguistiques le contenu d'un objet constitué à l'aide de concepts d'un autre ordre, d'histoire littéraire en l'espèce.

3.2. Conception de l'époque. D'une manière générale, une époque est pour Bruneau—tout comme pour l'historien de la littérature—la période approximative où une 'école' s'est imposée et a jusqu'à un certain point triomphé (on sait pourtant à quel point la place du linguistique est réduite dans les triomphes de cet ordre). Car il ne faut pas être mauvais juge et prendre au pied de la lettre la déclaration (p. v) liminaire de Bruneau, suivant laquelle 'Le terme d'Epoque Réaliste, que j'ai adopté pour la période qui commence vers 1852 pour se terminer vers 1886, est un terme commode; toutefois, le mot "réalisme", qui sert à la

caractériser, est aujourd'hui à peu près dépourvu de sens. "Époque Réaliste" comme "Époque Romantique", "Époque Symboliste", est pour moi un nom propre tel que "Siècle d'Auguste" ...; il désigne une période arbitrairement délimitée. Commode, ce terme il l'est certainement, et même un peu trop. Quant à être dépourvu de sens, voire! Car un terme 'à peu près dépourvu de sens' comme romantisme (ou romantique) repose en réalité sur un sens peut-être imprécis mais incontestable, celui que Bruneau lui attribue dans le corps de l'ouvrage chaque fois qu'il parle du romantisme de tel auteur ou du caractère romantique de telle oeuvre. Dans tous ces cas, le mot romantique ne saurait en aucune façon être défini en termes de temps, comme le prétend Bruneau, qui veut assimiler 'Époque romantique' à 'Siècle d'Auguste' ou 'Siècle de Louis XIV'. Cette réduction est tout à fait arbitraire, étant donné que ces dernières notions s'appuient sur des bases entièrement différentes, exclusivement à partir du temps et sans référence à leur contenu différentiel, tandis que dans 'Époque romantique,' où la chronologie n'est pas pertinente (au moins par rapport aux écoles contemporaines), le contenu spécifique joue un rôle primordial. C'est pourquoi il aurait mieux valu renoncer à la solution chronologique, qui est une solution de facilité, et en évitant tout 'à peu près,' essayer d'axer les concepts historiques sur des bases linguistiques. Ceci aurait évité à Bruneau de tomber dans l'arbitraire, comme lorsqu'il écrit: 'L'époque réaliste est une époque de confusion' (v). Mais un peu plus loin: 'L'époque que j'ai appelée romantique est, elle aussi, une époque de confusion' (vii). La différence entre le romantisme et le réalisme serait-elle, en dernier lieu, une différence de degré de confusion? Toute époque ne se prête pas à la Periodisierung sur des bases chronologiques: si cette division convient plus ou moins aux époques de cohérence, où le caractère de l'époque est évident, elle ne convient surtout pas aux époques 'de confusion,' qui réclament que soit dégagé le système d'affinités insaisissable 'à l'oeil nu.'

3.3. L'enchaînement historique. Quels que soient les critères utilisés dans cette opération, les 'époques' ou 'périodes' une fois délimitées on peut donner une description plus ou moins fidèle des stades successifs de la langue littéraire qui, soumis à une interprétation diachronique, devraient nous livrer le sens et le caractère propre du processus historique: la description des mutations n'est que la condition préliminaire de leur interprétation. Sur ce plan, l'échec de la doctrine illustrée par l'*Histoire* est flagrant: non seulement dans la description, où une époque est atomistiquement morcelée en un certain nombre d'auteurs juxtaposés mécaniquement, mais surtout dans la succession, où le nexus historicus fait entièrement défaut. Les auteurs se suivent à la queue leu leu et l'enchaînement historique se fait par des petites phrases de cet ordre: 'Banville est souvent classé parmi les Parnassiens. En fait, il est un romantique' (55); 'Élève de Banville, lui même disciple de Hugo, Glattigny est le reflet d'une lueur' (65); 'Il [Cladel] fut l'élève de Victor Hugo, auquel il dédia *Ompdrailles*' (170). L'établissement du nexus est conventionnel, l'interprétation des mutations superficielle, la perspective de la langue littéraire comme un tout en devenir fait entièrement défaut.

La méthode de cet autre historien du français littéraire, Karl Vossler, d'une quarantaine d'années le prédécesseur de Bruneau, est autrement convaincante.

Bien qu'elle soit elle aussi souvent discutable et largement tributaire de l'histoire littéraire, la conception historique illustrée par *Frankreichs Kultur und Sprache*¹¹ est beaucoup plus concluante lorsqu'il s'agit de surprendre le français littéraire comme un tout en devenir. Vossler envisage l'histoire d'une langue littéraire de manière analogue, axée sur une succession de 'moments,' culminants et homogènes. Mais les 'moments' ne sont, chez lui, que des points de repère: l'histoire du français littéraire est conçue comme une suite de passages d'un 'moment littéraire' à l'autre, d'une manière qui satisfait beaucoup mieux les exigences des disciplines historiques. Même lorsque l'interprétation est peu convaincante ou franchement manquée, elle est toujours là, tendant constamment à surprendre le principe de l'enchaînement et à justifier le titre d'histoire.

3.4. Le concept de romantisme. Les deux premiers Livres sont consacrés à la fin du romantisme en France, à *La poésie 'romantique'* (1-77) et à *La prose 'romantique'* (79-200) respectivement. Bien que Bruneau revienne à plusieurs reprises sur ce terme après lui avoir consacré un chapitre entier dans le tome 12, il n'est pas facile de préciser ce qu'il entend par romantisme (et par romantique): 'Ce qui caractérise, au point de vue de la langue et du style, les écrivains que je classe sous l'étiquette romantique, c'est d'abord l'esprit révolutionnaire' (v). Ceci paraît axer le romantisme sur le politique, voire sur une politique de langage, ce qui est solidement confirmé par la Conclusion Générale: *Le mouvement romantique* (198-200), où il est dit entre autres: 'Hugo n'a pas voulu considérer le Romantisme comme autre chose qu'une révolution linguistique'; 'L'esprit Romantique vise, en général, à la libération de l'artiste'; 'Le "romantisme" est la revanche de l'individu tenu en lisière par la société'; 'Ce qui pouvait survivre alors des valeurs littéraires de l'ancien "Esprit Romantique"—j'entends d'esprit révolutionnaire ...'. Tout ceci me paraît gravement insuffisant et beaucoup trop vague pour ne pas créer des confusions. La nature politique de la 'libération' de l'esprit révolutionnaire romantique paraît apparemment la conception de Bruneau à celle de Benedetto Croce qui, d'une manière différente, reliait indissolublement romantisme et mouvement de libération politique; cf. Croce, *Storia di Europa nel secolo decimonono* 44-64, surtout 57 (Bari, 1932). Cette perspective n'est pourtant pas exploitée par Bruneau; elle conviendrait d'ailleurs mal à des écrivains comme Barbey par exemple, ou Villiers. Il est vrai qu'on peut trouver chez eux aussi des traits 'révolutionnaires'—la haine du bourgeois, l'excentricité de Barbey—mais que Baudelaire partageait aussi, ce qui trahit l'insuffisance du critère. Il reste à savoir avant tout si les traits de cet ordre peuvent tenir avec l'esprit foncièrement bourgeois de Hugo, chantre du peuple et socialiste dans ses vieux jours. On peut également se demander si, malgré l'influence de Hugo, la langue de Villiers se prête à un rapprochement avec celle des *Misérables*. La langue passe encore, mais il s'agit ici de langue littéraire, de style, et sur ce terrain l'affinité est très discutable, même avec la prose de Gautier. Il y a affinité plutôt avec Aloysius Bertrand, qui professait—en 1830!—une admiration idolâtre pour Hugo, dont en fait il ne s'approchait que très peu.

¹¹ Version française par A. Juilland: *Langue et culture de la France: Histoire du français littéraire des origines à nos jours* (Paris, 1953).

3.5. Incorporation des écrivains au romantisme. S'appuyant sur une conception aussi lâche et imprécise du romantisme, Bruneau est constamment tenté d'incorporer dans ce mouvement—et comment s'en défendrait-il, armé d'une définition aussi flexible?—un nombre toujours plus grand d'auteurs et même de 'mouvements' et d'écoles'. Les écrivains d'abord: presque toutes les annexions au romantisme des auteurs traités aux chapitres 4 et 5 du Livre second (Banville aussi, pour la poésie), me semblent discutables.

A propos de Banville: 'Il est souvent classé parmi les Parnassiens. En fait, il est un romantique' (55). Bruneau prend trop à la lettre les déclarations de Banville. Les titres mêmes des premiers recueils font pressentir une sorte de Parnasse: *Les Cariatides*, *Stalactites*—Banville a beaucoup imité 'ces imitations (non seulement de Hugo, mais de Musset),' comme dit Jean Pommier, *Dans les chemins de Baudelaire* 197 (Paris, 1945). Mais il a imité Baudelaire aussi: 'Si la ré-édition de 1864 des *Cariatides* n'a rien qui vaille d'être signalé, celles qui eurent lieu après la mort de Baudelaire présentent un phénomène assez curieux. Non seulement Banville s'y inspire ... de Baudelaire, mais encore ces imitations ont parfois lieu dans des endroits qu'on a déjà dû rapprocher l'un de l'autre. Et il arrive qu'en se ressemblant si bien, par son nouveau texte, à Baudelaire, Banville avertisse que l'ancien déjà y ressemblait un peu, là même où on ne s'en était pas aperçu' (Pommier 203). Si la dette de Banville envers Baudelaire pour les *Cariatides* de 1842 est sujette à caution, elle est indiscutable à partir de cette date, où ils s'étaient liés d'amitié. Or, il n'y a pas échange de quelque importance sans affinité.

Barbey d'Aurevilly 'est un romantique de l'école de Hugo. Il a de Hugo l'orgueil' (1952). Que Barbey soit un romantique, cela peut se défendre: un romantique d'une espèce un peu particulière. Mais le rattacher précisément à Hugo, c'est courir au paradoxe. Comme Bruneau le remarque lui-même, 'alors que Hugo possède un certain bon sens bourgeois qui, dans ses relations avec la grammaire, la poétique et la rhétorique, le maintient dans une juste mesure' (142), Barbey est tout ce que l'on peut imaginer de plus étranger au bourgeois: son allure, sa langue, ses manuscrits mêmes nous indiquent dans quel sens il se dirige et où il faut le placer: parmi les décadents.

Villiers est classé parmi les prosateurs 'romantiques'. Mais dès le début, nous sommes alertés par une liste des admirateurs de Villiers qui comprend Henri de Regnier et Mallarmé dans le texte, Banville, Baudelaire, et Wagner dans une note (163 n. 2). J'ajoute de mon côté Stefan George: 'VILLIERS sich hoch genug für einen thron | VERLAINE in fall und busse fromm und kindlich | Und für sein denkbild blutend: MALLARMÉ'.¹² Avec Stefan George, nous voilà bien loin du romantisme de Hugo!

Jules Vallès est revendiqué lui aussi pour le romantisme: 'C'est aussi un romantique que Jules Vallès' (173). Mais une demi-page plus loin, son étude est interrompue: 'Les romans de Vallès, parus en feuilleton, sont écrits dans la langue du journal plutôt que dans celle du feuilleton. Je l'étudierai dans un autre chapitre' (174)—c'est à dire au volume suivant. Procédé peu édifiant. Déplorons

¹² Franken (groupe: Zeitgedichte; cycle: Der siebente Ring), *Blätter für die Kunst*, 7. Folge; *Gesamt-Ausgabe* 6/7.19 (Berlin, 1905).

en passant le penchant trop fréquent de Bruneau à morceler l'étude d'un auteur en la distribuant à des chapitres parfois très éloignés.

3.6. Critique de cette opération. Tous ces écrivains en litige présentent sans doute des traits romantiques, ce qui est tout à fait normal chez des artistes ayant grandi à l'ombre du romantisme. D'autre part, chez les écrivains 'mineurs,' c'est une tendance universellement reconnue que celle d'un certain retard. Mais ce qui compte, c'est la façon particulière dont ce retard s'est manifesté et encore plus la façon dont il a été dépassé et contredit par la suite. Car un écrivain uniquement attardé est négligeable et certains d'entre ceux que Bruneau revendique pour le romantisme conduisent à vrai dire vers les décadents; l'auteur réserve ce qualificatif pour la période d'après 1880, donc pour le prochain volume, cf. 200. Même chez les décadents proprement dits, on retrouve de nombreux motifs romantiques, comme Mario Praz l'a bien montré.¹³ Mais ce ne sont là que des résidus romantiques, le système poétique n'est plus du tout le même, il s'apparente plutôt à celui des Petits Romantiques.

Ces rattachements un peu abusifs au romantisme, dus essentiellement à l'imprécision du concept 'romantique' avec lequel opère Bruneau axé sur un vague esprit 'révolutionnaire',¹⁴ me semblent également résulter d'un défaut de perspective, qui consiste à identifier par principe—d'un geste apparemment arbitraire mais en fait commandé par des habitudes d'enseignement—la langue de Hugo avec la langue du romantisme français. Ceci fait, chaque fois qu'en étudiant la langue d'un écrivain Bruneau rencontre des traits hugoliens, il se trouve dans l'obligation de rattacher l'écrivain en question au romantisme. On peut bien, par la suite, relever les défauts criants de Hugo, aussi longtemps que l'identité langue de Hugo = langue du romantisme français reste par définition posée le mécanisme continue.

3.7. Incorporation des 'écoles' au romantisme. Venons en plutôt à la tendance parallèle qui consiste à rattacher à un romantisme trop vaguement conçu certains mouvements littéraires ultérieurs: 'Le Symbolisme, le Surréalisme ne sont-ils pas les enfants du Romantisme? (des enfants terribles!). Les Symbolistes et les Surréalistes n'ont-ils pas renouvelé, en les poussant jusqu'à l'absurde, les outrances linguistiques et littéraires romantiques?' se demande Bruneau, qui s'étonne aussi de voir que, contrairement à ce qui s'est passé avec le mot *romantique*, 'ni le mot *symboliste*, ni le mot *surréaliste* ne sont devenus des termes méprisants' (199).

Il est douteux que le symbolisme et le surréalisme soient les enfants du romantisme, en tout cas pas de la manière dont Bruneau paraît l'entendre. Car dans ce cas, le romantisme est lui aussi l'enfant du néoclassicisme et du rococo, mais comme il les a malmenés! La 'filiation' historique n'est pas une chose tellement linéaire: il vaut mieux considérer l'histoire, avec Hegel, comme une dialectique

¹³ *La carne, la morte e il diavolo nella letteratura romantica*³ (Firenze, 1948); version anglaise par Angus Davidson: *The romantic agony*² 502 (London and New York, 1951).

¹⁴ Comparer: 'Most of these [general] "laws" [in literature] turn out to be only such psychological uniformities as action and reaction, or convention and revolt, which, even if they were beyond doubt, could not tell us anything really significant about these processes of literature' (Wellek and Warren, *Theory of literature* 6).

d'idées incorporées. Dans cette perspective, si le symbolisme et le surréalisme sont les enfants du romantisme, il en sont du même coup l'antithèse.

Quant à la dépréciation subie par le romantisme, elle dépend, une fois de plus, de ce que l'on entend par ce terme et des valeurs propres qu'on lui attribue. Car si d'après l'échelle de valeurs de Bruneau le romantisme s'est déprécié, selon d'autres axiologies, correspondant peut-être davantage à la situation sur le marché international des idées, il connaît une brillante révalorisation. Le surréalisme par exemple, mouvement poétiquement maigre mais intellectuellement intéressant, a beaucoup contribué à la révalorisation du romantisme qui est ces derniers temps en pleine hausse. Pour confronter un peu plus concrètement les deux échelles, passons en revue les romantiques révalorisés: un Petit Romantique français totalement ignoré auparavant, Xavier Forneret (*l'Histoire* de Bruneau continue à l'ignorer); Gérard de Nerval (également ignoré par Bruneau), qui occupe une place tout à fait à part dans le mouvement romantique de France où il fait presque figure d'un étranger; von Armin, Grabbe, quelques autres romantiques allemands; et celui qui, déjà pour Sainte Beuve en 1843, était à l'origine de tout un courant de forces dans la littérature de l'époque et de plus tard, le Marquis de Sade. Il suffit de parcourir cette courte liste pour se rendre compte que le système des valeurs est tout à fait différent.

La perspective fallacieuse qui consiste à confondre les auteurs et les mouvements issus (au sens propre) du romantisme sous la vague notion de 'libération de l'artiste' transparait encore plus nettement dans des sentences de cet ordre: 'Il est malaisé, alors que nous vivons en pleine époque romantique, et même ultra-romantique' (232). Il ne fait pas de doute que romantique est pris ici dans un tout autre sens que dans 'école romantique', autrement nous irions tout droit à la négation de l'histoire, de la présente *Histoire*. N'empêche que ce sens est lâche, trop lâche. Bruneau s'en est d'ailleurs aperçu, qui essaie d'y remédier en reprenant la caractérisation plus serrée du romantisme donnée par Valéry (199 n. 3) et qui a surtout le mérite de faire voir à quel point, en France, on fait du romantisme un pendant du classicisme. Pour la discussion du sens qu'il convient d'attribuer au mot *romantique*, je préfère renvoyer à l'étude de Mario Praz citée plus haut, ou à O. Lovejoy, *On the discrimination of romanticisms*, *PMLA* 39.229-53 (1924).

3.8. Victor Hugo. Les deux premiers Livres consacrés à la fin du romantisme—et ceci vaut aussi pour une bonne part pour le troisième consacré aux Parnassiens—sont axés sur Hugo, dont la 'situation' dans les lettres actuelles est assez précaire: le discrédit du poète torrentiel est actuellement un fait consommé et, qu'on l'approuve ou non, les faits sont là. Dans ces conditions, poser la valeur de Hugo c'est faire geste révolutionnaire: il ne suffit plus de l'affirmer, il faut l'imposer ou, plus exactement, la réimposer. Pour ma part, il me semble que sur la base de nos critères actuels la cause de Hugo a été entendue et qu'il est inutile de rouvrir son dossier poétique si ce n'est que pour le réexaminer avec les instruments en circulation. Pour aborder cette tâche avec quelques chances de succès, il faudrait ni plus ni moins qu'une nouvelle échelle des valeurs esthétiques, autrement dit une nouvelle méthode d'investigation des faits littéraires. Une étude fouillée, même une monographie très ample, ne sauraient y remédier. Il

ne faut pas non plus trop se fier au temps qui, le 'moment difficile' une fois passé, pourrait faire remonter son crédit.

L'école française a probablement tort d'utiliser Hugo, avec une foi tenace, comme modèle de poésie. Les désaccords avec les juges se rapportant à d'autres codes esthétiques sont parfois flagrants. Ceci ne tend nullement à mettre en cause la place de tout premier ordre qu'occupe Hugo dans l'histoire de la langue littéraire: tout en étant d'accord avec le verdict porté par le temps—à savoir que la valeur poétique absolue de Hugo est très réduite—il reste que son influence dans le développement du français littéraire a été de grande envergure. Surtout en poésie il lui revient la part principale dans la transformation qui s'opère entre Lamartine, où les tours 'classiques attardés' sont encore fréquents, et Leconte de Lisle, avec sa poésie barbare et informée par l'histoire.

3.9. Economie générale de l'ouvrage. Dans les deux premiers Livres, consacrés à la fin du romantisme, les auteurs sont traités séparément; dans le troisième, réservé au Parnasse, ils sont traités globalement, en tant qu'école. 'Au bouillonnement romantique succède une réaction de caractère classique. Il serait imprudent de fixer des dates précises à des "mouvements" de ce genre. De fait, après la Révolution Romantique, il ne peut plus exister, à proprement parler, d'École littéraire (le mot d'*Ecole* impliquant non seulement un Maître, une Doctrine et des Disciples, mais l'adhésion d'une majorité d'écrivains au cours de plusieurs générations). Des artistes jaloux de leur individualité ne peuvent guère constituer que des groupes peu nombreux et éphémères (des chapelles)' (200). Si ceci est vrai—et il semble bien l'être—on comprend mal l'économie générale de l'ouvrage: je ne vois pas pourquoi Bruneau traite globalement, en tant qu'École, des Parnassiens 'jaloux de leur individualité, qui ne pouvaient constituer 'à proprement parler d'École littéraire' mais uniquement des 'chapelles', lorsqu'il aborde individuellement des romantiques qui avaient, eux, une école, c'est à dire 'un Maître, une Doctrine et des Disciples'.

Pour ce qui est de la répartition de l'espace entre les divers écrivains, on reproche généralement aux grands ouvrages d'histoire littéraire de traiter injustement les écrivains de première grandeur, en les alignant sur les écrivains de second ordre sans refléter par le nombre de pages imparti à chacun les proportions littéraires réelles, ni les rapports de valeur esthétique. Pour nous en tenir au seul tome 13.1, voici, en pourcentages, l'espace réservé aux écrivains les plus importants: les Parnassiens 39, Hugo 28, Gautier 13, Michelet 6, Barbey 5.5, Banville 2.5, Villiers 2, Glattigny 2. Il faut avouer que l'image qu'on nous offre est quelque peu en désaccord avec ce que nous étions en droit d'attendre. Manquent tout à fait Gérard de Nerval, Xavier Forneret, Mérimée, Tocqueville entre autres. On réserve peut-être Tocqueville pour le tome suivant, pour lequel on nous promet explicitement Sainte-Beuve historien de la littérature. Mais il est déplorable de voir Stendhal expédié en 4 pages—et encore ne sont-elles consacrées qu'à sa seule brochure *Racine et Shakespeare II*—ou Balzac disposant de 27 pages face au 40 de Michelet, aux 60 de Gautier et aux 140 de Victor Hugo. Sans doute, il y a ici défaut de méthode: puisque rien ne nous autorise à suspecter Bruneau d'avoir négligé des écrivains de la taille d'un Stendhal ou d'un Balzac, la seule conclusion raisonnable que l'on puisse en tirer c'est que la

méthode stylistique que Bruneau nous propose est esthétiquement inadéquate. Tout comme il y a défaut de méthode dans le fait que l'époque réaliste éclate en deux parties, la première consacrée aux romantiques et axée sur Hugo, la seconde consacrée au Parnasse mais sans atteindre Flaubert.

3.10. Remarques de détail. (75) 'Pommier crée des mots composés: homme-machine'. Ce mot composé ne peut pas être la création de Pommier puisque l'ouvrage bien connu de La Mettrie, *L'homme machine*, qui fit scandale en son temps, est de 1748. (147) Je ne vois pas pourquoi le 'néologisme' *explosionné*, de Barbey, serait 'un de ces mots aventuriers qui ne peuvent avoir qu'une vie éphémère'. La 'vie éphémère' de *explosionné*, qui n'était pas condamné d'avance, est due au hasard (c'est à dire à des facteurs inconnus mais non nécessaires). Ceci est prouvé par d'autres mots 'aventuriers' du même type, comme *contorsionné*, *convulsionné*, *contusionné*, qui ont survécu. (185) 'Il faut se faire à cet air frais lentement, ne "pas vouloir expressément l'aspirer".' Dans ce passage de *La mer* de Michelet, *expressément* ne me semble pas employé improprement, comme le suggère Bruneau. Il s'agit probablement du procédé stylistique que je désignerais comme 'sémantisme analytique': dans cet *expressément*, il y a sans doute *pression* (de l'air). (267) Dans le vers *Fais sculpter sur ton arc ... Imperator Illustre, ... des tronçons d'armures et de nef, Et la flotte captive et le rostre et l'alphustre* (Hérédia, *Trophées* 74), *la flotte captive* est interprété comme un tour de phrase antique du type *Sicilia amissa*, lorsqu'il s'agit d'un simple complément direct: *Fais sculpter sur ton arc ... la flotte captive*. En traduction latine, on aurait ici un accusatif et non pas un ablatif absolu. (282) Dans le vers *Sa soeur avait dix ans et lisait, la savante, Les contes ...* (Merat, *Chimères* 152), *la savante* est donné comme exemple d'un emploi curieux de l'article défini devant un adjectif. Or, *savante* n'est pas ici adjectif mais substantif, comme le prouve justement la présence de l'article défini, qui n'a donc rien de curieux. (283) Dans le vers *Réside Bhagavat dont la face illumine* (Leconte de Lisle, *Poèmes antiques* 17), *illumine* nous est donné comme un exemple du procédé 'qui consiste à donner aux verbes transitifs une valeur intransitive en les employant sans complément' car 'il est plus expressif de ne pas limiter par un objet le sens des verbes'. En réalité, l'emploi sans objet du verbe *illuminer* est calqué ici sur l'intransitif *irradier* 'dont la face irradie'. (332) Au sujet du vers d'Hérédia *Malheur au criocère imprudent qui s'y glisse!*, Bruneau pense qu'"On imagine assez facilement ce que peut être cet insecte". J'avoue qu'il m'a fallu de la *Grande encyclopédie* pour 'imaginer' qu'il s'agissait de ce coléoptère-phytophage qui a donné son nom au groupe des Criocérides. Et malgré cette aide, je n'ai pas pu savoir s'il s'agit d'un *Crioceris asparagi*, d'un *C. merdigera* ou d'un *C. duodecimpunctata*.

Les erreurs matérielles sont très rares. (xii) Canut pour Canu; l'erreur se répète partout à propos de ce nom. (64) On parle d'un *Traité de versification* de Théodore de Banville; il s'agit de son *Petit traité de poésie française* (Paris, 1872). (64 n. 2) Le renvoi au susdit *Petit traité* est défectueux; manque aussi le renvoi à la page: il s'agit de la page 8. (64 n. 3) On cite deux quatrains de Banville avec, pour référence, 'id. 273'. 'Id.' est pour *Odes funambulesques*, ce qui ne ressort pas du contexte. Il s'agit de la 2^e édition (Paris, 1859). (375-81)

L'index général, lacunaire et très incomplet, n'a qu'une fonction purement symbolique. (383-4) La table des matières renferme un certain nombre d'erreurs: le chapitre 3 du Livre premier débute à la page 55 et non 56; le chapitre 1 du Livre second débute à la page 79 et non 80; le chapitre 5 du même Livre commence à la page 163 et non 164; le chapitre 7 du Livre troisième commence à la page 327 et non 325.

4. CONCLUSION

Les réserves qui précèdent ne visent pas à mettre en cause la valeur de cet ouvrage, mais plutôt à poursuivre l'effort qu'il amorce et à tirer profit du message qu'il renferme. Le caractère linguistique du périodique qui accueille ce compte-rendu et l'orientation théorique et méthodologique que j'ai adoptée m'ont amené à passer sous silence de nombreuses contributions originales à l'histoire littéraire de l'époque dite réaliste, qui témoignent de l'incontestable maîtrise de Bruneau. Je ne répare que partiellement cette injustice en disant que, par cet ouvrage, l'auteur s'impose comme l'un des maîtres incontestés de la critique universitaire française et s'affirme, une fois de plus, comme le digne successeur d'une longue et belle tradition.

Si j'ai cru devoir soulever certaines objections d'ordre méthodologique, je n'oublie pas qu'elles ne valent qu'au niveau très élevé où Bruneau a placé le cadre de sa recherche. Si la cohérence de son système stylistique m'a parfois semblé douteuse, je n'oublie pas que c'est justement à Bruneau que l'on doit de nous avoir enseigné l'exigence en ces matières, en nous prévenant contre l'incohérence et le relâchement de nos méthodes. Si la méthode qu'il nous a proposée à son tour a souffert d'être confrontée avec les techniques d'investigation linguistique, n'oublions pas que c'est Bruneau lui-même qui nous a invité à entreprendre cette prometteuse confrontation. Si les définitions sur lesquelles s'appuie sa stylistique ne nous ont pas toujours satisfaits, rappelons-nous encore que c'est toujours lui qui nous a prévenu contre les pièges tendus par la terminologie traditionnelle et qui nous a demandé de bien définir ce dont nous parlons. Disons enfin que si, dans son ensemble, la doctrine stylistique offerte par l'*Histoire* nous a parfois semblé manquer de rigueur et de cohérence, c'est peut-être dans la mesure où elle reflète, sinon la nature des choses, du moins l'état actuel de nos moyens d'investigation en ces matières.

Il est toujours facile de critiquer les pionniers. En matière d'analyse des faits littéraires, Bruneau en est un. Sa doctrine représente un pas considérable en avant, sinon par les principes qu'elles renferme ou par les résultats auxquels elle aboutit, du moins par son orientation hardie vers un examen plus positif des faits littéraires.

PUBLICATIONS RECEIVED

This listing acknowledges the receipt of recent works that appear to bear on the scientific study of language. No book can be returned to the publisher, nor can the Editor promise that every book received will be reviewed in this journal. Reviews are published as circumstances permit, and copies are sent to the publishers of the works reviewed.

- Abrégé de langue malgache. By Faublée. Pp. 72, mimeographed. Paris: Librairie C. Klincksieck, n.d.
- Acta linguistica Academiae Scientiarum Hungaricae 3.1-209 (1953).
- Aegyptus 33.1-192 (1953).
- African languages and English in education: A report of a meeting of experts on the use in education of African languages in relation to English, where English is the accepted second language, held at Jos, Nigeria, November 1952. (Educational studies and documents, Vol. 2.) Pp. 91. Paris: Unesco, Education Clearing House, [1953].
- Afrika und Übersee 37.145-92 (1953); 38.1-48 (1953).
- Allgemeine Stilistik. By Herbert Seidler. Pp. 366. Göttingen: Vandenhoeck & Rupprecht, 1953.
- Allgemeine und vergleichende Sprachwissenschaft Indogermanistik; Keltologie. By Vittore Pisani; Julius Pokorny. (Wissenschaftliche Forschungsberichte, geisteswissenschaftliche Reihe, Vol. 2.) Pp. 199. Bern: A. Francke AG. Verlag, 1953.
- Anales del Instituto de Lingüística 5.1-447 (1952).
- Analyse et enregistrement de la voix parlée et chantée. By Henri Malhiac. Pp. 71. Toulouse: Société Générale d'Impression, 1953.
- Annual report of National Research Institute 1951-1952. Pp. 152. Tokyo: Yotuya, Sinzyuku, 1952.
- Archiv für Orientforschung 16.197-424, plates ix-xviii (1953).
- Archivos Venezolanos de Folklore 1.235-469 (1952). Universidad Central de Venezuela, Facultad de Filosofía y Letras; Caracas.
- Asia major NS 3.117-238 (1952).
- The auxiliary *do*: The establishment and regulation of its use in English. By Alvar Ellegård. (Gothenburg studies in English, Vol. 2.) Pp. 320. Stockholm: Almqvist & Wiksell, 1953.
- Bahasa dan budaja 1:5.1-40 (1953).
- Beowulf and Judith. Ed. by Elliott Van Kirk Dobbie. (The Anglo-Saxon poetic records: A collective edition, Vol. 4.) Pp. c, 289. New York: Columbia University Press, 1953.
- Bibliographie linguistique de l'année 1951 et complément des années précédentes. Publiée par le Comité International Permanent de Linguistes ... (Publiée sous les auspices du Conseil International de la Philosophie et des Sciences humaines.) Pp. xxxi, 298. Utrecht-Anvers: Spectrum, 1953.
- La bibliothèque et les publications de l'Académie Slovène des Sciences et des Arts dans les années 1938-1951. By Primož Ramovš. (Academia Scientiarum et Artium Slovenica, Bibliotheca, No. 1.) Pp. 137. Ljubljana, 1952.
- Bulletin analytique: Philosophie 7.451-710 (1953).
- Bulletin of the School of Oriental and African Studies 15.423-632 (1953). University of London.
- The classical weekly 46.233-62 (1953); 47.1-80 (1953-54).
- Contribution to methods in onomastics: Topo- and choronyms and their origin [in Ukrainian]. By G. M. Lucyk. (Onomastica, No. 6.) Pp. 23. Winnipeg: Ukrainian Free Academy of Sciences, 1953.
- Cultura neolatina 12.185-300 (1952).
- Danske metrikere: 1. Fra Bielke til Gerner. Ed. by Arthur Arnholtz, Erik Dal, Aage Kabell.

- (Universitetsjubilæets Danske Samfund, Publikation nr. 365, bekostet af Carlsberg-fondet.) Pp. xv, 410 including 24 plates. København: J. H. Schultz Forlag, 1953.
- Destiny and motivation in language: Studies in psycholinguistics and glossodynamics. By A. A. Roback. Pp. 474. Cambridge, Mass.: Sci-Art Publishers, 1954.
- Der Dialog 'Kratylos' im Rahmen der platonischen Sprach- und Erkenntnisphilosophie. By Josef Derbolav. (Schriften der Universität des Saarlandes.) Pp. 120. Saarbrücken: West-Ost-Verlag, 1953.
- Esquisse d'une syntaxe structurale. By Lucien Tesnière. Pp. 30, offset. Paris: Librairie C. Klincksieck, 1953.
- Études germaniques 8.241-320 (1953).
- Evolution of Malayalam. By Anantaramayyar Chandra Sekhar. (Deccan College dissertation series, No. 10.) Pp. [vii], 220. Poona: Deccan College Postgraduate and Research Institute, 1953.
- Fair, foul, nice, proper: A contribution to the study of polysemy. By Arne Rudskoger. (Gothenburg studies in English, Vol. 1.) Pp. xi, 505. Stockholm: Almqvist & Wiksell, 1952.
- Finnisch-ugrische Forschungen 31.1-148, Anzeiger 1-75 (1953).
- Frei Antônio das Chagas: Um homem e um estilo do Séc. XVII. By Maria de Lourdes Belchior Pontes. (Instituto de Alta Cultura; Publicações do Centro de Estudos Filológicos, Vol. 5.) Pp. xx, 503. Lisboa, 1953.
- The French expressions for spirituality and devotion: A semantic study. By Sister Lucy Tinsley. (Catholic University of America, Studies in Romance Languages and Literatures, Vol. 47; Catholic University dissertation.) Pp. xxvii, 302. Washington Catholic University of America Press, 1953.
- Gengo kenkyu, Nos. 22/23 (1953). Journal of the Linguistic Society of Japan: Tokyo.
- Geschichte der griechischen Sprache, I. Bis zum Ausgang der klassischen Zeit. By Otto Hoffmann, 3d ed. by Albert Debrunner. (Sammlung Götschen, Vol. 111.) Pp. 156. Berlin: Walter de Gruyter & Co., 1953.
- Gestirnnamen bei den indogermanischen Völkern. By Anton Scherer. (Indogermanische Bibliothek: 3. Reihe, Untersuchungen: Forschungen zum Wortschatz der indogermanischen Sprachen, No. 1.) Pp. 276, plates i-viii. Heidelberg: Carl Winter, Universitätsverlag, 1953.
- Handbuch der italischen Dialekte; Vol. 1: Texte mit Erklärung, Glossen, Wörterverzeichnis. By Emil Vetter. (Indogermanische Bibliothek; erste Reihe, Lehr- und Handbücher.) Pp. viii, 447. Heidelberg: Carl Winter, Universitätsverlag, 1953.
- Handbuch des Gotischen. By Wolfgang Krause. (Handbücher für das germanistische Studium.) Pp. xx, 306. München: C. H. Beck'sche Verlagsbuchhandlung, 1953.
- Handbuch des Sanskrit: Eine Einführung in das sprachwissenschaftliche Studium des Altindischen; II: Texte und Glossar. By Albert Thumb, 2d edition by Richard Hauschild. (Indogermanische Bibliothek; erste Reihe, Lehr- und Handbücher.) Pp. xii, 356. Heidelberg: Carl Winter, Universitätsverlag, 1953.
- An historical dictionary of German figurative usage. By Keith Spalding, with the assistance of Kenneth Brooke. Fasc. 3, pp. 89-136. Oxford: Basil Blackwell, 1953.
- Historyczna posieć perska: Materiały do studiów nad współczesną literaturą Iranu. By Franciszek Machalski. (Biblioteka Wschodnia, No. 4.) Pp. 156. Kraków: Nakład Polskiego Tow. Orientalistycznego, 1952.
- Humanitas 1.1-439 (1953) Revista de la Facultad de Filosofía y Letras, Universidad Nacional de Tucumán; Argentina.
- Interlingua a prime vista. By Alexander Gode. Pp. 79. New York: Storm Publishers, 1954.
- International anthropological and linguistic review 1.107-200 (1953).
- International journal of American linguistics 19.253-322 (1953); 20.1-82 (1954).
- Italica 30.129-256 (1953).
- Jazykovedný sborník Slovenskej Akadémie Vied a Umení 5.1-328, plates i-xx (1951). Bratislava: Nakladateľstvo Slovenskej Akadémie Vied a Umení.
- Jezik 2.1-32 (1953).

- Journal of the Oriental Institute 2.303-410, 49-56 (1953); 3.1-110, 57-64 (1953). Baroda.
- Kształtowanie się mowy dziecka [Allgemeine Übersicht über die Gestaltung der Sprache des Kindes]. By Leon Kaczmarek. (Poznańskie Towarzystwo Przyjaciół Nauk, Wydział filologiczno-filozoficzny, Prace Komisji Filologicznej, Tom 15, Zeszyt 2.) Pp. 91 [German summary, 69-79]. Poznań: Nakład Poznańskiego Towarzystwa Przyjaciół Nauk, 1953.
- Langage et versification d'après l'oeuvre de Paul Valéry: Étude sur la forme poétique dans ses rapports avec la langue. By Pierre Guiraud. Pp. 239. Paris: Librairie C. Klincksieck, 1953.
- Language and history in early Britain: A chronological survey of the Brittonic languages first to twelfth century A.D. By Kenneth Jackson. Pp. xxvi, 752. Cambridge, Mass.: Harvard University Press, 1953.
- Language teaching: A guide for teachers of foreign languages. By Edwin T. Cornelius Jr. Pp. vii, 168, offset. New York: Thomas Y. Crowell Company, 1953.
- La langue étrusque et ses origines. By A. Carnoy. (Extrait de *L'Antiquité classique*, Tome 21, no. 2.) Pp. [43]. Bruxelles, 1952.
- Letters of Noah Webster. Ed. with an introduction by Harry R. Warfel. Pp. xlv, 562. New York: Library Publishers, 1953.
- Leuvense bijdragen 43.1-52 (1953).
- Levende talen, Nos. 171, 172 (1953).
- Lingua 3.371-494 (1953).
- Lingua nostra 14.61-120 (1953).
- Lingua posnaniensis, Vol. 4 (1953), supplement: Biuletyn fonograficzny 1.1-106.
- Lulelappsk ordbok (Lulelappisches Wörterbuch). By Harald Grundström. (Skrifter utgivna genom Lansmåls- och Folkminnesarkivet i Uppsala, Ser. C.1.) Fasc. 12.1: Grammatische Übersicht über die lulelappische Dialekte, 1. Deklination, pp. 1713-1843. Uppsala: A.-B. Lundequistska Bokhandeln; København: Einar Munksgaard, 1953.
- Le maître phonétique III.31.27-46 (1953).
- Man 53.81-144 (1953).
- A method for collecting dialect. By Frederic G. Cassidy, with the collaboration of Audrey R. Duckert. (Publication of the American Dialect Society, No. 20.) Pp. 96. Gainesville, Fla., 1953.
- Namn och bygd 39.81-177 (1951); 40.1-194 (1952).
- De Nederduytsche Grammatica ofte Spraec-konst. By Christiaen van Heule, ed. by W. J. H. Caron. (Trivium: Oude Nederlandse Geschriften op het gebied van de Grammatica, de Dialectica en de Rhetorica, Nr. 1, Deel 1.) Pp. lvi, 111. Groningen: J. B. Wolters, 1953.
- De Nederduytsche Spraec-konst ofte Tael-beschrijvinghe. By Christiaen van Heule, ed. by W. J. H. Caron. (Trivium, Nr. 1, Deel 2.) Pp. 117. Groningen: J. B. Wolters, 1953.
- Neophilologus 37.193-256 (1953); 38.1-80 (1954).
- Nueva revista de filología hispánica 7.1-336 (1953) = Homenaje a Amado Alonso.
- Old-Church-Slavic as a religious cult language [in Ukrainian]. By G. M. Lucyk. (Slavistica, No. 17.) Pp. 32. Winnipeg: Ukrainian Free Academy of Sciences, 1953.
- Orbis 2.297-589 (1953).
- Orientalia NS 22.329-452, 57*-63* (1953).
- Orientalia suecana 2.1-36 (1953).
- Orientalistische Literaturzeitung 48.341-560 (1953).
- L'origine du nom des Ruthènes. By Boris Unbegaun. (Onomastica, No. 5.) Pp. 13. Winnipeg: Ukrainian Free Academy of Sciences, 1953.
- Origines des noms de familles en Belgique. By Albert Carnoy. Pp. 408. Louvain: Editions Universitas, 1953.
- Papers from the Symposium on American Indian linguistics held at Berkeley July 7, 1951. (University of California publications in linguistics, Vol. 10, pp. 1-68.) Pp. [v], 67. Berkeley and Los Angeles: University of California Press, 1954.
- Para o estudo da fonêmica portuguesa. By J. Mattoso Câmara Jr. (Coleção 'Rex', No. 11.) Pp. 176. Rio de Janeiro: Edição da 'Organização Simões', 1953.

- Petite introduction à la psychomécanique du langage. By Roch Valin (Cahiers de linguistique structurale, Faculté des Lettres de l'Université Laval, No. 3.) Pp. 91. Québec: Les Presses Universitaires Laval, 1954.
- The philosophy of symbolic forms; Vol. 1. Language. By Ernst Cassirer, transl. by Ralph Manheim, with preface and introduction by Charles W. Hendel. (Published on the Louis Stern Memorial Fund.) Pp. xiv, 328. New Haven: Yale University Press, 1953.
- Polsko-laskie pogranicze językowe na terenie Polski [La frontière linguistique polono-lach sur le territoire de la Pologne]. By Karol Dejna. (Societas Scientiarum Lodziensis, Sectio 1, Nos. 12, 15.) Vol. 1, Atlas, pp. 408; Vol. 2, pp. 270, with folding maps. Łódź, 1953.
- Pravidlá slovenského pravopisu: S pravopisným a gramatickým slovníkom. Pp. 408. [Bratislava]: Slovenská Akadémia Vied, 1953.
- Profilo di storia linguistica italiana. By Giacomo Devoto. Pp. [vii], 173. Firenze: La Nuova Italia, 1953.
- Recherches sur *khṛē*, *khṛēsthai*: Étude sémantique. By G. Redard. (Bibliothèque de l'École des Hautes Études, Sciences historiques et philologiques, Fasc. 303.) Pp. 122. Paris: Librairie Ancienne Honoré Champion, 1953.
- Report of the fourth annual round table meeting on linguistics and language teaching. Ed. by Archibald A. Hill. (Monograph series on languages and linguistics, No. 4.) Pp. v, 116. Washington: Institute of Languages and Linguistics, School of Foreign Service, Georgetown University, 1953.
- Research on vocabulary in women's magazines. [Ed. by Nisio Minoru.] Pp. 338. Tokyo: Yotuya, Sinzyuku, 1953.
- Revista colombiana de antropología 1.1-458 (1953). Organo del Instituto Colombiano de Antropología [new series replacing *Revista del Instituto Etnológico Nacional* and *Boletín de arqueología*]; Bogotá, Colombia.
- Rocznik orientalistyczny 16.i-xlvi, 1-577, plates i-xii (1950); 17.i-xxxvi, 1-480, plates i-v (1951-52). Wydaje Polskie Towarzystwo Orientalistyczne, Kraków.
- The Russian word count and frequency analysis of grammatical categories of standard literary Russian. By Harry H. Josselson. Pp. 274. Detroit: Wayne University Press, 1953.
- Schriftsprache und Mundarten: Ein Überblick über ihr Verhältnis und ihre Zwischenstufen im Deutschen, 2nd ed. By Walter Henzen. (Bibliotheca germanica, No. 5.) Pp. 303. Bern: Francke Verlag, 1954.
- Scottish Gaelic studies 7.105-208 (1953).
- The semantic spectrum of the Russian infinitive. By A. G. F. van Holk. Pp. xii, 110, with folding chart. Leiden: A. W. Sijthoff's Uitgeversmaatschappij N.V., 1953.
- Slavića canadiana A.D. 1953. By Jaroslav B. Rudnyč'kyj. (Slavistica, No. 18.) Pp. 32. Winnipeg: Ukrainian Free Academy of Sciences, 1953.
- Slovenské odborné názvoslovie 1.161-224 (1953). Vydáva Nakladateľstvo Slovenskej akadémie vied; Bratislava.
- Slovo 2.1-91 (1953).
- Speculum 28.643-922 (1953).
- Sprachlich-kulturhistorische Untersuchungen vornehmlich über den deutschen Einfluss im Lettischen. By Johannes Schwes. (Veröffentlichungen der Abteilung für slavische Sprachen und Literaturen des Osteuropa-Instituts an der Freien Universität Berlin, Band 4.) Pp. xvi, 446. 2nd ed., unchanged [1st ed. 1936]. Berlin (In Kommission bei Otto Harrassowitz, Wiesbaden), 1953.
- The structure and development of Russian. By W. K. Matthews. Pp. ix, 225. Cambridge: At the University Press, 1953.
- Strukturele analyse van visueel taalgebruik binnen een groep dove kinderen. By Bernard Theodoor Marie Tervoort S.J. (Academisch proefschrift ...) Deel 1: Tekst, pp. 308; Deel 2: Materiaal, Register, enz., pp. 188. Amsterdam: N.V. Noord-Hollandsche Uitgevers Maatschappij, 1953.
- Studia linguistica 6.1-112 (1952).
- Studien über sprachökonomische Entwicklungen im Deutschen. By W. H. A. Koenraads. Pp. xxviii, 191. Amsterdam: J. M. Meulenhoff, 1953.

- Studien zur Geschichte des Infinitivs im Griechischen. By Penntti Aalto. (*Annales Academiae Scientiarum Fennicae*, Ser. B, Vol. 80.2.) Pp. 116. Helsinki: [Suomalainen Tiedeakatemia], 1953.
- The study of language: A survey of linguistics and related disciplines in America. By John B. Carroll. Pp. xi, 289. Cambridge: Harvard University Press, 1953.
- Thesaurus 8.1-312 (1952).
- Tijdschrift voor nederlandse taal- en letterkunde 71.241-323 (1953).
- Transactions of the Philological Society 1953. Pp. 179, xiii. Oxford: Basil Blackwell, 1953.
- 1200 Chinese basic characters: An adaptation for students of Cantonese of W. Simon's National Language version. By K. P. K. Whitaker, with an introduction by W. Simon. Pp. xlii, 316. London: Lund Humphreys, 1953.
- Über die Wortfolge im Finnischen und Urfinnischen. By Lauri Kettunen. (*Annales Academiae Scientiarum Fennicae*, Ser. B, Tom. 81, No. 3.) Pp. 67. Helsinki, 1953.
- The use of vernacular languages in education. (Monographs on fundamental education, Vol. 8.) Pp. 156. Paris: Unesco, 1953.
- Verzeichnis der etymologisch behandelten finnischen Wörter: II. 1933-1950. By Elvi Erämetsä. (*Annales Academiae Scientiarum Fennicae*, Ser. B, Vol. 77.1.) Pp. 174. Helsinki: Suomalainen Tiedeakatemia, 1953.
- A vida de Santo Aleixo. By Dinorah da Silveira Campos Pecoraro. (Tese de doutoramento apresentada à cadeira de Filologia e Língua Portuguesa da Faculdade de Filosofia, Ciências e Letras da Universidade de São Paulo.) Pp. 234, [2]. São Paulo, 1951.
- Virittäjä 57.241-432 (1953).
- Zeitschrift für Phonetik und allgemeine Sprachwissenschaft 7.1-288 (1953).
- Zeitschrift für vergleichende Sprachforschung 71.129-252 (1954).

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DELIMITATION OF SYNTACTIC UNITS

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In the present state of our techniques we may assume that we know how to isolate morphemes properly—that is, unequivocally and without unaccountable residue. It is not so certain that we know how to isolate words, and hence how to separate morphology from syntax. Given a viable definition of the word for some particular language, we come finally to the problem of assigning words properly to relevant syntactic units such as phrases, clauses, and sentences. To illustrate one possible technique of discovering the syntactic framework of a language, I shall analyze a short text of Ponapean, a Micronesian language of the Eastern Caroline Islands.¹

The text is an excerpt from a spontaneous conversation recorded on tape and later transcribed. Roman numerals mark four UTTERANCE UNITS—stretches of speech activity by a single speaker preceded and followed by silence.² Each utterance unit consists of one or more CONTOURS (marked by slant lines), defined as a phoneme sequence between two audible pauses or between pause and silence, such that all morphophonemic processes take place within the contour and only within this. Every contour has one primary stress and a pitch sequence; it may also have secondary stresses. (There are differences among Ponapean contours which affect the classification of these segments but not the segmentation itself. They will be disregarded in this paper.)

In the text, numbers in parentheses identify the individual words (but hesitation noises are not numbered). The word is defined, for Ponapean, as a sequence of morphemes and morpheme clusters in fixed order. Sequences with variable order consist of separate one-morpheme words.

The following phoneme symbols appear in the text: SHORT VOWELS a ɔ o e u i, ə; LONG VOWELS aː ɔː oː eː uː iː; CONSONANTS p t t̚ k m n ŋ s r l; STRESSES primary (acute) and secondary (grave), unmarked vowels being unstressed; PITCHES rising from 1 to 4, with pitch marks written after the vowel to which they apply and unmarked vowels having the same pitch as the last preceding marked vowel.

The text is followed by a verbatim translation; every numbered word in the text is glossed by one or more words preceded by the same number. A superior H designates an honorific form.

I. (1) áː²³/ (2) à²ramasakan (3) mwáw³ (4) kà²matipwè⁴/ (5) nàː³npey (6) pwé²ki/ (7) yè²t̚ (8) só²net̚/ (9) mè³ (10) re (11) mwáw²ki/ ə²/ (12) má⁴/ (13) kó²m (14) paː³n/ (15) kà³mati²pwa (16) náː³npey⁴/ (17) a² (18) kóm (19) paː³n/ (20) kà²t̚ila (21) lúː¹k/ (22) a¹ (23) kòmwi (24) pu²r̥³to²/ (25) kò¹mwi (26)

¹ A preliminary form of this paper was read at the meeting of the Linguistic Society in New York, 28 December 1953.

² Cf. the definition given by C. C. Fries, *The structure of English* 23 (New York, 1952): 'any stretch of speech by one person before which there was silence on his part and after which there was also silence on his part.'

kaó³no²pata³/ (27) ð²mwi (28) kamá²tip/ (29) a² (30) ná²npey³ (31) á²m (32) pa²n/ (33) ku²puré²ta⁴/ (34) ì²s (35) me (36) á (37) pa²n/ (38) káw¹⁻³lò³⁻²ŋ/ (39) ð²mwi (40) ù²mwəf²yo/

II. (41) éy³⁻²/

III. (42) éy³⁻²/ (43) í⁴ (44) tu²we³/ (45) a²ri (46) ná²npey³ (47) pà²n/ (48) ka²títò²ŋ (49) ní²n (50) kámati²pwo⁴/ (51) á²p/ (52) tú²we (53) ʔa/ (54) káw¹⁻³lò³⁻²ŋ/-f²ye³/ (55) ó²m/ (56) kà²matf²pwo/ (57) a²rf³/ (58) kà²piko³ (59) kò²rú²si²ya/

IV. (60) kò²m (61) pa²n (62) sa²pwl³li²ma²nikí²la³/

I. (1) well (2) people (3) like (4) give feasts to (5) Nahnpey [a rank title] (6) because (7) *voici* (8) this manner (9) which (10) they (11) like (12) if (13) you^H [sg.] (14) will (15) give feast to (16) Nahnpey (17) and/but (18) you^H (19) will (20) go there^H (21) invite (22) and/but (23) you^H (24) return (25) you^H (26) prepare (27) your^H (28) feast (29) and/but (30) Nahnpey (31) however (32) will (33) think over (34) who (35) that/which (36) he (37) will (38) give to (39) your^H (40) that stone oven feast

II. (41) yes

III. (42) yes (43) this one (44) thus (45) well then (46) Nahnpey (47) will (48) come to^H (49) in (50) that feast (51) but (52) thus (53) only (54) gives me (55) your (56) that feast (57) well then (58) yams (59) all of them

IV. (60) you^H (61) will (62) receive^H

It is a commonplace that phonemic and morphemic segments need not coincide. An inspection of the contour boundaries in our text shows that none of them cuts through a morpheme: every contour boundary is a morpheme boundary (though not of course conversely). But when we test our contours for their word content, we find that not every contour boundary is a word boundary. Some contours contain several words (for instance 2-4 and 34-37); but the single word 54 belongs to two different contours. We conclude that contours cannot be considered the framework of word distribution: they are not syntactic units. For the same reason, utterance units, being composed of contours, are not syntactic units either.

Turning to nonphonemic criteria for delimiting the syntactic units, we shall apply the REGISTRATION OF DEPENDENCES described by Hjelmslev and the SUBSTITUTION TECHNIQUE of Harris.³ We shall apply these tests to words and word sequences. By the latter term I mean an unanalyzed succession of words, as opposed to syntactic units, which are properly defined successions. The dependences to be registered are three relations of presupposition (mutual, bilateral, unilateral) and one of mutual tolerance.⁴ For our present purpose these are defined as follows: two words or word sequences are MUTUALLY DEPENDENT if each is a necessary condition for the occurrence of the other; one word or word sequence is BILATERALLY DEPENDENT on two others if both the latter are necessary

³ Louis Hjelmslev (transl. F. J. Whitfield), *Prolegomena to a theory of language* (Baltimore, 1953); Z. S. Harris, From morpheme to utterance, *Lg.* 22.161-83 (1946).

⁴ These terms are used in preference to Hjelmslev's tripartite division into interdependence (mutual dependence), determination (both bilateral and unilateral dependence), and constellation (mutual tolerance).

conditions for the occurrence of the first; one word or word sequence is **UNILATERALLY DEPENDENT** on another if the latter alone is a necessary condition for the occurrence of the former; two or more words or word sequences are **MUTUALLY TOLERANT** if they can occur together but none is a necessary condition for the occurrence of any other. In applying the substitution technique, a substitution is considered **RIGHT** if it produces no change in the previously registered dependences. Sequences suitable for a right substitution are **RIGHTLY SUBSTITUTABLE**. From what has been said it is clear that rightly substitutable sequences have identical dependences and are therefore syntactically equivalent.

The first segmentation of our text yields a number of words and word sequences which are mutually tolerant. These are: 1; 2-11; 12-24; 25-40; 41; 42; 43-44; 45-56; 57-62. We shall call each of these units a **SENTENCE**, defined for Ponapean as a word or word sequence mutually tolerant with all other word sequences in the same text.

Some of the sentences consist of a single word (1, 41, 42); others contain several words, which exhibit various kinds of dependence among themselves. We shall analyze these, leaving aside for the time being the short sentence 43-44.

Certain of our sentences (2-11, 12-24, 25-40, 45-56) contain shorter sequences which in other texts occur in mutual tolerance as independent sentences but upon which, in this text, certain other words are bilaterally dependent. Thus, word 6 is bilaterally dependent on the sequences 2-5 and 7-11; words 12, 17, and 22 are bilaterally dependent on the sequences 13-16, 18-21, and 23-24; word 29 is bilaterally dependent on the sequences 25-28 and 30-40; and word 51 is bilaterally dependent on the sequences 45-50 and 52-56. The bilaterally dependent words here listed are **CLAUSE LINKS**; each of the enumerated sequences is a **CLAUSE**, defined for Ponapean as one of the two or more sentence-like word sequences on which a clause link is bilaterally dependent. Any sentence (such as 57-62) which does not contain clauses as just defined will be described as consisting of a single clause.

In some clauses, one word is unilaterally dependent on the rest of the clause; thus word 45 is dependent on 46-56, and word 57 is dependent on 58-62. Such words are **INTRODUCERS**. In what follows we shall disregard them.

Within each clause of the sentences dealt with so far, we find certain words and short sequences which are **UNCONDITIONALLY DEPENDENT** on other words or sequences. By this term we mean that they are unilaterally dependent on these words or sequences not only in this text but in any text of the language. In the following list, words and sequences to the left of the line are unconditionally dependent on those to the right of the line:

9-11	8	31-32	33	52-53	54
13-14	15	35-40	34	55	56
18-19	20-21	47	48	59	58
25	26	49	50	60-61	62
27	28				

Words and sequences which are unconditionally dependent on others are **MODIFIERS**; a word or sequence upon which a modifier is dependent is a **HEAD**. All

multi-word modifiers are rightly substitutable for appropriate one-word modifiers; their internal structure varies considerably, but will not be investigated here. A head is a **HEAD WORD** if it consists of a single word, a **HEAD EXPRESSION** if it consists of several words. All head expressions are rightly substitutable for appropriate head words; their internal structure varies considerably, but (again) will not be investigated here. Any head together with its modifier is a **PHRASE**.

This leaves words 2, 3, 4, 5, 7, 16, 30, and 46 unaccounted for. Since none of these is unconditionally dependent on any other, they are not modifiers; we shall regard them as heads. (The sequence 3-4 can be replaced, in right substitution, by a single head word; it is thus a head expression.) Any such head without a modifier is rightly substitutable for an appropriate head-and-modifier phrase.

The phrases and unmodified heads within a clause exhibit various relations of unilateral and mutual dependence, which affect the classification of these segments but not the segmentation itself. They will be disregarded in this paper.

We now return to the short sentence 43-44. This consists of two head words, for which an appropriate single head word is rightly substitutable. Since such a head word is also rightly substitutable for a phrase of head plus modifier, we regard the sequence 43-44 as also a phrase. The sentence, then, is syntactically equivalent to the one-word sentences noted above (1, 41, 42).

This concludes our sample analysis. We have isolated and delimited the syntactic units of our text unequivocally and without unaccountable residue, using as our criteria a set of dependences and substitutions that would be equally applicable to any Ponapean text in the same style.

SOME GROUPS OF ANCIENT ANATOLIAN PROPER NAMES*

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Ever since it was discovered that Hittite, the language in which the bulk of the cuneiform tablets from Boğazköy is composed, is an Indo-European language¹ or a language related to Indo-European,² historians and linguists alike have been interested in the question as to the time when these people entered Asia Minor. The tablets were mostly written between 1400 and 1200 B.C., but the history of the Hittite state can be traced back to about 1900 B.C.; at that time, at the latest,

* In this article the following abbreviations are employed:

(1) Periodicals: *Afo* = *Archiv für Orientforschung*; *AHDO* = *Archives d'histoire du droit oriental*; *AJA* = *American journal of archaeology*; *Arch. Or.* = *Archiv Orientalní*; *BASOR* = *Bulletin of the American Schools of Oriental Research*; *Bibl. Or.* = *Bibliotheca orientalis*; *Dergi* = *Ankara Üniversitesi Dil ve Tarih-Coğrafya Fakültesi Dergisi*; *IF* = *Indogermanische Forschungen*; *JAOS* = *Journal of the American Oriental Society*; *JCS* = *Journal of cuneiform studies*; *JHS* = *Journal of Hellenic studies*; *JKF* = *Jahrbuch für kleinasiatische Forschung*; *KlF* = *Kleinasiatische Forschungen*; *MDOG* = *Mitteilungen der Deutschen Orient-Gesellschaft*; *Orient.* = *Orientalia*; *PSBA* = *Proceedings of the Society of Biblical Archaeology*; *RA* = *Revue d'assyriologie*; *RHA* = *Revue hittite et asianique*; *ZA* = *Zeitschrift für Assyriologie*; *ZDMG* = *Zeitschrift der Deutschen Morgenländischen Gesellschaft*.

(2) Publications of Boğazköy texts: *ABoT* = Kemal Balkan, *Ankara Arkeoloji Müzesinde bulunan Boğazköy tabletleri*; *BoTU* = E. Forrer, *Boghazköi Texte in Umschrift*; *HT* = *Hittite texts in the cuneiform character from tablets in the British Museum*; *IBoT* = Bozkurt-Çiğ-Güterbock, *Istanbul Arkeoloji Müzesinde bulunan Boğazköy tabletleri*; *KBo* = *Keilschrifttexte aus Boghazköi*; *KUB* = *Keilschrifturkunden aus Boghazköi*; *VBoT* = A. Götze, *Verstreute Boghazköi Texte*. Furthermore *Bo* (with following number) = unpublished Boğazköy tablet (quoted by museum number). *CH* = F. Hrozný, *Code hittite*.

(3) Publications of Kültepe texts: *BIN* = *Babylonian inscriptions in the collection of James B. Nies*; *CCT* = *Cuneiform texts from Cappadocian tablets in the British Museum*; *Chantre* = E. Chantre, *Mission en Cappadoce*; *Cont.* = G. Contenau, *Trente tablettes cappadociennes*; *Gol.* = W. Golénischeff, *Vingt-quatre tablettes cappadociennes*; *Hahn* = J. Lewy, *Die Kültepetexte aus der Sammlung Frida Hahn*; *ICK* = *Inscriptions cunéiformes du Kültepe*; *KTP* = F. J. Stephens, *The Cappadocian tablets in the University of Pennsylvania Museum, Journal of the Soc. of Oriental Res.* 11.101-36 (1927); *KTS* = J. Lewy, *Die altassyrischen Texte von Kültepe bei Kaisarije (Keilschrifttexte in den Antiken Museen zu Stambul)*; *Liv.* = T. G. Pinches, *Liverpool annals of arch. and anthrop.* 1.49-80 (1908); *OIP* = *Oriental Institute publications*; *TCL* = *Musée du Louvre, Textes cunéiformes*; *TMH* = *Texte und Materialien der Frau Professor Hilprecht Collection of Babylonian Antiquities im Eigentum der Universität Jena*. Furthermore *EL* = E. Eisser-Lewy, *Die Altassyrischen Rechtsurkunden vom Kültepe*.

(4) Unpublished tablets: *NBC* = Nies Babylonian Collection (Yale University); *VAT* = Vorderasiatische Abteilung (der Staatlichen Museen in Berlin) Tontafel.

(5) More frequently quoted publications: *Laroche, Rec.* = E. Laroche, *Recueil d'onomastique hittite*; *Laroche, Rech.* = E. Laroche, *Recherches sur les noms de dieux hittites*; *Sommer, AU* = F. Sommer, *Die Aḫḫijawā-Urkunden*; *Sommer, Bil.* = F. Sommer (and A. Falkenstein), *Die hethitisch-akkadische Bilingue des Ḫattušili I.*

¹ This is the opinion of the majority of scholars. See most recently Holger Pedersen, *Hittitisch und die anderen indoeuropäischen Sprachen* (Copenhagen, 1938), and F. Sommer, *Hethiter und Hethitisch* (1947).

² This view has been advocated by E. H. Sturtevant. See his *Comparative grammar of the Hittite language*, revised edition (New Haven, 1951).

Hittites must have been present in Anatolia. The problem of their origin has been complicated by the fact that the Hittite language is not isolated in the peninsula, but forms part of an 'Anatolian' group of languages³ to which Luwian and Palaic—likewise attested in the Boğazköy material—and furthermore the language of the inscriptions written in Hittite hieroglyphs also belong. The latter can at present be traced back to the middle of the 2nd millennium but for the most part cover the early centuries of the 1st millennium.

The period which preceded the creation of the Hittite state is illuminated by the so-called 'Cappadocian' tablets,⁴ records of Assyrian merchants which come from a site close to the 'Kültepe', the mound under which the ancient city of Kaniš lies buried. They are dated to the 20th century B.C. and are written in a type of Akkadian which must be called Old Assyrian. However, the tablets contain numerous proper names of Anatolian natives⁵ with whom the Assyrians came into contact. These names must be collected and analyzed, and the problem of their relationship to Hittite and the other Anatolian languages must be investigated.

It is universally agreed that part of the Kültepe names must be explained from Hattic, i.e. from a language which was spoken in eastern Anatolia by a pre-Hittite (and pre-Luwian and pre-Palaic) population, and is preserved in remnants on Boğazköy tablets.

Beyond this, several scholars claim to have discovered in the material various substrate languages. B. Landsberger has introduced the term 'Proto-Luwian',⁶ by which—contrary to linguistic parlance customary in the United States—he means a pre-Luwian, neither Indo-European nor (Proto-)Indo-European language. J. Lewy classifies many names of Anatolians as 'Hurrian' or 'Old Hurrian'.⁷ I. J. Gelb finds in them the reflex of the presence of Indo-Europeans or Proto-Indo-Europeans among the early population of Anatolia.⁸

My own endeavors in this direction are more limited in scope. Before the larger problem of the ethnic composition of the Anatolian population at the beginning of the 2nd millennium can be tackled and problems of paleolinguistics are discussed, we must gradually try to eliminate those groups of proper names which, by their basic elements and by their morphological structure, we can assign to

³ Cf. Sturtevant, l.c. 5 ff.

⁴ For a resumé see A. Götze, *Kleinasien in Kulturgeschichte des Alten Orients* 3:1.61 ff. (Handbuch der Altertumswissenschaft 3:1.3, 1933). The more recent literature is quoted by E. Bilgiç, *AfO* 15.1 (1950).

⁵ See Bilgiç, l.c. 2 ff.

⁶ *Belleten* 3.214 f. fn. 17 (1939); *Arch. Or.* 18:1/2.341 f. fn. 67 (1950). He has recently (*JCS*, Vol. 8, in press) changed his opinion and terminology and distinguishes now between a 'muwa-language' and an 'aḫḫu-language'. To the latter—now classified as Indo-European—he assigns the names treated here as groups 1, 2, and 3.

⁷ *Arch. Or.* 18:3.318 ff. (1950), particularly 381 f. fn. 73. Lewy posits his term 'mainly for the sake of convenience', but proceeds immediately to apply to the material rules of Hurrian phonology and grammar as he understands it, i.e. to take his term at face value.

⁸ *JKF* 2.23 ff. (1951). Though Gelb is closer in substance to my own position, I cannot approve of his ranging far and wide from the columns of Hercules to the eastern borders of Iran, or of the use he makes simultaneously of cuneiform texts and of the *Gazetteer to the Times Survey Atlas of the World*.

languages of which we otherwise have some knowledge. The specific question which must be asked is this: do the Cappadocian tablets of the 20th century B.C. furnish evidence of the presence, in this peninsula, of the so-called Anatolian languages which have become known to us through the Boğazköy tablets? In the following I investigate certain groups of proper names from this point of view. The answer which I reach is affirmative, and I thus re-affirm an opinion which I first advanced in 1931 (ZA NF 6.260 ff.).

1. NAMES IN *-uman*

This group was treated in ZA NF 6.260 ff. (1931) on the basis of 17 names from Kültepe tablets, namely *A-kà-li-ú-ma-an*, *Ar-nu-ma-an*, *Iš-pu-nu-ma-an*, *Ku-pu-ú-ma-an*, *Ta-ak-ša-nu-ma-an*, *Ti-li-ú-ru-ma-an*, *Ha-ar-šu-um-nu-ma-an*, *Li-iḫ-šu-ma-an*, *Lu-ḫa-tù-ma-an*, *Ša-ak-ri-ú-ma-an*, *Ša-ak-tu-ma-an*, *Ša-li-nu-ma[-an]*, *Ši-ta-ra-ma-an*, *Ši-im-nu-ma-an*, *Šu-pi-ú-ma-an*, *Šu-pi-ša-am-nu-ma-an*, and *Tù-nu-um-na*.⁹ One of the names recurs as *Šu-up-pi-u-ma-an* (acc.) (*Šu-up-pi-um-ni* dat.) in Boğazköy (2 BoTU 12 A II 20, 24). Following a suggestion of Landsberger's it was proved that the group consists of adjectives of appurtenance mainly derived from place names. This is confirmed by the material which has accrued during the last twenty years and which comprises the following 10 new cases:

(18) *A-du-ma-an* (ICK I 119 10), (19) *Ha-ši-ú-ma-an* (VAT 9282 [Hahn 52]), (20) *Hi-iš-tù-ma-an* (OIP XXVII 59 38), (21) *Hu-ši-li-u-ma-an* (OIP XXVII 19a 21, b 21), (22) *Ki-pu-ú-ma-an* (ICK I 129 7), (23) *Lá-wa-du-ma-an* (ICK I 117 3), (24) *Šu-pu-nu-ma-an* (OIP XXVII 49a 2, b 7), (25) *Ta-wa-ú-i-ma-an* (Bilgiç¹⁰), (26) *Ū-ši-nu-ma-an* (BIN VI 222 7), *Wa-ši-nu-ma-an* (OIP XXVII 27 2), (27) *Ū-šu-nu-ma(-an)* (ICK 35b 10, cf. a 1). No. 19 belongs to *ḫa-aš-ši-i* ID-*i* 'to the ... River'¹¹ (KUB V 1 III 66), No. 20 either to ^{URU}*Ha-aš-tu-wa* (Bo 7110 unpublished) or possibly to the ^Ḫ*ḫešta* 'bone house(?)',¹² No. 12 to *ḫuššili* 'clay pit', No. 23 possibly to ^{URU}*La-a-an-da*, No. 27 (identical with No. 26?) to ^{URU}*Uš-šu-na* (KBo IV 13 I 39).

The formation is also observable in (old) Hittite gentilicia like DUMU ^{URU}*Bu-ru-uš-ḫa-an-du-um-na-an* (acc.) (2BoTU 10 II 4), LÚ ^{URU}*Uš-šu-um-na-aš* (2BoTU 14 II 6) etc.¹³ Furthermore in the archaic pronoun *kuenzumnaš* 'quoiās'.

⁹ The occurrences can be found in Bilgiç's article. Add for *Išpunuman* TCL XXI 254 5, 11; 255 5, 10, for *Šakriuman* TCL XIX 48b 17; 97 18 ff.; XXI 253 9; BIN VI 226 7; ICK I 30 pass.; 604, for *Šuppiuman* ICK I 115 5. With *Liḫšuman* compare now ^{HUR-SAG}*Li-iḫ-ša-aš* IBoT II 131 obv. 21. Besides *Tù-nu-um-na* with a thematic vowel, BIN IV 208 3 offers *Tù-nu-um-na-aš* with an additional Luwian or Hittite nominative -š.

¹⁰ The notation 'Bilgiç' refers to unpublished material quoted by Bilgiç in his article in *AfO* 15.1 ff. (1951).

¹¹ *ḫašši*- (probably *i*-stem) should be a color, cf. the common *ÍD.SA*, 'Red River', *ÍD.SIG*, 'Yellow River'.

¹² Cf. Otten, *JCS* 4.130.

¹³ With assimilation *mn > mm*, e.g. LÚ ^{URU}*Za-al-pu-u-ma-aš* (2BoTU 10b 28), LÚ ^{URU}*Ha-aš-šu-u-ma-aš* (ibid. 29), LÚ ^{URU}*Hal-pu-u-ma-aš* (ibid. 30). This explains the name *Šuppiluliumaš* and his relationship to the river and the town *Šuppiluliyāš* (KUB XVII 20 III 14 and XXII 51 obv. 11, the latter now also BIN VI 215 10). Note also LÚ ^Ḫ*ḫe-eš-tu-u-um-ni* (dat.) Bo 2866 (*JCS* 4.130 fn. 31).

The god ^PHu-ur-du-ma-na-an (acc.) (KUB XX 1 II 26, III 2), variant ^PHu-tu(-u)-ma(-na)-an (ibid. III 21, 28), may belong to the town ^{URU}Hu-ru-ul-ta-aš (2BoTU 23A III 29).

About the linguistic appraisal of the formation no agreement has been reached among competent scholars. In my opinion, it is certainly not Hattic.¹⁴ The assumption remains most natural that it originates in one of the Anatolian languages,¹⁵ and, indeed, E. Laroche now¹⁶ points to the gentilics of hH in -wan(a) as related. However this may be, the suffix must be judged not in isolation, but together with those of the groups dealt with in the sequel. These groups cannot be separated from one another and must be explained as witnesses for one and the same language.

On the analogy of the adjective *Palaumna-, to be deduced from *palaumn-ili* 'in the fashion of a Palaite' and based on the name of the country *Pa-la-a*, one would expect to find *Lukkaumna- from *Lu-uk-ka-a*. Since *Lukka*-people are in the Boğazköy texts found in the hinterland of the southern coast, I have little doubt¹⁷ that the posited gentilic survives in the name *Λυκαονία*. Other names of the same type are *Καραονία*,¹⁸ *Βαγδαονία*, and *Παφλαγονία*; the *Mήνορες* and *Ίάορες* may also belong here.¹⁹

2. NAMES IN -lika, -lka, AND -nika

The appurtenance of these exclusively female²⁰ names is proved by the pairs *Šakriuman* and *Šakrielka*, *Haršumnuman* and *Haršumalka* (< **Haršumnalka*), and perhaps *Lawaduman* and *Lapatannika*. Compare, furthermore, *Niwalka* with *Niwaḥšu* and *Šašalika* with *Šašahšu* (both below, group 3).

The evidence is as follows:

(A) *A-zu-we-el-kà* (TCL XXI 254 5), *A-zu-e-el-kà* (Cont. 26 1, 3; Hahn 51 25), *Ha-ar-šu-ma-al-kà* (OIP XXVII 49a, 1, b 6), *I-a-ta-al-kà* (VAT 13505 quoted AHDO 2.136), *I-lá-le-el-kà* (OIP XXVII 49a, 4, var. *I-lá-li-al-kà*), *I-lá-li-iš-kà* (TCL XIV 66 8 = XXI 240 4;²¹ 241 4; BIN VI 236 5; ICK I 181 29), *E-lá-li-iš-kà* (TCL XIV 78 18), *Me-nu-ze-el-kà* (BIN IV 71 19), *Ni-wa-al-kà* (ICK I 123 3), *Pé-ti-a-na-al-kà* (TCL XIV 66 7; XXI 240 3; BIN VI 236 4), *Ša-ak-re-el-kà* (CCT III 14 3), *Št-ik-re-el-kà* (BIN VI 5 4, 17), *Št-ik-re-el-kà* (BIN VI 93 3), *Ša-ša-li-kà* (TCL XXI 214 2, 5, 14).

¹⁴ I cannot understand what moves Sommer, *Hethiter und Hethitisch* 85 (1947) to claim the suffix for Hattic.

¹⁵ See already ZA NF 6.263 (1931); *Haverford symposium* 147 (1938). Landsberger, *Belleten* 3.220 fn. 4 (1939), maintains the pre-Indo-European character of the suffix, and this view is reaffirmed *Arch. Or.* 17:1/2.341 f. fn. 67 (1950), and also reflected by E. Bilgiç, *Afo* 15.1 ff. (1951). A different opinion is now offered in *JCS*, Vol. 8 (in press).

¹⁶ *Recueil* 105. He states that they are neither Hattic nor Neshite, but probably belong to a Luwian dialect. He emphasizes the neat point that the adverb *nešumn-ili* appears in the second Arzawa letter and contrasts with Hitt. *naš-ili*. But we have also *palaumn-ili*.

¹⁷ Already E. Forrer, *MDOG* 63.4 (1924).

¹⁸ Hitt. **Kalaumna-* is unknown; but an *utne katera* (ideogr. KUR ŠAPLITU) 'Lower Country' exists.

¹⁹ Cf. P. Kretschmer, *KlF* 1.1 ff.

²⁰ J. Lewy, *AHDO* 2.136 (1938).

²¹ In the latter passage the name ends in -kán, which makes it look like an accusative.

(B) *I-a-ni-kà* (OIP XXVII 49a 3, b 8), *I-a-ta-ni-kà* (TCL IV 62 14), *La?-ba-da-ni-kà* (KTP 43 4), *Ma-ga-ni-kà* (BIN VI 20 2), *Ša-am-na-ni-kà* (BIN IV 283 3, 10), *Šu-pé-a-ni-kà* (Liv. 8 2 ff.; TCL XXI 252 3; ICK 3 26; 35a 6, 7, b 7); *Šu-pt-a-ni-kà* (ICK I 19a 8, b 2).

To group (B) add from Alalakh (D. J. Wiseman, *The Alalakh tablets*, 1953) *Ar-na-ni-kà* (298 42).

In Hittite texts we find ^{SAL}Šu-šu-ma-an-ni-ga (KUB VII 1 IV 5; XXX 48 obv. 15; 49 IV 12). With this we must compare the Hurrian god ^DŠu-šu-ma-a-*hi* (KUB XXVIII 103 I 8, etc.); his name may be derived by means of the *-hi/ħe* suffix from a place name *Šušuma.

The suffix in (A) seems to be originally *-lika* (note Šašalika);²² the *i* is normally syncopated. *Ilališka*, instead of *Ilalelka*, seems to be due to dissimilation. The *e* in front of the suffix obviously develops from *-iya*, *Ilalelka* deriving from **Ilaliya-lika* (cf. Hitt. ^DIlaliyantaš KUB II 4 IV 27 [gen.], IX 34 III 35 [acc.] and the verb *ilaliya* 'desire'), *Šakrelka* from **Šakriyalka* (cf. **Šakriy-uman*). Thus *Menuzelka* must be combined with ^{URU}Me-nu-zi-ia (IBoT I 29 obv. 9) which no doubt is identical with ^{URU}Manuz(z)iya.²³ With *Šašalika* the name of the town ^{URU}Ša-a-ša-na (KUB XXXI 44 I 3, 11) is comparable. For the other names no corresponding place names have become known. However *A-zu-e* which can hardly be separated from *Azu(w)e-lka* is itself a female name (ICK I 16a 4, 7, b 3). For *Niwa-lka* see below on *Niwa-ħšu*.

As regards (B), the *-n-* in *-nika* seems to be double,²⁴ a fact which would explain the non-syncopation of the *i*. The pair *Iatalka* and *Iatannika* is instructive. We shall have to analyze **Iata-li-ka* and *Iata-nni-ka*. The meaning of the final suffix cannot be treated without reference to Palaic ^DI-la-li-an-ti-ke-eš (nom. pl.) and ^DI-la-li-an-ti-g[a-aš] (dat. pl.), ^DGul-za-an-ni-ke-eš (nom. pl.) and ^DGul-za-an-ni-ga-aš (dat. pl.) ^DŪ-li(-li)-an'-ti-ga-aš (dat. pl.) (ZA NF 14.141 rev. 14 ff.). Otten, l.c. 128 fn. 19, ascribed to the suffix plural function, but that does not carry conviction. Of the corresponding Hittite deities the *Gulšeš*²⁵ are certainly female, and the Kültepe names prove the feminine force of the suffix. Note also the name of the queen *Harapšeki*²⁶ and compare the basic **Harapša-* (cf. ^{URU}Ha-ra-pa-ša KUB XXXIV 38 6 f.) with *Aranapšu*. If so, the suffixes *-(an)ni* and *-(a)li* must make adjectives of appurtenance. Indeed *Šamna-(n)ni-ka* may be the female counterpart of *Šimn-uman* and *Labada-(n)ni-ka* may belong to ^{URU}La-an-da (if *La'anda* < **Lawa(n)da*).

It is of great importance that the suffix *-(an)nika*, with *ka* denoting the femi-

²² The structure recalls *-šepa*, *-zipa* (cf. Laroche, *RHA* 7.4 ff.); ^DHilanzipaš of the Palaic passage to be quoted presently makes this reference very pertinent. This has wider implications since the well-known Anatolian god *Κακωβος* (see L. Robert, *Hellenica* 3.66) also belongs here.

²³ Situated in Kizzuwatna; cf. ZA NF 12.92 (1940).

²⁴ This is suggested by the Hittite occurrence; the Cappadocian tablets do not indicate double consonants.

²⁵ See A. Goetze, *Tunnawi* 58 ff.; J. Friedrich, *JCS* 1.288 (1947). One obtains here a strong argument in favor of the reading *gulš-* to which I have adhered all along. The pertinence of Kültepe *Kulšan* etc. is also brought into sharper focus.

²⁶ For occurrences see *BASOR* 122.21 ff.

nine, is duplicated in Palaic. We may get here close to the 'Luwian' dialect from which the names of groups 1 and 2 are taken.

3. NAMES IN *-aḥšu*

In this group are the following names from Kültepe:

Apizi-aḥšu (Bilgiç), *Araw-aḥšu* (Bilgiç), *A-ta-aḥ-šu* (TCL XXI 238 B 4), **A(d)-du-(a)-aḥ-šu* (CCT I 23 21; 39b 8),²⁷ *Ḥapiḥšu* (Bilgiç), **Ḥapuwaḥšu* in *Ḥapuwaḥšu-šar*,²⁸ *Ḥa-dš-ta-aḥ-šu* (Hahn 36 4) and *Ḥi-iš-ta-aḥ-šu* (Gol. 20 29; TCL XXI 254 3; ICK I 19 pass., 115 4), *I-lá-li-aḥ-šu* (BIN IV 146 24; Hahn 37 13, 17) and *I-lá-li-a-aḥ-šu* (ICK I 83 24), *I-na-ra-aḥ-šu* (KTS 58a 7; TCL XX 191 15); *Ki-ni-a-aḥ-šu* TCL IV 87 15; 100 24); *Lu-úḥ-ra-aḥ-šu* (Chantre 2 14; TCL IV 87 32; KTS 58a 5; Hahn 28a 4, b 6 ff.), *Ma-ḥi-ra-aḥ-šu* (TCL IV 87 48), *Na-ki-aḥ-šu* (CCT I 37b 17), *Ni-ma-aḥ-šu* (CCT I 41b 6) and **Niwaḥšu* in *Niwaḥšu-šar*, *Pé-er-wa-aḥ-šu* (BIN IV 190 14), *Pé-ša-aḥ-šu* (OIP XXVII 19a 24), **Šašaḥšu* and **Šišaḥšu* in *Ša/išaḥšu-šar*, *Ša-da-aḥ-šu* (TCL IV 99 4; XIV 30 3; XXI 211 8; 253 3; ICK I 129 1, 15) and *Ša-ta-aḥ-šu* (TCL XX 191 32; 218a 5, b 9), *Šu-pé-a-aḥ-šu* (BIN IV 190 17; BIN VI 226 pass.; TCL XXI 253 5, 13; ICK I 172 19) and *Šu-pt(-a)-aḥ-šu* (ICK I 16a 1, b 7), *Ū-pa-ti-a-aḥ-šu* (TCL XX 191 16), *Ut-ni-aḥ-šu* (Liv. 8 16), *Wa-la-aḥ-šu* (ICK 123 19).

A-x-na-aḥ-šu (Liv. 8 16), *Ar-za-na-aḥ-šu* (Gol. 5 12; KTS 51a 11; TCL IV 87 57; XIV 33 rev. 20; ICK I 125 517), *Pu-li-ta'-na-aḥ-šu*²⁹ (TCL IV 87 32), *Ki-kàr-na-aḥ-šu* (Bilgiç), *Ku-lu-na-aḥ-šu* (TCL XIV 66 1 = XXI 240 13), **Kuninaḥšu* in *Kuninaḥšu-šar*, *Šu-pu-na-aḥ-šu* (Gol. 11 6, 17; CCT I 10b 2; 23 10; OIP XXVII 49 pass.; TCL IV 68 2; 87 35).

Iš-pu-tá-aḥ-šu (ICK I 115 8), *Ku-nu-ta-aḥ-šu* (de Clercq = EL No. 209 25; BIN IV 170 4), *Pé-ru'-ta-aḥ-šu* TCL XXI 255a 13).

In Boğazköy the group is represented by a much smaller number of occurrences, namely:

Iš-pu-taḥ-šu-uš (nom.) (2BoTU 12 B I 12); *Ta-ru-uḥ-šu-un*³⁰ (acc.) (2BoTU 23A II 8, 27); *Š[u'up-pt]-ia-aḥ-šu-uš* (nom.) (2BoT 14 + KUB XXXI 4 obv. 20).

Iš-pu-taḥ-šu also figures on the bulla from Tarsus (AJA 40.210 ff.).

When it comes to interpretation, the group must be treated jointly with the next one.

4. NAMES IN *-(a)ḥšu-šar*

The names from Kültepe are all feminines:

Ḥa-pu-wa-aḥ-šu-šar (TCL XXI 218a 4, b 4, 8); *Ḥi-iš-ta-aḥ-šu-šar* (Liv. 8 17; CCT I 238; TCL IV 87 8; EL 36 3); *Ni-wa-aḥ-šu-ša-ar* (TCL I 240 21 = *Na-aḥ-šu-ša-ra*³¹ ibid. 1) and *Ni-wa-aḥ-šu-šar* (TCL IV 33 2, 15; XXI 252 4, 6, 15),

²⁷ Both times genitive ending in *-šī*.

²⁸ Compare the name of the Cilician queen *Ervaša* (Xenophon).

²⁹ For the reading see Kraus apud Bossert, *Asia* 169.

³⁰ Probably from **Tarawaḥšu*.

³¹ Note the final thematic *a*.

Šašahšu-šar (Bilgiç) and Ši-ša-aḥ-šu-šar (CCT III 7a 2; TCL XIV 47 2; KTS 13a 4, b 3; Hahn 26 8), Ša-da-aḥ-šu-šar (ICK I 24 b 3) and Ša-ta-aḥ-šu-šar (ibid. a 3, 5).

Ku-ni-na-aḥ-šu-ša-ar (TCL XIV 67 4 ff.).

Examples from Boğazköy are lacking. This may be purely accidental; in the texts of the Old Kingdom only very few women are mentioned.

Like the names in *-uman*, the names in *-(a)ḥšu* and the corresponding feminines in *-(a)ḥšu-šar* have elicited very divergent opinions regarding their linguistic appurtenance. Sommer takes them as a matter of course as Hittite;³² Bossert³³ and Laroche³⁴ declare them to be Neshite, with the additional assumption that *-ḥšu* contains the word *ḥaššu-* 'king' in a shortened form.

One thing has been clear for a long time: the *-šar* of the feminines is identical with the suffix which in Hittite, as Ehelolf has demonstrated,³⁵ creates feminine nouns like *išḥa-(š)šara-* 'mistress' (besides *išḥa-* 'master'³⁶), *GEME-a-(š)šara-* 'slave girl' (besides *IR-na-* 'slave'), *šuppiya-(š)šara-* 'virgin', and probably also *ḥaššu-šara* 'queen' (besides *ḥaššu-* 'king').³⁷

It is now certain that in hH the word for 'queen' also ends in the suffix *-sar(a)*.³⁸ In other words it occurs in at least two of the early Anatolian languages. Since the suffix is not found in Hittite, the probability is very high that it is of 'IE' origin. The case seems to be clinched by the etymology which so cautious a scholar as J. Lohmann³⁹ has proposed.

In the circumstances one will certainly be inclined to attempt an explanation on an IE basis for *-(a)ḥšu* as well. I cannot approve the view of Bossert,⁴⁰ who interprets the second part of *Išputahšuš* as a shortened form of the noun *ḥaššuš* 'king'; this would imply that the names in *-(a)ḥšu-šar* contain the word for 'queen'. In my opinion we deal with a conglomeration of suffixes. I propose to consider *-ḥšu* as a formation like IE **sthu-* 'standing', i.e. a zero-grade element plus *-u*, resulting in a kind of participle.⁴¹ I am inclined to derive *-ḥšu* from *ḥaš-* 'engender, beget, give birth'. Hittite names of the type *Inara-ḥšu*, then, are comparable to the Greek type in *-γένης*, and those in *-(a)ḥšu-šar* to Greek names in *-γένεια*.

³² *Hethiter und Hethitisch* 85 f. But that Kültepe is 'vorhethitisch' is merely *petitio principii*; the historical situation does not recommend Sommer's interpretation, nor is it borne out by the archeological results of the excavations at Kültepe.

³³ *Asia* 155 ff.

³⁴ *Recueil* 104 f.

³⁵ *ZA* NF 9.184 ff.; he quotes in addition the name of the goddesses *Ḫa-ḥa-aš-ša-ra-aš* (KUB VI 46 II 58).

³⁶ Which has a good etymon in Lat. *erus*.

³⁷ Cf. the Kültepe name *Ḫa-šu-šar* (EL No. 36 2).

³⁸ I. J. Gelb, *Hitt. hieroglyphs* 3.18 (1942); H. Th. Bossert, *Asia* 150 ff., 171 f. (1946); P. Meriggi (correct *Glossar* 164 in accordance with *Athenaeum* NS 29.32 [1951]); J. Friedrich, *Heth. Wörterbuch* 64 (1952).

³⁹ *IF* 54.291 f. (1936); see also Sturtevant, *Lg.* 25.343 ff. (1949).

⁴⁰ Which was endorsed by Laroche, *Rec.* 105.

⁴¹ K. Brugmann, *Grundriss*² 2:1.176 ff.; H. Hirt, *IF* 32.283 f. (1914); Benveniste, *Origines de la formation des noms* 52 ff.

The initial elements of the names in question are in a relatively large number of cases theophorous; thus *Ilaliya*-,⁴² *Inar(a)*-,⁴³ *Išput(a)*-,⁴⁴ *Perwa*-,⁴⁵ *Šat(a)*-,⁴⁶ and *Tarawa*-.⁴⁷ The point deserves emphasis here that these deities are by no means Hattic; more about them later. Another segment of the first elements are known in Hittite as adjectives; thus *appezzi(ya)*- 'last', *arawa*- 'free', *kunna*- 'right', *nakki(ya)*- 'important, mighty', *ni/ewa*- 'new, innocent',⁴⁸ *šuppi(ya)*- 'holy'. They may well have served as divine epithets gradually replacing the names of certain gods in other 'Anatolian' languages likewise. The same may be assumed for nouns like *happu*- 'shelter',⁴⁹ *hašta*- 'hero?',⁵⁰ *wetna-/utna*- 'wolf'.⁵¹

It is not superfluous to remark that in a number of cases one observes between stem and suffix a mediating *-n-* or *-t-* (or perhaps both in combination). Note above all the series:

<i>Is-pu-a</i> ⁵²	<i>Išpu-n-uman</i> ⁵³	<i>Išpu-ta-</i>
		<i>Išpu-ta-hšu</i>
<i>Per(u)wa</i>	* <i>Peru-n-</i> ⁵⁴	<i>Peru-ta-hšu</i>
	<i>Šupu-n-aḫšu</i> ⁵⁵	
		<i>Kunu-ta-hšu</i>

There can hardly be any doubt that the god *Išputa*- must be connected with *Išpant*- 'night', likewise venerated as a deity. It has been shown elsewhere⁵⁷

⁴² Cf. E. Laroche, *Rech.* 74; the name is explicable from Hittite.

⁴³ Ibid. 82 f. It is now clear—see *JCS* 4.125 (1950)—that *Inara* is indeed the (or one) reading of ²KAL (LAMA). Moreover the ²KAL is worshipped by the 'singer of Kaniš' (Forrer, *ZDMG* NF 1.197; Bossert, *Königssiegel* 28 and passim). Cf. H. G. Güterbock, Hittite religion 93 (in V. Fern, *Forgotten religions*).

⁴⁴ Cf. *Išpanza* ibid. 75; ²*Iš-pu-da-aš-I-na-ra-aš* 2BoTU 12A II 15. On the name see presently.

⁴⁵ Ibid. 87; see particularly Otten, *JKF* 2.62 ff. (1951); worshipped *kanišumnili* or *luwili*.

⁴⁶ This need not be *Šanda*- (ibid. 88). In view of the doublet *Niwaḫšušar* and *Naḫšušar* the assumption is possible and seems quite likely that *Šat(a)*- is for *Šiwat(a)*-.

⁴⁷ Besides (Hattic) *Taru* (ibid. 32 f.) note also Hittite-Luwian *Tarawaš* (F. Sommer, *AU* 294) and particularly the Luwian proper name *Piyama-Tarawaš* (KBo II 1 II 19).

⁴⁸ Note Hittite *niwalli*-. *Ni-wa* occurs as a proper name on OIP XXVII 1 2 according to Lewy, *Orient.* 21.405 fn. 3 (1952).

⁴⁹ This word will be treated elsewhere. Cf. also *Ha-pu-(w)a-la* (Gol. 13 6; TCL IV 87 30, 55; ICK I 3 6, 13; 19 pass.; 83 24; 93 4; 106 3; 181 22) and *Ha-pu-a-šu* (see presently).

⁵⁰ To be assumed on the basis of the equation *haštaliyatar* = UR.SAG-tar (Otten, KUB XXXIV 105, p. III). Hence, UR.SAG-iš (KUB V 12 rev. 5) must be read *haštališ* and equated with the Kültepe name *Ha-aš-ta-li* (TCL XX 191 3). The same word stem yields—besides *Hištuman* and *Ha/ištaḫšu*—also (this time attested at Boğazköy) *Ha-aš-ta-ia-ar* (f.) (2BoTU 8 III 64), *Hé-eš-ta-ia[-ra]* (f.) (VBoT 33 8), *Hi-iš-ta-i-ia-ra* (f.) (2BoTU 12A II 2). The last quoted names are more closely related to *ha/eštai*- 'strength' (see Ehelolf apud Sommer, *AU* 181). Note also ^{URU}*Ha-aš-du-ri-ia* (HT 2 I 11).

⁵¹ F. Sommer, *Bil.* 75, 77. Cf. furthermore *Wa-at-ni-iš-du-a-an* TCL XX 191 27.

⁵² VAT 13465 (transliterated EL 102) 19. Is Lewy's *Is-bu-a* (with *s* and not *š*) correct?

⁵³ Also ^{BAL}*Iš-pu-un-na-la-aš* KBo II 12 V 11.

⁵⁴ Note in particular Hitt. ^{NA}*ḫé-kur pt-ir-wa* (KBo VI 28 rev. 30; KUB XVI 27 3; VBoT 110 11) and ^{NA}*ḫé-kur Dpt-ir-wa* (KUB XVI 42 rev. 1) where the meaning of *ḫekur* 'mountain, summit' indicates the semantic field in which *pirwa* must fall (cf. F. Sommer, *AU* 318; Otten, *JKF* 2.72 fn. 18).

⁵⁵ *Šupu*- may or may not have anything to do with *šuppi*-.

⁵⁶ *Lg.* 27.473 ff. (1951).

that *išpanti-* contains the zero grade of Skt. *kṣap-* and goes back to IE **ksep-*; the word is enlarged first by an *n*-element (**ksep[e]n-*) and once more by a *t*-element (**ksepont-*). The quoted series represents these various steps. With respect to *per(u)wa* it is a noteworthy coincidence that F. Sommer (apud J. Friedrich, *Heth. Wörterbuch* 168) has connected Hitt. *peruna-* 'mountain top, crag' with Skt. *pārvata* 'mountain' and has at the same time said that *pirwa-* (cf. fn. 54) may also be included in the word family. Here again we find in Indo-European the same formations as are present in the Kültepe names, the best confirmation of our appraisal of the names that could be desired. The *kunūt(a)* of *Kunutaḥšu* may conceivably belong to Hitt. *kunna-* 'right, correct, favorable' and further to IE **ǵenē*, as in Gmc. **koni-* 'noble', the source of **kuninga-* 'king' (cf. Lat. *ingenuus*, *genuīnus*).

5. NAMES IN *-aš(š)u*

The type is represented both in Kültepe and in Boğazköy.

Kültepe: *A-pá-zi-a-šu* (TCL IV 99 5) and *A-pé-zi-ā-šu* (TCL XXI 252 1), *Ha-al-GI-a-šu* (KTP 43 21; Hahn 286 9, 21; TCL IV 122 16) and *Ha-al-ki-a-šu* (Cont. 16 13; Hahn 28a 3; TCL XXI 252 1; ICK I 129 4), *Ha-pé-a-šu* (TMH 28d 3; TCL XX 191 5), *Ha-pu-wa-šu* (BIN VI 236 10) and *Ha-pu-a-šu* (Cont. 16 22; CCT II 30 9; TCL IV 87 22; XIX 14 9; ICK I 19a 3, b 25), *Irnu-a-šu* (Bilgiç), *Kà-nu-a-šu* (ICK I 172 4), *Lá-ak-na-šu* (quotation lost), *Ni-wa-šu* (TCL IV 68 2), *Ša-ar-ni-kà-šu* (Cont. 16 15), *Šawatašu* = *Šaḫutašu* (Bilgiç), *Tamnašu* (Bilgiç), *Ut-ru-wa-šu* (BIN IV 68 15);

Aš-kà-na-šu (TCL XX 191 8), *Šu-pé-maš-na-šu* (TCL XX 191 19);

Hu-ma-tá-šu (TCL IV 99 13; 122 1; XIV 66 8 = XXI 240 4).

Boğazköy: *Ha(-ap)-pu-wa-aš-šu*, *Šar-ma(-a)-aš-šu-un* (acc.), *Dam-na-aš-šu-un* (acc.), *Wa-at-ta-aš-šu-uš*.⁵⁷

Add from Nuzu:⁵⁸ *Ha-pt-a-šu* and *Ha-pt-ia-šu*, *Ha-ta-pt-a-šu*, *Ku-ni-a-šu* (f.), *Na-ḫi-a-šu* and *Na-ḫi-ia-šu*.

The close relationship of the formations in *-aḫšu* and *-aš(š)u* with each other is evident from the pairs *Appezziyaḫšu* and *Appezziyaššu*, *Ḫapiaḫšu* and *Ḫape-aššu*, *Ḫappuwaḫšu* and *Ḫappuwaššu*, *Niwaḫšu* and *Niwaššu*, *Šad/taḫšu* and *Šawataššu*. Again the intermediary *n* and *t* are found. Moreover, as in *-aḫšu* names also, many first elements found here are clearly names of gods or equivalents of such. *Ḫalki(ya)*-⁵⁹ is clearly theophorous and *aška-n*, certainly to be combined with Hitt. *aška-* 'gate',⁶⁰ has religious implications. An epithet is probably concealed behind *kunni-* 'right(?)' and *naḫ(ḫ)i(-ya)-* 'venerable'.⁶¹ With *damna-* the basic element of Hitt. *damna-(š)šaraš*—with and without the determinative god⁶²—must be compared.⁶³

⁵⁷ Occurrences in Laroche's *Recueil*.

⁵⁸ For occurrences see Gelb-Purves-McRae, *Nuzi personal names*. The names are isolated at Nuzu.

⁵⁹ See E. Laroche, *Rech.* 73. *Ḫalkiḫ* is the Hittite form of the Ḫattic grain deity *Kait*.

⁶⁰ Cf. in last place J. Friedrich, *Heth. Wörterbuch* 36 (1952) and the literature quoted there.

⁶¹ Cf. *naḫ-* 'fear, venerate'.

⁶² See A. Goetze, *KlF* 1.228 f.

⁶³ One may ask whether *dam(ma)na-* might be related to *dammara-*, which may stand

Ša-ar-ni-kà-šu may not contain the element *niga*⁶⁴ but rather may be a derivative of Hitt. **šarnikāi-* 'restitution', abstract of *šarni(n)k-* 'restitute, replace', a verb of clear IE habitus (so-called 7th class of Sanskrit grammars). Cf. *Ša-ar-ni-kà* (TCL XX 191 47; ICK I 61 2, 7, 16; cf. TMH 26d 5; VAT 13535 [= EL p. 273 ff.] 4 with the variant *Ša-ar-ni-kà-at*), *Ša-ar-ni-kà-an* (TCL XX 191 9; XIV 66 3 = XXI 240 15) and *Ša-ar-ni-kà-ar* (KTS 1a 29).

The possibility must be considered that *-aš(š)u* and *-aḫšu* are merely dialectic variants of the same suffix.

6. NAMES IN *-iyat*

Kültepe:⁶⁵ *A-ši-at* (EL 167 9); *A-ši-i-et* (Hahn 28 4, 7); *A-ši-e-et* (TCL XX 191 4); *A-ši-it* (ICK I 9 16); *Na-ki-le-et* (TMH 17d a 11; TCL XXI 253 4, 7, 11; ICK I 9 15), *Ni-ki-le-et* (TCL XIV 33 16; XXI 214 1); *Na-ki-li-e-et* (TCL I 242 5; BIN IV 190 16; EL 38 17); *Na-ki-li-a-at* (CCT I 38c 11; ICK I 181 39); *Na-ki-le-e-yi-i-it* (KTP 44 20); *Tá-me-ši-e-et* (KTS 57c 13); *Tá-ar-ḫa-ši-at* (TCL I 242 3); *Wa-li-ši-e-et* (TCL XX 191 16); *Wa-li-ši-it* (Chantre 2 16).

Boğazköy: *Ku-li-e-et* (KBo I 11 rev. 7⁶⁶); *Na-ak-ki-le-et* (2BoTU 12 B II 30); *Pi-im-pi-re-et* (2BoTU 12A III 16).⁶⁷

Hittitologists will at once point out that the many orthographic variants are due to the instability of the sequence *iya*. They find their analog in the various spellings of the 3rd sg. pret. *tiyat*, which, besides the relatively rare *ti-ia-at*, also appears as *ti-e-et*, *ti-i-e-et*, etc.

This allows us to point to the following elements of the Hittite vocabulary as the ones from which the names are derived: *aššiya-* 'love' and the deity *A-aš-ši-ia-za*;⁶⁸ **nakkili/a-*, an enlargement of *nakki-* 'heavy, mighty' in the fashion of *tarḫuili/a-* from **tarḫu-* 'victorious'; the deity *Tamišiyaš* (KUB VI 45 III 2 = 46 III 39); *walla-*, **wallešša(i)-* 'praise'; and perhaps *ku-li-i*, *ku-le-e-i* (CH §46, §47 B) of unknown meaning.⁶⁹

With the thematic vowel we have furthermore, from Kültepe, *Ša-li-a-ta* (f.) (BIN IV 209 6),⁷⁰ from Boğazköy, the name of the goddess *Kulitta(š)*, hierodule of Sauškaš,⁷¹ and, from both Kültepe and Boğazköy, the important *Anitta(š)*

for **damnara-*. The *damnara-* women are well known (KUB V 6 II 16, 36, 46; XVI 16 obv. 13, 18, 23, 26, rev. 19; XVIII 37) and once (KUB V 6 II 16) paralleled by *d.-man*. The word appears twice with the Luwian plural suffix: ^{SAL}*dam-ma-ra-a-an-zi* nom. (KUB XVIII 37) and ^{SAL.MES}*dam-ma-ra-an-za* acc. (KUB V 6 II 46).

⁶⁴ Its existence is assured because of *Ni-ga* (TCL XX 158 17), *Ni-ga-at* (TCL XX 158 5) and *Ni-ga-an* (Bilgiç); compare Hittite ^D*Ni-in-ga-aš* (see Goetze, *Madduwattaš* 118 and KIF 1.405).

⁶⁵ I have omitted *Ši-a-at* (KTS 47b 1, 3) since it probably belongs to the River *Šiyanta* of the Hittite texts.

⁶⁶ The variant *Ku-li-a-et* (ibid. 26) is faulty (thus also Güterbock, *ZA* NF 10.127 f.), but perhaps influenced by an older **Ku-li-a-at*.

⁶⁷ Cf. *Pi-im-pi-ra-aš* (2BoTU 9 IV 11; etc.) and *Pi-pi-ri-ri-ia-an* acc. KUB XXIII 87 16.

⁶⁸ I.e. *Aššiyaat+s*; cf. H. Th. Bossert, *Asia* 119. According to the same author, *Königs-siegel* 70, E. Bilgiç rendered the name by 'Liebling'. The name of the deity is however in most cases spelled without the *n* of the participle, and the *n* may not be genuine to the form.

⁶⁹ Cf. A. Götz, *Neue Bruchstücke* 58; F. Sommer, *Bil.* 126 fn. 5.

⁷⁰ With the variant *Ša-lu-wa-an-ta* for which *Ša-lu-a-ta* (f.) (ICK I 35 b 1, with the variant *Ša-lu-a-ta-at* a 10) and *Ša-lu-wa-ta* (f. ?) (TCL IV 99 3) must be compared.

⁷¹ *Kulittaš* is generally considered as Hurrian of origin. However, her name, like that of

(name of a king, the author of the oldest inscription in Hittite). One can hardly avoid the conclusion that they belong to *šalliya*- 'become great' and *an(n)iya*- 'perform', the second of which is also the source of the noun *aniyatta/i* '(ritual) vestments'.⁷²

Attention should also be called to *Ku-ul-ša-ta-aš* gen. (ICK I 16 a 2⁷³).

The six groups here discussed are connected with one another by the theophorous elements that appear in them. They will afford the best chance of finding out what we should call the people from whom and the language from which they are taken. The gods that occur in them are *Aššiyat*-, (*Aška*-), *Halki(ya)*-, *Ilaliya*-, *Inar(a)*-, *Išput(a)*-, *G/Kulša*-, *Perwa*-, *Šata*- (< *Šawat[a]*-, *Šiwa[a]*-?),⁷⁴ *Tarawa*-. Their representation in the various groups is shown in the following list:

<i>Aššiyat</i> -				<i>Aššiyat</i>
<i>Aška</i> -				<i>Aškanašu</i>
<i>Halki(ya)</i> -				<i>Halkiašu</i>
<i>Ilaliya</i> -	<i>Ilalelka</i>	<i>Ilaliahšu</i>		
<i>Inar(a)</i> -		<i>Inarahšu</i>		
<i>Išput(a)</i> -		<i>Išputahšu</i>		
<i>G/Kulša</i> -				<i>Kulšataš</i>
<i>Perwa</i> -		<i>Perwahšu</i>		
<i>Šata</i> -		<i>Šatahšu</i>		
<i>Tarawa</i> -		<i>Taruhšu</i>		
(<i>Niwa</i> -)	<i>Niwalka</i>	<i>*Niwahšu</i>	<i>Niwašu</i>	

It has been shown in an earlier article in this journal (*Lg.* 29.263-77) that these are the gods who in Boğazköy texts are mentioned as the 'gods of Kaniš'. Their names, then, many of them demonstrably of Indo-European origin, are best claimed for 'Kanishite', the language used by the 'Singer of Kaniš' and recognized as a separate language in the Hittite texts.⁷⁵

her constant companion *Ninattaš*, betrays no Hurrian characteristics. Neither of them—nor, for that matter, *Sauška*, their mistress—ever occurs in Nuzu. Just as there are Hurrian elements in the Hittite pantheon, we will have to acknowledge the presence of Hittite (or 'Anatolian') elements in the Hurrian pantheon. The existence of a Hurrian noun *ku-u/i-li* can of course be purely accidental (KUB XII 11 IV 27 f.; XXVII 1 II 67); *kulaḫena* (see Laroche, *JCS* 2.121) is probably derived from it. That a homonymous Luwian stem exists is proved by *ku-li-wi* KBo IV 14 IV 34 and *ku-la-ni-wi* KUB XXI 20 3.

⁷² Goetze, *JCS* 1.176 f. (1947).

⁷³ Previously quoted by Hrozný, *ZA* NF 4.183. The name has the variant *Ku-ul-ša-an*. Both belong with Hrozný to Hitt. *g/kulš*- 'impress, inscribe'.

⁷⁴ Here the well-attested Cappadocian name *Ši-wa-aš-me-i* (KTS 52 a 11; BIN IV 31 2; 203 17; 209 2; cf. H. Th. Bossert, *Asia* 81, 89) may also be mentioned. One cannot help but compare the other Cappadocian names *Ši-wa-na-lá* (OIP XXVII 7 10) and *Ši-nu-na-la* (KTS 58 a 11); cf. *Ḫa-pu-a-la*, *Ta-ar-ḫu-a-la* on the one hand, and Hittite *šiu*-, *šivan*- 'god' on the other. One is reminded particularly of the name given to the god of Neša in the Anittaš text (2BoTU 7, 2BoTU 30 and duplicates), namely *Ši-u-šum-mi-iš* (acc.), *Ši-u-na-šum-mi-iš* (gen.), to be analyzed either as *šiuš-miš*, *šiu-naš-miš* (?), literally 'my god' or as *šiuš-šmiš*, *šiu-naš-šmiš* 'their/your god'. The occurrence of this god as a proper name in the Kültepe tablets—just like *Inaraš*, *Pirwaš* and others—makes it in my opinion impossible to regard the name of the god of Neša in the Anittaš text as a later addition, as has been asserted (Güterbock, *ZA* NF 10.139 ff.; Landsberger, *Arch. Or.* 18:1/2.341 fn. 66).

⁷⁵ Note the adverb *kanišumnili* 'in the fashion of a Kaneshite' Bo 6222 (*JKF* 2.67).

THE SEGMENTAL PHONEMES OF SANSKRITIZED TAMIL

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1. This is a phonemic analysis of the Sanskritized literary Tamil spoken in Madras by those whose vernacular is Tamil and whose fluent knowledge of Sanskrit is due to their heritage and upbringing.¹ This variety of the language is distinct, on the one hand, from the several geographically determined dialects and, on the other, from Tanittamil, 'pure Tamil', which, free (at least in intention) of all Indo-Aryan influences, has records possibly two thousand years old and is in use today by certain people in the Tamil area. Sanskritized Tamil is a cultivated, literary language subject to strict canons of grammar and style, but it is a living speech and not an artificial medium used only by scholars.

The most difficult problem in the phonemic analysis of Sanskritized Tamil is the disposition of the sounds occurring in words borrowed from Sanskrit. These words are assimilated to the morphological structure of Tamil, but, despite the rules of the classical grammarians for the conversion of Sanskrit to Tamil,² the sounds of the words borrowed are not always changed. Some words are completely Tamilized, e.g. Skt. *ākāṣam* 'sky' > Tam. *ākāyam* [o'ho:jam];³ in others a Sanskrit phoneme is preserved intact, e.g. Tam. *janāṅkaḷ* 'people' < Skt. *√jan* with [dʒ] in initial position, where it is not used in any Tamil word of Dravidian origin; others exist in two forms, one half-Sanskrit, half-Tamil, the other totally Tamilized, e.g. Skt. *pustakam* 'book' > Tam. *pustakam* [p'ust'ʌḥam] or *puttaham* [p'ut'ʌḥam]; and still others, which contain only phonemes readily convertible to their Tamil equivalents, are indistinguishable from their Sanskrit originals, e.g. Tam. *palam* 'strength' [baɭam] < Skt. *balam*.

Merely to excise all evident Sanskrit sounds is no solution at all to this problem, since the peculiar quality of Sanskritized Tamil consists precisely in the common use of words containing these foreign elements. There is, moreover, a subordinate problem—that of Caldwell's 'law' of the convertibility of surds and sonants⁴—

¹ The pronunciation here recorded is that of Sri E. K. Natesa Sarma, formerly of Loyola College, Madras.

² Thus Nannūḷ 147. Although these rules are for the transliteration of the Devanāgarī script (i.e. for the spelling of Sanskrit words in Tamil), their ultimate effect is to dictate the pronunciation. The rules are explicit and detailed, resulting in such conversions as of Skt. *ṛṣabha* 'bull' to Tam. *iḍapam* and of Skt. *lakṣmi* 'fortune' to Tam. *ilakkumi*.

³ The following symbols are used in this article. The Devanāgarī script is transliterated in the accepted manner. The transliteration of the Tamil alphabet follows the usage of the Tamil lexicon of the University of Madras, and the symbols chosen for phonemic writing likewise are those of that romanization. The phonetic symbols are those of the IPA (rev. 1951) with the following exceptions and additions: a subscript dot marks retroflexion; underscoring marks alveolopalatal articulation; the letter r with superscript dot and breve denotes alveolopalatal flaps, tense and lax respectively; [ɲ] is a palatalized nasal; [ɸ] is a lax glottal spirant. — A semicolon separating two English glosses of one Tamil word indicates that, so far as can be determined, the meanings are of homonyms, not of the same word.

⁴ See §5 and fn. 22.

which can only be considered properly by the admission of these sounds. In this analysis, therefore, all the sounds recorded have been treated alike without regard either for their origin or for their degree of variance from it, excepting that an attempt has been made to observe the effect of sounds of indubitably Sanskrit origin upon the distributional pattern of the language.

2. In the idiolect examined, 32 consonantal and 15 vocalic phones have been recorded. These may be combined and classified into 22 consonantal and 5 unitary vocalic phonemes.⁵

2.1. VOWELS. The 5 unitary vowels /i e a o u/, either single or double, serve as syllabic nuclei. Single vowels are lax; double vowels are tense.

/i, ii, e/: *niḷam* [niḷam] 'ground' /niḷam/; *niḷam* [niḷam] 'the color blue' /niḷam/; *iṅkē* [iŋge] 'here' /imgee/ (§2.25); *eṅkē* [eŋge] 'where' /emgee/. In utterance-initial and after postvocalic word juncture when no other intervocalic glide is introduced, the initial component of /i, ii, e/ is a palatal frictionless continuant.

/e, ee, a, aa/: *peṇ* [p'eŋ] 'girl' /peṇ/; *pēṇ* [p'ejŋ] 'desire, lovable quality' /peen/; *kaḷ* [k'ʌl] 'stone' /kaḷ/; *kāl* [k'ɒ'l] 'leg' /kaal/. The final component of /ee/ is an alveolopalatal frictionless continuant.

Before /l/, /a/ is [ə], a lower mid central vowel with rounded oral cavity; before /y/ it is [ə], approaching a very short [e]; elsewhere it is [ʌ]. /aa/ before /y/ approaches [ɔ:]; after word juncture it is [a:] with no perceptible rounding; elsewhere it is [ɒ:], a partly rounded, very low back vowel.

/o, oo, u, uu/: *koḷu* [k'ɔḍi] 'give'⁶ /koḍu/; *kōḷu* [k'owḍi] 'summit' /kooḍu/; *uḷi* [wu.ɹi] 'place' /uḷi/; *ūḷi* [wuw.ɹi] 'eon' /uuli/. /o/ is a slightly rounded [ɔ]. The final component of /oo/ is a bilabial frictionless continuant.

In utterance-initial and after postvocalic word juncture when no other intervocalic glide is introduced, the initial component of /u/ and /uu/ is a bilabial frictionless continuant. Before word juncture /u/ is [ɪ], a high central unrounded vowel.

2.2. CONSONANTS. There are 22 consonantal phonemes: /p b t d ṭ ḍ k g m n ŋ l ʃ v c r x h w y r l/.⁷ In stop consonants paired by similarity of articulatory position, tenseness is the significant feature in opposition to laxness, rather than voicelessness in opposition to voice. Tense stops, unless affricated, are aspirated. No tense stop follows a nasal.

2.21. /p, b, m/: *paḷam* [p'ʌḷam] 'unit of weight = 3 tolas' /paḷam/; *paḷam* [baḷam] 'strength' /baḷam/;⁸ *maḷam* [maḷam] 'filth, feces' /maḷam/.

⁵ Intonation and stress have been omitted from this study. The following observations have been made of stress. In two-syllable words stress is equally distributed. In polysyllables, a three-syllable law exists for short open syllables: the accent is recessive, but only as far as the antepenult; if there is a second accent, that also falls no farther back than three syllables before the antepenult (e.g. *'kuṛriya'ḷukaram* 'the short u sound'). Long vowels in pretonic position are shortened (e.g. *āru* 'six' + *pattu* 'ten' yields *aṛupatu* 'sixty').

⁶ Verbs glossed in this form are cited in the singular imperative, which is the normal way of naming a verb in Tamil.

⁷ Eight of these contrast in an identical environment: *pāy* 'mat', *tāy* 'mother' (sic), *kāy* 'unripe fruit', *māy* 'conceal', *nāy* 'dog', *vāy* 'mouth', *cāy* 'lean', *yāy* 'mother' (sic.)

⁸ /b/ occurs after word juncture only in words borrowed from Sanskrit.

2.22. /t, d, n/: *tāy* [t'ə:j] 'mother' /taay/; *nāy* [nə:j] 'dog' /naay/. Although occurring after word juncture (e.g. *tēcam* [dejɕam] 'country' /deecam/),⁹ /d/ cannot be shown to contrast with /t/ in that environment. After syllable juncture, /t/, /d/, and the alveolopalatal allophone [ɲ] of /n/ (§2.25) are in contrast with one another: *katti* [k'at'tɪ] 'knife' /kati/; *kati* [k'adɪ] 'rise' /kadi/; *kaṇi* [k'ʌɳɪ] 'fruit' /kani/.

Tense dental stops are strictly dental in point of articulation. After postvocalic syllable juncture, a lax dental stop approaches the interdental articulatory position.

The tense alveolopalatal [t] allophone of /t/ occurs after postvocalic syllable juncture when itself followed by the tense flap /ɾ/; and before syllable juncture which is followed by the [ɕ] allophone of /c/ (§2.29): *nerru* [nej-tɾɪ] 'yesterday' /neetɾu/; *kaccu* [k'ʌt-tɕɪ] 'girdle' /katcu/.

The lax alveolopalatal [d] allophone of /d/ occurs preceded by the alveolopalatal [ɲ] allophone of /n/ (§2.25) plus syllable juncture, and followed by the lax alveolopalatal flap allophone of /ɾ/ (§2.26):¹⁰ *mūṇru* [muwɲ-dɾɪ] 'three' /muundɾu/; *konṛān* [k'ʌɲ-dɾɔɲ] 'he killed' /kondraan/.

2.23. /t, d, n/ do not occur after word juncture. They can be shown to contrast with one another only after postvocalic syllable juncture: *ōṭṭu* [wow-tɾɪ] 'drive' /ootu/; *ōṭu* [wow-dɪ] 'run' /oodu/; *paṭam* [p'ʌ-dam] 'picture' /paḍam/; *paṇam* [p'ʌ-ɳam] 'money' /paṇam/.

The retroflex and the dental stops are likewise in contrast with one another in both their tense and their lax varieties: *paṭṭu* [p'ʌ-tɾɪ] 'ten' /patu/; *paṭṭu* [p'ʌ-tɾɪ] 'silk' /paṭu/; *maṭu* [mʌ-dɪ] 'honey' /madu/; *maṭu* [mʌ-dɪ] 'deep spot in a tank, river, or lake' /maḍu/.

2.24. /k, g, h/: The tense and lax velar stops contrast after open juncture; medially, as can be observed in the automatic alternants of the plural morpheme /ka/,¹¹ the tense velar stop presents a contrast with the lax glottal spirant /h/. Since the glottal spirant in intervocalic position is not strongly tense, it is classified as lax, written [h]. This classification is consistent with the arrangement of the stop consonants and the tense alveolopalatal fricative /c/. Examples: *kati* [k'adɪ] 'rise' /kadi/; *kati* [gʌdɪ] 'tongue' /gadi/;¹² *nakku* [nɔk'ɪ] 'tongue' /naaku/; *nāku* [nɔ'hɪ] 'youthfulness' /naahu/.

2.25. /m, n, ŋ/: Each of these nasals has been shown to be in contrast with its homorganic stop (§2.21, §2.22, §2.23).

The dental allophone [n] of the dental nasal phoneme occurs (1) after word

⁹ /d/ occurs after word juncture only in words borrowed from Sanskrit.

¹⁰ The name of the city of Madras, [mataɾās], a word of non-Dravidian origin, in rapid speech appears to have a lax dental stop before the lax flap; but in careful pronunciation the medial syllable is retained, and the very nearly interdental articulation of the [d] even in rapid speech indicates that the following vowel never completely vanishes.

¹¹ There are three alternants, the distribution of which need not here be mentioned. It should be noted, however, that the phonemic problem of [g] comes to the fore again in the analysis of these morpheme variants, since if /g/ were not established as a phoneme /ga/ could be removed from the list of plural alternants. In the plural 'trees', then, [g] as the lax allophone of /k/ would presuppose an immediately preceding lax phoneme, while the velarity of the nasal would presuppose a following velar (§2.25).

¹² /g/ occurs after word juncture only in words borrowed from Sanskrit.

juncture and (2) before a dental stop; elsewhere (3) the alveolopalatal alternant [ɲ] occurs: (1) *nāṇ* [nɔːɲ] 'I' /naən/; (2) *vantāṇ* [vɔːɲɔːn] 'he came' /vandaən/; (3) *kaṇi* [k'ɔːɲi] 'fruit' /kani/, *paiyaṇkal* [p'əjjɛŋgəl] 'boys' /payyengal/.

/n/ is in contrast with /m/ and with the [ɲ] allophone of /n/ after postvocalic syllable juncture: *aṇai* [ɔːɲəj] 'bank of river or tank' /aṇay/; *amai* [ɔːməj] 'bamboo' /amay/; *kaṇi* [k'ɔːɲi] 'astrologer' /kani/; *kani* [k'ɔːɲi] 'fruit' /kani/.

The palatalized allophone [ɲ] of the dental nasal phoneme shares a preceding postjunctural environment with both the bilabial and the dental nasal but is never in contrast with either in an identical following environment. In some words the dental nasal alternates freely with the palatalized nasal, e.g. [ɲɔːjiɾɪ] or [nɔːjiɾɪ] 'sun'. Since the nasality of the palatalized nasal is certain, the available evidence shows only that [ɲ] is in complementary distribution with both the other nasals which appear after word juncture, and that it is a free variant of [n]. By applying the criterion of acuteness opposed to gravity, however, it may more certainly be considered a member of the dental phoneme occurring before a palatal frictionless continuant. *ṇāyirū* 'sun' is therefore /nyaayirū/.¹³

The velar nasal [ŋ] is in contrast with the alveolopalatal allophone [ɲ] of the dental nasal phoneme in a similar environment, e.g. *eṇkē* [jɛŋgɛj] 'where' : *eṇ ka* [jɛŋgɔː] 'say so'; but it is in complementary distribution with the bilabial nasal. [m] never appears before an immediately following /g/, whereas an immediately following /g/ is presupposed by the velar nasal;¹⁴ e.g. /maram/ 'tree' + the plural morpheme /ka/ yields [maɾaŋgəl]. Both the other nasal phonemes occur before /g/; e.g. [p'ɛŋ] 'woman' : [p'ɛŋgəl] 'women', [p'əjjɛŋ] 'boy' : [p'əjjɛŋgəl] 'boys'. [ŋ] is therefore classified as a member of the phoneme /m/; *marāṇkal* [maɾaŋgəl] 'trees' is /maramgal/.

2.26. /ɾ, r/ can be shown to be in contrast only after postvocalic syllable juncture: *kaɾi* [k'ɔːɾi] 'curry' /kaɾi/;¹⁵ *kari* [k'ɔːɾi] 'charcoal; witness' /kari/.

A distinctive difference between /ɾ/ and /r/ can be shown to exist in many pairs of words; nevertheless the phonetic difference between the flap and the continuant is the one most easily annulled in Tamil, the tense flap replacing the lax continuant and thus obliterating the phonemic distinction. In view of the ease of this annulment, two further items of evidence are adduced for the retention of the difference as a significant feature of the language. (1) Following [ɾ] all consonants are tense, e.g. *nārkāli* [nɔːɾk'ɔːli] 'chair'; following [r] they may be either tense or lax, e.g. *pārkkiṛēṇ* [p'ɔːɾk'ɾiɛjɲ] 'I see', compared with *avarkaḷ* [ɔːvɔːɾhəl] 'they'. (2) Before a tense velar stop, either /l/ or the [ɲ] allophone of /n/ is implied in the alveolopalatal flap; e.g. *kaɾkiṛēṇ* [k'ɔːɾk'ɾiɛjɲ] 'I learn', which may be decomposed into √/kaɾ/ + /kiɾu/ + /een/; cf. the related *kalai* [k'ɔːləj] and *kalvi* [k'ɔːlvi], both 'learning'.¹⁶ There is no such implication in the alveolopalatal retroflex continuant [ɾ̥].

It is always /ɾ/, moreover, which asserts itself over the /r/; i.e. the flap some-

¹³ The nasal appearing before [dʒ] is the alveolopalatal allophone of the dental phoneme /n/ (§2.25, §2.29):

¹⁴ The [ɲaṇam] of the grammarians (e.g. Nannūḷ 106) cannot exist after word juncture, and all derivatives have the requisite /g/.

¹⁵ The word is of Dravidian origin.

¹⁶ A less transparent example is the word for 'chair', cited under (1) just above. This = *nāṛ* = *nālu* 'four' + *kāl* 'foot' + the nominal suffix *i*.

times replaces the continuant, but the continuant never replaces the flap.¹⁷ The phonemes are not interchangeable.

The lax alveolopalatal fricative [ʃ] always presupposes a preceding [d] allophone of /d/ (§2.22) and has therefore no independent existence. It is an allophone of the tense alveolopalatal flap /ɽ/: *mūnru* [muwɽɽɽ] 'three' is thus /muundru/.

2.27. /l, ɭ, ɻ/: /l/ is an alveolodental frictionless continuant with lateral release; /ɭ/ is a palatal retroflex frictionless continuant with lateral release; /ɻ/ is a velar retroflex frictionless continuant articulated with the tongue retroflexed almost to the velum, then rolled forward to the alveolar ridge without lateral positioning.¹⁸

/l, ɭ, ɻ/ do not occur after word juncture. They contrast with one another after postvocalic syllable juncture; /r, ɽ, d, ɽ/ are likewise in contrast with them in the same environment: *kali* [k'ʌɽɽ] 'noise' /kali/; *kaɭi* [k'ʌɭɽ] 'joy' /kaɭi/; *kaɻi* [k'ʌɻɽ] 'shallow backwaters' /kaɻi/; *kari* [k'ʌɽɽ] 'charcoal' /kari/; *kaɽi* [k'ʌɽɽ] 'curry' /kaɽi/; *kati* [k'ʌɽɽ] 'rise' /kadi/; *kaɽi* [k'ʌɽɽ] 'abandon' /kaɽi/.

2.28. /w, v, y/: /w/ is a lax bilabial continuant with slight friction at the lips; /v/ is a labiodental continuant with slight friction; /y/ is a tense palatal frictionless continuant: *vāy* [vəj] 'mouth' /vaay/;¹⁹ *yāy* [jəj] 'mother' /yaay/; *vīram* [vijram] 'valor' /viiram/; *uyaram* [wijram] 'height' /wiiram/.

Evidence for the /w/ phoneme is meager. A [w]-like sound occurs as an initial on-glide before /o/ and /u/, functionally equivalent to the /y/ used as a glide before /i/ and /e/ (§2.1). /v/ is never used as an initial on-glide. The use of these glides causes no change in the content of an utterance, but the presence among them of [w] as a recognizable sound lends some support to the interpretation of *uyaram* as /wiiram/ and thus to the establishment of /w/ as a phoneme.

2.29. /c/ has four allophones: [s, ç, tʃ, dʒ]. Of these, [tʃ] and [dʒ] are in some environments determined, and in others free variants; [s] and [ç] are free variants. In a word of Sanskrit origin which contains one of these sounds, a citation pronunciation will preserve the articulation prescribed for Sanskrit; but in the ordinary stream of speech any one of them may appear as a variant of any other. This mixing has been facilitated by the representation of them all by one symbol in the Tamil alphabet. Thus *tēcam* 'country' < Skt. *deśa*, when carefully pronounced, is [dejçam], but [dejsam] is also heard; *cakotaraṇ* 'brother' < Skt. *sahodara* 'co-uterine' is pronounced either with the Sanskrit dental sibilant *ṣ* [saḥowdaɽaṇ] or with initial [ç] or [tʃ] instead. Even the proper name *Cāstri* is rendered both as [çə'stri] and as [tʃə'stri].²⁰

The Sanskrit dental sibilant [s], having no exact phonemic equivalent in Tamil, has associated itself to the /c/ phoneme. When pronounced in Tamil, the dental

¹⁷ If a generalization were to be made from this one instance, it would have to be that the intensive /ɽ/ is obliterating the extensive /r/. The change from intensive to extensive is contrary to what has generally been observed in morphology and syntax.

¹⁸ The lack of lateral positioning should be emphasized. This /ɻ/ is the final phoneme in the name Tamil, of which the English spelling, the romanization *tamil*, and the phonemic writing /tamil/ here adopted, if mistakenly read as phonetic, all give a false impression.

¹⁹ Cf. fn. 7.

²⁰ The name (of Sanskrit origin) also contains the non-Tamil cluster [str].

sibilant may thus represent a Sanskrit dental [s], or it may be a substitute for a Sanskrit palatal [ç]; and the palatal [ç] when pronounced in Tamil may likewise correspond to its Sanskrit equivalent or be a substitute for a Sanskrit [s], e.g. *cinkam* [çiŋgam] 'lion' < Skt. *siṃha*. Where a Sanskrit [s] occurs before a dental stop, no other allophone of /c/ is used, but the [s] may not be pronounced at all; e.g. [p'ustaḥam] or [p'ut'ḥam] 'book'. Sometimes an uncertain compromise between Sanskrit and Tamil occurs; e.g. [stirij] 'woman' < Skt. *strī*, with the alternant [jistirij], approaching [jɪstirij].

The affricated dental stop [tʃ] is an allophone of /c/ after postvocalic word juncture and after syllable juncture preceded by /t/, by /r/, or by the [t] allophone of /t/. In the former environment the stop component of the affricate is double, and is decomposable into a tense alveolar stop [t] (§2.22) which closes the last syllable of the preceding word, and a tense dental stop [t] which opens the first syllable of the following word. Example after word juncture: *anta* [ʌnda] 'that' + *ciriya* [çiriʎa] 'small' yields [ʌndaʈtʃiriʎa]; examples after syllable juncture: *kaṭci* [k'ʌt-tʃi] 'jungle' /kaṭci/, *paṭci* [p'ʌt-tʃi] 'bird' /paṭci/ (cf. *paci* [p'ʌçi] 'hunger' /paci/), *kārcari* [k'ʌr-tʃari] 'anklet' /kaarçari/, *kaccu* [k'ʌt-tʃi] 'girdle' /katcu/. This conditioned variant [tʃ] has become free, as already indicated, occurring in utterance-initial as well as elsewhere (cf. *Cāstri* above).

A lax affricated [dʒ] allophone of /c/ occurs after syllable juncture preceded by the alveolopalatal [ɲ] allophone of /n/ (§2.25): *mañcal* [maɲdʒʌ] 'saffron' /mancal/. This conditioned variant has, like [tʃ], become free, occurring in utterance-initial as well as elsewhere; e.g. *jannal* [dʒaɲɲʌ] or *cannal* [çaɲɲʌ] 'window' < Ptg. *janela*, *jepam* [dʒabam] or [çabam] 'prayer' < Skt. *japa*. In certain words of Sanskrit origin, such as *jepam*, [dʒ] reproduces a Sanskrit sound, but in all such instances a variant exists; e.g. *irājan* [jɪrɔ'dʒʌ] or *aracan* [ʌraçʌɲ] 'king', both < Skt. *rāja*. There is no evidence for the phonemic independence of [dʒ] in Tamil.

2.210. /x/: The contrast between *kaṭcu* [k'ʌxçi] 'measure of weight = ¼ palam' /kaxcu/ and *kaccu* [k'ʌtʃi] 'girdle' /katcu/ isolates the tense velar fricative /x/. The phoneme is of rare occurrence.

3. DOUBLE CONSONANTS. The frictionless continuants and nasals /l, ɭ, v, y, m, n, ɳ/ occur in single and double forms which contrast with one another:²¹ *pōlam* [p'owlam] 'beauty, fairness' /poolam/ : *pōllam* [p'owllam] 'sewing, joining together' /poollam/; *veḷi* [veɭi] 'air, sky, open plain' /veɭi/ : *velli* [vellɪ] 'Friday; silver, whiteness' /vellɪ/; *pavam* [p'ʌvʌm] 'sin' /pavam/ : *pavvam* [p'ʌvvʌm] 'ocean' /pavvam/; *kuyam* [k'wiʎam] 'sickle' /kwiyam/ : *kuyyam* [k'wiɲɲam] 'deception, hypocrisy' /kwiyyam/; *kamam* [k'ʌmam] 'fullness, entirety' /kamam/ : *kammam* [k'ʌmmʌm] 'blacksmithing' /kammam/; *paṇi* [p'ʌɳi] 'fear, dread' /paṇi/ : *paṇṇi* [p'ʌɳɳi] 'wife' /panni/; *kaṇi* [k'ʌɳi] 'name of a caste; count (verb)' /kaṇi/ : *kaṇṇi* [k'ʌɳɳi] 'bud, wreath of flowers, noose' /kaṇṇi/.

4. THE SYLLABLE. Any vowel may constitute a syllable by itself, and every syllable contains a vowel. Vowels and consonants are distributed in the following relations to word and syllable juncture:

/i, e, a, o, u/ occur before and after word and syllable juncture.

²¹ No examples have been found of double /l/ and double /w/.

- /m, n, r, w, v, y/ occur before and after word and syllable juncture.
 /p, b, t, d, k, g, c/ occur after word and syllable juncture.
 /t, d, n, r, l, ɭ/ occur before word and syllable juncture and after postvocalic syllable juncture.
 /x/ occurs before syllable juncture.
 /h/ occurs after postvocalic syllable juncture.
 /rn, yn, yt/ occur before preconsonantal syllable juncture
 /kw, ny/ occur after word juncture.
 /tr, dr/ occur after syllable juncture.

The syllabic division thus appears before any single consonant, before the clusters /tr, dr, kw, ny/, between /rn, yn, yt/ and a following consonant, and between any other contiguous consonants.

Syllables are of the following types:

V	/u-li/ 'place'	CVVC	/taay/ 'mother'
VV	/uu-li/ 'eon'	CCV	/nee-tru/ 'yesterday'
CV	/pa-lam/ 'unit of weight'	CCVV	/nyaa-yi-ru/ 'sun'
CVV	/koo-du/ 'summit'	CCVVC	/kon-draan/ 'he killed'
CVC	/pa-lam/ 'unit of weight'	CVVCC	/uṭ-kaarn-du/ 'seated'

5. THE EFFECT OF SANSKRIT. It seems clear that the lax stops /b, d, g/ owe their phonemic status to the widespread use of Sanskrit loanwords (fnn. 8, 9, 12). Caldwell's law,²² so called, of the convertibility of surds and sonants appears to be based upon a spelling pronunciation, or a wholly Tamilized pronunciation, of these borrowings. Such a pronunciation cannot always be assumed and in some instances cannot be found at all. Thus Skt. *danta* [danta] 'tooth' becomes Tamil *tandam*,²³ which is pronounced either [danda] or [t'anda], but more normally and more correctly in the former manner, with the memory of its Sanskrit origin provoking the initial [d] and the normal requirement of Tamil distribution requiring the medial [nd]; the form [t'anda] is a spelling pronunciation. Similarly, the common word *putan* [buda] 'Wednesday' is always pronounced with initial [b].

In the case of /c/ and its allophones, the influence of Sanskrit is no less sure, as the number of Sanskrit words in Tamil pronounced with one of these sounds shows, but the precise definition of the influence is far less certain. It is possible that the representation of several Sanskrit sounds by one letter (transliterated *c*) in the Tamil alphabet has resulted in hesitation between the Sanskrit pronunci-

²² R. Caldwell, *Comparative grammar of the Dravidian or South Indian family of languages* 138. The law is, in its author's phrasing, completely untenable, since it embodies a confusion of sound and symbol and of historical and synchronic analysis. It is possible that the graphic representation of Tamil sounds at some time in the past showed a complementary distribution of surds and sonants at some preceding period—and that, in modern terms, is what Caldwell is saying. ('A sonant cannot commence a word, neither is a surd admissible in the middle, except when doubled.') But that the law operated diachronically, holding the language in a perpetual complementary distribution of these sounds, is not credible. In modern Sanskritized Tamil, surd and sonant (i.e. tense and lax) stops are in contrast (§§2.21-24).

²³ The example is Caldwell's. The word is not in common use in modern Tamil, being almost completely restricted to reference, in elegant language, to the tusk of an elephant.

ation and a spelling pronunciation, just as in the variation between [d̪andam] and [tʰandam], except that here the number of choices is greater. Whether the transformation of the sandhi variant [tʃ] and the conditioned allophone [dʒ] into free variants is due wholly to Sanskrit influence it is impossible to say, but it may be that the apparently random extension of [tʃ] owes its impetus to uncertainty and its erratic development to the force of analogy. It seems likely that in the phoneme /c/ a phonemic split is now in process and that this split has been caused by the influence of Sanskritized pronunciation.

CONSONANT CLUSTERS IN TAI

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1.1. Very few of the languages of the Tai family still preserve consonant clusters of the type *pl-*, *kl-*, *pr-*, *kr-*, etc. The two languages generally known to preserve such clusters, at least in part, are Siamese and Ahom, the latter an extinct language of Assam.¹ The dialects of Wu-ming and Lung-an, both in central Kwangsi province,² are the other dialects I know which preserve them to a certain extent. The widely scattered distribution of these languages shows that the preservation of the clusters indicates no specially close linguistic relationship among them. Three other languages—Lao,³ a language closely related to Siamese; Shan,⁴ closely related to Ahom; and Ddoi of Kweichow province,⁵ with which Wu-ming shares many common characteristics—have all simplified such clusters. It is safe to assume that the simplification is in most cases an individual dialect development, and that the clusters persisted in each of the various dialect groups before their simplification took place.

1.2. Since we are limited in this study to clusters of consonant plus liquid, the problem is to determine the various existing combinations. This task is complicated by two factors. (1) The first consonant is often modified by the following liquid, so that the modern forms often show altogether different articulations; cf. Siam. *plaa* 'fish' but T'ien-chow *čaa*,⁶ Siam. *took* 'strips of bamboo' but Lung-chow *phjook*.⁷ (2) There are two liquids, *l* and *r*, in Proto-Tai (abbr. PT), which are often redistributed in the dialects according to special conditions. Thus, Ahom has *l* after unaspirated initial consonants and *r* after aspirated initial consonants; Wu-ming has *l* in clusters, but *r* alone. Siamese is the only language where various contrasts are possible (*pl-*, *pr-*, *phl-*, *phr-*, etc.); but even there, as the comparative evidence shows, many clusters have been simplified, and some combinations (*pr-*, *tr-*, etc.) are hardly found to have correspondences in any other language except Lao. These forms may be either proven or suspected

¹ See Rai Sahib Golap Chandra Borua, *Ahom-Assamese-English dictionary* (Calcutta, 1920); G. A. Grierson, Notes on Ahom, *ZDMG* 56.1-59 (1920).

² Material on the Wu-ming dialect was collected by the author in 1935, and a study of this dialect with texts, songs, and a glossary was sent in 1941 to Hongkong to be published. The manuscript was apparently lost during the Japanese occupation of Hongkong. A brief study of Wu-ming phonology appears in *Bulletin of the Institute of History and Philology (Academia Sinica)* 12.293-303 (1947). Some material on the Lung-an dialect was also obtained by the author in 1935, but this is not sufficient for a systematic study.

³ See T. Guignard, *Dictionnaire laotien-français* (Hongkong, 1912).

⁴ See J. N. Cushing, *A Shan and English dictionary* (Rangoon, 1914).

⁵ See J. Esquirol and G. Williate, *Essai de dictionnaire ddoi-français* (Hongkong, 1908).

⁶ T'ien-chow is a district in western Kwangsi. The material for this dialect was also collected by the author in 1935. A long vowel is written double only when the length is phonemic.

⁷ Lung-chow is in southeastern Kwangsi near the Vietnam border. The orthography adopted here is re-phonemicized, and is therefore slightly different from that in my book. *The Tai dialect of Lung-chow: Texts, translations, and glossary* (Shanghai, 1940).

to be loans from Sanskrit and Khmer, or seem to be Siamese (and Lao) innovations not found in the common Tai.⁸

While some clusters are not difficult to determine, others involve rather complicated considerations, such as the development of the tones and the distribution of consonants and vowels. In order to give a fairly full picture of the problem of consonant clusters, the following correspondences are given which seem to indicate traces of clusters.

2.1. The forms compared are taken from material compiled by the author for the comparison of twenty languages and dialects. Seven languages and dialects are chosen to illustrate the various developments of the clusters, and to make available a few interesting dialects in China not easily available to scholars. Siamese, Lao, Ahom, and Lung-chow forms are quoted from published sources (cf. *supra*, notes); forms of Wu-ming, Po-ai,⁹ and T'ien-chow are from the author's notes, recorded 1935-40 for the Academia Sinica. The Ahom forms show many irregularities, partly because the native orthography permits several interpretations in reading, very much like the Shan orthography, and partly because Ahom is no longer a living language and the Deodhais (priests) are probably not always certain of the pronunciation. Borua's spelling in his dictionary does not permit a phonetic transcription; his forms are quoted without alteration. The Lao forms are phonemized according to the description given by Guignard, but one may suspect that Guignard tends to lean too heavily on Siamese, and the system may look too Siamesish.

When a form is not available in Ahom, a form will be quoted in brackets from Shan, a closely related language. Similarly, a Nung, Tho, or Tay form¹⁰ may be given in brackets when a Lung-chow form is not available or when there is some divergent development in this group, and a Dioi form¹¹ may be given in addition to the Po-ai form.

2.2. The system of tones has been studied by Maspero, Wulff, and recently by Haudricourt.¹² The correspondences in tones are important for the reconstruction of the initial consonant because of their close relationship with the voiced and the voiceless nature of the consonant. The following summary may be useful as a guide for the examination of the forms compared.

2.3. There are four tone categories in PT, designated here by A, B, C, and D. The numerals 1 and 2 are added after the letters to show (1) tones regularly developed from a voiceless consonant and (2) tones regularly developed from a

⁸ See A. G. Haudricourt, *Les phonèmes et le vocabulaire du thai commun*, JA 1948.226-7.

⁹ Po-ai is a district in southeastern Yunnan on the Kwangsi border. The material was recorded by the author in 1940.

¹⁰ Cf. F. M. Savina, *Dictionnaire étymologique français-nung-chinois* (Hongkong, 1924); E. Diquet, *Étude de la langue tho* (Paris, 1910); F. M. Savina, *Dictionnaire tay-annamite-français* (Hanoi, 1909). The original orthography is based either on the French spelling or on the Vietnamese romanization. A phonetic transcription is attempted here.

¹¹ Cf. Esquirol and Williate, *op.cit.* Forms are quoted in phonetic transcription instead of the original orthography based on the French spelling.

¹² H. Maspero, *Contribution à l'étude du système phonique des langues thai*, BEFEO 19.152-69 (1911); K. Wulff, *Chinesisch und Tai: Sprachvergleichende Untersuchungen* 123-66 (København, 1934); A. G. Haudricourt, *op.cit.* 207-11; Li, The hypothesis of a pre-glottalized series of consonants in primitive Thai, *Bull. Inst. of Hist. and Phil.* 9.177-87 (1947).

voiced consonant, although most voiced stops and fricatives are unvoiced in nearly all the modern dialects. While there are special conditions for tonal developments in some dialects, this system seems satisfactory for the general purpose of comparison. The accompanying table will give a general idea of the tones and their development in the six languages and dialects quoted here. The tones of Ahom are unknown, and the Lao tones are given according to Guignard's description.

SIAMESE			LAO	WU-MING	LUNG-CHOW	PO-AI	T'IEH-CHOW
A	1	rising mid level	rising	mid level	mid level	rising falling	low rising
	2	mid level	mid level	low falling	mid falling	high level	low falling
B	1	low level	low level	high rising	high level	low level	high rising
	2	falling	low rising-falling	low rising	low level	falling	mid level
C	1	falling	falling	high level	rising	mid-high level	high level high falling
	2	high (rising-falling)	high rising-falling	high falling	low falling	mid level	high falling
D	1	low level	rising (S) low level (L)	high rising	high level	high level (S) mid-high level (S) low level (L)	high rising
	2	high (S) falling (L)	high rising-falling low rising-falling	low rising	low falling	mid-high level (S) falling (L)	mid level

2.4. From the preceding table we can see that Siamese has five tones, if we identify D2 (S) with C2, while all the other languages have six. D1 and D2 often present different development according to whether the vowel is long (L) or short (S).¹³ Further conditions for the special development of some tone categories are as follows:

¹³ This refers to the vocalic lengths in the modern dialects. A short or a long vowel in one modern dialect need not always correspond to a short or a long vowel in another dialect. The problem of vocalic lengths and the vowel system of PT as a whole are yet to be studied.

A1 becomes mid level in Siamese if the initial is an unaspirated stop or affricate. It becomes low falling in Po-ai if the initial was PT ʔ- , ʔb- , ʔd- , or ʔj- , becoming respectively Po-ai ʔ- , m- , n- , or j- .

C1 becomes high falling in T'ien-chow if the initial was PT ʔ- , ʔb- , ʔd- , or ʔj- , preserved as such in T'ien-chow.

D1 (S) becomes mid-high level in Po-ai if the initial was PT ʔ- , ʔb- , ʔd- , or ʔj- .

Where the initial is a cluster the choice between tones 1 and 2 is determined by the first consonant, even where this has disappeared in the modern language. As regards their influence on the development of the tones, PT ʔ- , ʔb- , ʔd- , and ʔj- ¹⁴ are treated in most dialects as a simple glottal stop, except for some tone categories in Po-ai and T'ien-chow.

With the tone correspondences thus established, it is not necessary to give the tones in each dialect. We need to designate the tone category only once, as A1, A2, B1, etc.; irregularities will be noted where they occur.

3.0. The following correspondences are given each under a hypothetical reconstruction. In some cases the reconstructions are doubtful, but the correspondences seem to me real. I have not refrained from giving lone correspondences, if parallel developments are found in other series. As many factors are involved in the development of the clusters, some irregularity is to be expected, which cannot be explained until all these factors are cleared up.

In the tables, names of languages are abbreviated as follows: S = Siamese, L = Lao, A = Ahom, W = Wu-ming, L-c = Lung-chow, P = Po-ai, T = T'ien-chow.

3.1. PT *pl-*:

	S	L	A	W	L-c	P	T
fish (A1)	<i>plaa</i>	<i>paa</i>	<i>plā</i>	<i>pla</i>	<i>pjaa</i>	<i>pjaa</i>	<i>čaa</i>
small end of stick (A1)	<i>plaa<i>i</i></i>	<i>paa<i>i</i></i>	-	<i>plai</i>	<i>pjaa<i>i</i></i>	<i>pjaa<i>i</i></i>	<i>čaa<i>i</i></i>
empty (B1)	<i>plau</i>	<i>pau</i>	<i>plāo</i>	<i>plāu</i>	<i>pjau</i>	<i>pjuu</i>	<i>čuu</i>
banana flower (A1)	<i>plii</i>	<i>p<i>ii</i></i>	<i>pi</i>	-	<i>p<i>ii</i></i>	<i>p<i>ii</i></i>	-
leech (A1)	<i>pliŋ</i>	<i>p<i>ii</i>ŋ</i>	<i>ping</i> (<i>peng</i>)	<i>pliŋ</i>	<i>p<i>ii</i>ŋ</i>	<i>p<i>ii</i>ŋ</i>	<i>p<i>ii</i>ŋ</i>
wake someone up (D1)	<i>pluk</i>	<i>puk</i>	<i>puk</i> 'call'	-	<i>pjuk</i>	<i>pjok</i>	<i>čok</i>
bark, shell (D1)	<i>pliak</i>	<i>p<i>ii</i>ak</i>	<i>plek</i>	<i>pluk</i>	<i>pæk</i>	-	-

The change of *l* into *j* and its disappearance in Lung-chow and Po-ai depend upon the following vowel. The influence of the vowel on the development of the liquid may be seen in §§3.2, 3, 4, 8, 17. Notice the neat correspondence in T'ien-chow, where *pj-* is further palatalized to *č-*, but *p* remains before *i*; cf. also §3.2. The Ahom orthography is inconsistent in writing the liquids, and the actual pronunciation (given in brackets if different from that indicated by the orthography) sometimes restores the liquid, but not always; cf. §§3.2, 4, 7, 18, etc.

¹⁴ For the reconstruction of this series of consonants and their influence on tone in various dialects, see Li, The hypothesis of a pre-glottalized series of consonants in PT.

3.2. PT *phl-*, *phr-*:

	S	L	A	W	L-c	P	T
rock (A1)	<i>phaa</i>	<i>phaa</i>	<i>phrā, phā</i>	<i>pla</i>	<i>phjaa</i>	<i>pjaa</i>	<i>čaa</i>
to walk (C1)	<i>phaai</i>	—	<i>phrāi</i>	<i>plai</i>	<i>phjaai</i>	<i>pjaai</i>	<i>čaai</i>
vegetable (D1)	<i>phak</i>	<i>phak</i>	<i>phāk</i>	<i>plāk</i>	<i>phjak</i>	<i>pjak</i>	<i>čak</i>
forehead (D1)	<i>phaak</i>	<i>phaak</i>	<i>phāk</i>	<i>plak</i>	<i>phjaak</i>	<i>pjaak</i>	<i>čaak</i>
hair (A1)	<i>phom</i>	<i>phom</i>	<i>phum</i> (<i>phrum</i>)	<i>plom</i>	<i>phjum</i>	<i>pjom</i>	<i>čom</i>
a kind of yam (D1)	<i>phīak</i>	<i>phīak</i>	<i>phūk</i> (<i>phok</i>)	<i>pliāk</i>	<i>phæk</i>	<i>piik</i>	<i>piak</i>
to burn (A1)	<i>phau</i>	<i>phau</i>	<i>phreu</i>	<i>plāu</i>	—	<i>pjau</i>	—
lean (A1)	<i>phoom</i>	<i>phoom</i>	—	<i>plöm</i>	<i>phjoom</i>	<i>pjoom</i>	—

PT *ph-* loses its aspiration in Wu-ming, Po-ai, and T'ien-chow. It is characteristic of many Tai dialects in Kweichow, Kwangsi, and part of Yunnan province to lose the aspiration of all aspirated consonants. As a liquid is indicated by all dialects except Siamese and Lao, it is assumed that it was dropped there. As a matter of fact the dropping of the liquid after an original aspirated voiceless stop is regular in Siamese; cf. §§3.12, 19, 20. It is not certain whether the liquid is *l* or *r*, because neither the Ahom nor the Wu-ming forms are decisive, and it is possible that the liquids were already confused in PT. A related group of languages, Kam-Sui,¹⁵ often shows divergent development. For example, Sui *pjām*¹ 'hair', *pja*¹ 'rocky mountain', *pjak*⁵ 'forehead', but *ʔyak*⁵ 'a kind of yam', *ʔyum*¹ 'lean'. Sui *γ-* often corresponds to Tai *r-*, cf. Sui *γan*¹ 'house', *γo*³ 'to know', *γo*³ 'or': Siam. *rian* (A2), *ruu* (C2), *rūi* (A1).

3.3. PT *pr-*:

	S	L	A	W	L-c	P	T
to expose to the sun (D1)	<i>taak</i>	<i>taak</i>	<i>tāk</i>	<i>rak</i>	<i>phjaak</i>	<i>taak</i>	<i>taak</i>
to break (D1)	<i>teek</i>	<i>teek</i>	<i>tik</i> (<i>tek</i>)	—	<i>pheek</i>	<i>teek</i>	<i>teek</i>
wasp (A1)	<i>teen</i>	<i>teen</i>	<i>tiñ</i> (<i>ten</i>)	—	<i>pheen</i>	<i>tin</i> (A2)	<i>tin</i> (A2)
strips of bamboo (D1)	<i>took</i>	<i>took</i>	<i>tāk</i>	<i>ruk</i>	<i>phjook</i>	<i>tuk</i>	—
shuttle of the loom (B1)	—	—	—	<i>rāu</i>	(Nung <i>pheau</i>)	<i>tau</i> (C2)	<i>tau</i>
hunt (B1)	—	—	—	—	<i>phjau</i>	<i>tau</i>	<i>tau</i>
cucumber (A1)	<i>teey</i>	<i>teey</i>	<i>ting</i>	—	(Nung <i>phēy</i>)	<i>tiy</i>	—

The importance of Wu-ming and Lung-chow for the reconstruction is obvious. For the development of PT *pr-* into *t-* in most dialects cf. *ʔbl-* > *ʔd-* or *d-* (§3.8.) and *ml-* > *n-* (§3.9). The aspirated *ph-* in Lung-chow is due to *r*, which in many dialects and in various conditions seems to show a strong tendency to become *h*. Thus Siam. *r-*, Shan and Lao *h-*; cf. also §§3.5, 6, 22 for the aspiration due to *r* in Ahom and Lao, and §3.11. for the Lung-chow group.

¹⁵ Cf. Li, The distribution of initials and tones in the Sui language, *Lg.* 24.165 note (1948).

3.4. PT *bl*-:

	S	L	A	W	L-c	P	T
to slip and fall (D2)	<i>phlaat</i>	-	(Shan <i>pat</i>)	-	<i>pjaat</i>	<i>pjaat</i>	<i>čaat</i>
to climb (A2)	-	-	-	<i>plen</i>	-	<i>peen</i>	-
half (B2)	-	-	<i>pung</i> (<i>plung</i>)	-	<i>pjoon</i>	<i>pjoon</i>	-
lance (B2)	-	-	<i>pāo</i> (<i>plāo</i>) 'arrow'	-	(Tho <i>yao</i>)	(Dioi <i>piao</i>)	-

The initial voiced consonant is indicated both by the tone and by the correspondence of an aspirated *ph*- in Siam. and Lao to an unaspirated *p*- in the others.

3.5. PT *br*-:

	S	L	A	W	L-c	P	T
to separate (D2)	<i>phraak</i>	<i>phaak</i>	<i>phāk</i> (Shan <i>phak</i>)	<i>plak</i>	<i>pjaak</i>	<i>pjaak</i>	-
to deceive (A2)	<i>phraan</i>	<i>phaan</i>	<i>phāng</i> (Shan <i>phan</i>)	-	<i>pjaan</i>	<i>pjaan</i>	-

The aspirated *ph*- in Ahom and Shan is due to *r*, while normally PT *b*- is represented by *p*-.

3.6. PT *vr*-:

	S	L	A	W	L-c	P	T
sword (C2)	<i>phraa</i>	<i>phaa</i>	(Shan <i>pha</i>)	<i>ša</i>	<i>pjaa</i> ¹⁶	<i>šaa</i>	<i>šaa</i>
evening meal (A2)	-	-	(Shan <i>phāu</i>)	<i>šau</i>	<i>pjau</i>	<i>šau</i>	<i>šau</i>
altogether (C2)	<i>phrɔɔm</i>	<i>phɔɔm</i>	<i>phām</i>	-	-	<i>šom</i>	-
tomorrow (D2)	<i>phruk</i>	-	(Shan <i>phuk</i>)	<i>šok</i>	<i>pjuk</i>	<i>šook</i>	<i>šook</i>

If the reconstructed form is correct, PT *vr*- has fallen together with PT *br*- in Siamese, Lao, Ahom, and Lung-chow, but shows quite a different development in Wu-ming, Po-ai, and T'ien-chow.¹⁶ The normal word for 'tomorrow' in Siamese is *phruŋ* (B2) *nii* (C2), where the final consonant is due to assimilation to the nasal initial of the following syllable; cf. a similar development in *waan* (A2) *nii* (C2) 'yesterday' < **waa nii* < PT *ɣwaa nii*.

3.7. PT *vl*-?

	S	L	A	W	L-c	P	T
ashes (B2)	<i>thau</i>	<i>thau</i>	<i>tāo</i>	<i>tāu</i>	<i>pjau</i>	<i>tau</i>	<i>tau</i>

Lung-chow is the only group which shows traces of a labial initial, cf. §3.3; elsewhere PT *vl*- > **d*-.

3.8. PT *ʔbl*- (*ʔbr*-?):

	S	L	A	W	L-c	P	T
to weed (A1)	<i>daai</i>	<i>baai</i>	<i>māi</i> (Shan <i>mai</i>)	<i>ʔdai</i>	<i>bjaai</i>	<i>naai</i> (Dioi <i>dai</i>)	<i>ʔdaai</i>
to pickle (A1)	<i>dɔɔŋ</i>	<i>bɔɔŋ</i>	(Shan <i>mɔɔŋ</i>)	-	-	<i>nooŋ</i> (Dioi <i>doŋ</i>)	-
bile (A1)	<i>dii</i>	<i>bii</i>	<i>di</i> (Shan <i>li</i>)	<i>ʔboi</i>	<i>dii</i>	<i>nii</i> (Dioi <i>bi</i> , <i>di</i>)	<i>ʔbii</i>

¹⁶ For a parallel development of consonant clusters into *š*-, cf. H. Maspero, *Études sur la phonétique historique de la langue annamite*, BEFEO 12.76-88 (1912).

	S	L	A	W	L-c	P	T
navel, umbilical cord (A1)	-	bīi	shāi-ni	?bī	-	nəə	-
whitish, white-spotted (B1)	daaŋ	baaŋ	dāŋ (Shan laŋ)	-	-	(Dioi daŋ)	-
flower (D1)	dɔk	dɔk	blɔk (Shan mɔk)	-	bjook	(Dioi do)	-
month (A1)	dian	dian	din (den) (Shan lēn)	?diān	bəən	nəən (Dioi diən)	?diən

Many dialects show irregularities in this series, except Siamese and Po-ai, but further differentiation seems not advisable. Some of these irregularities seem to be due to the influence of the following vowel, others probably due to interdialect loans. Notice that Lao and Siamese, extremely close to each other in many aspects, are here divergent. Apparently this cluster yields different developments in closely related groups of dialects, and is thus particularly liable to dialect mixture; cf. similar irregularities in §3.9.

3.9. PT *ml-* (mr-?):

	S	L	A	W	L-c	P	T
saliva (A2)	laai	laai	lāi	mlai	laai (Nung meai)	naai	naai
to destroy (A2)	ma-laai	-	māi (dam-age)	-	(Nung meāi)	(Dioi nai)	-
to love (D2)	mak, ma-lak	mak	(Shan māk)	-	-	(Dioi nak)	-
insect (A2)	mɛɛŋ, ma-lɛɛŋ	mɛɛŋ	mīng (mleng)	nɛŋ	meɛŋ	neɛŋ	neɛŋ
grain, seed (D2)	let, met, ma-let	mit	(Shan met)	-	-	net	nat
to rust (C2)	-	miəŋ?	-	mlāi	-	nai	-
slippery (D2)	-	-	-	mlak	mjaak	mjaak	-
to flash (D2)	map	map	-	-	meep	mjaap	-

Notice the divergent developments and occasional doublets in Siamese. Such doublets with practically identical meanings seem best explained as dialect mixtures.

3.10. PT *tl-*:

	S	L	A	W	L-c	P	T
banana leaf (A1)	toŋ	toŋ	-	rōŋ	toŋ	looŋ	-
to break wind (D1)	tot	tot	tet	-	(Nung tət)	lot	lot
full (A1)		tem	tem	tim	rim	tim	lim
to hold on the lap, to carry in the bosom (D1)		tak	tak	-	rāk	-	lak

The reconstruction of *tl-* for this series is largely based on the contrast with the next series, §3.11. Po-ai and T'ien-chow *l-* is not significant, because they have no *r-*. PT *tl-* presumably gives Wu-ming *r-*.

3.11. PT *tr*-:

	S	L	A	W	L-c	P	T
eye (A1)	<i>taa</i>	<i>taa</i>	<i>tā</i>	<i>ra</i>	<i>haa</i> (Tho <i>tha</i>)	<i>taa</i>	<i>taa</i>
to die (A1)	<i>taai</i>	<i>taai</i>	<i>tāi</i>	<i>rai</i>	<i>haai</i> (Tho <i>thai</i>)	<i>taai</i>	<i>taai</i>
grasshopper (D1)	<i>tak</i> ¹⁷ <i>teen</i>	<i>tak-teen</i>	-	<i>rāk</i>	(Tay <i>thāk</i>)	<i>tak</i>	<i>tak</i>

The aspiration in Lung-chow and in Tho is due to the influence of *r*; cf. PT *pr*- > Lung-chow *phj*- (§3.3.).

3.12. PT *thl*-:

	S	L	A	W	L-c	P	T
to wait (C1)	<i>thaa</i>	<i>thaa</i>	<i>thā</i>	-	(Tho <i>tha</i>)	<i>šaa</i>	<i>šaa</i>
to plow (A1)	<i>thai</i>	<i>thai</i>	<i>thāi</i>	<i>šai</i>	<i>thai</i>	<i>šai</i>	<i>šai</i>
bag (C1)	<i>thuŋ</i>	<i>thooŋ</i>	<i>thung</i>	<i>šun</i>	<i>thuŋ</i>	<i>šoŋ</i>	-
to ask (A1)	<i>thaam</i>	<i>thaam</i>	<i>thām</i>	<i>šam</i>	<i>thaam</i>	<i>šaam</i>	<i>šaam</i>

3.13. PT *thr*-:

	S	L	A	W	L-c	P	T
several persons carry (A1)	<i>haam</i>	<i>haam</i>	<i>rām</i>	<i>ram</i>	<i>haam</i> (Tho <i>tham</i>)	<i>leem</i>	<i>laam</i>
to chop (C1)	<i>ham</i>	-	-	<i>rām</i>	<i>tham</i> (dial.)	<i>lam</i>	<i>lam</i>
tail (A1)	<i>haaŋ</i>	<i>haaŋ</i>	<i>rāŋ</i>	<i>riaŋ</i>	<i>haaŋ</i> (Tho <i>thaŋ</i>)	<i>ləŋ</i>	<i>liəŋ</i>
to carry on shoulder with pole (D1)	<i>haap</i>	<i>haap</i>	<i>rāp</i>	<i>rap</i>	<i>haap</i> (Tho <i>thap</i>)	<i>laap</i>	<i>laap</i>
headlouse (A1)	<i>hau</i>	<i>hau</i>	<i>rāo</i>	<i>rāu</i>	<i>hau</i> (Tho <i>thao</i>)	<i>lau</i>	<i>lau</i>
warm (C1)	-	-	<i>rāo</i>	-	<i>hau</i> (Nung <i>thāu</i>)	<i>lau</i>	-
head (A1)	<i>hua</i>	<i>hua</i>	<i>ru, rū</i>	-	<i>huu</i> (Tho <i>thua</i>)	-	-
stone (A1)	<i>hin</i>	<i>hin</i>	<i>rin</i>	<i>rin</i>	<i>hin</i> (Tho <i>thín</i>)	<i>hin?</i>	<i>lin</i>
loom (D1)	<i>huuk</i>	<i>huk</i>	-	<i>rök</i>	<i>huk</i> (Tho <i>thúk</i>)	<i>look</i>	<i>look</i>
sweat (B1)	<i>hīa</i>	<i>hīa</i>	<i>ru, rū</i>	-	<i>həə</i> (Tho <i>thīa</i>)	-	-
to break a stick (D1)	<i>hak</i>	<i>hak</i>	<i>rāk</i>	<i>rāk</i>	<i>tak?</i>	<i>lak</i>	<i>lak</i>

The dental of the cluster has disappeared in practically all dialects, except Tho, which is rather important for our reconstruction, and is therefore quoted under Lung-chow.

3.14. PT *dl*-?

	S	L	A	W	L-c	P	T
stairs, ladder (A1)	<i>ban-dai</i>	<i>dai</i>	<i>khā-dāi</i>	<i>loi</i> (C2)	<i>dai</i>	<i>lai</i> (A2)	-

¹⁷ Siamese *tak*- has a high tone, and therefore disagrees with the usual development of D1. The second syllable *-teen* may be the word for 'wasp'; cf. §3.3.

3.15. PT *nl*-?

	S	L	A	W	L-c	P	T
to tell (B2)	<i>lau</i>	<i>lau</i>	<i>lāo</i>	<i>nāu</i> (A2)	(Nung <i>lǎu</i>)	<i>nau</i> (A2)	<i>nau</i> (A2)

3.16. PT *nl*-?

	S	L	A	W	L-c	P	T
water (C2)	<i>nam</i>	<i>nam</i>	<i>nām</i>	<i>rām</i>	<i>nam</i> ?	<i>lam</i>	<i>lam</i>
bird (D2)	<i>nok</i>	<i>nok</i>	<i>nuk</i>	<i>rok</i>	<i>nuk</i>	<i>lok</i>	<i>lok</i>
outside (D2)	<i>nɔk</i>	<i>nɔk</i>	-	<i>rōk</i>	<i>nook</i>	<i>look</i>	<i>look</i>
bee's sting (A2)	<i>nai</i>	<i>lai</i> ?	-	-	-	<i>lai</i>	-

3.17. PT *kl*-:

	S	L	A	W	L-c	P	T
young rice plant (C1)	<i>klaa</i>	<i>kaa</i>	(Shan <i>ka</i>)	<i>kla</i>	<i>kjaa</i>	<i>ɛaa</i>	<i>ɛaa</i>
far (A1)	<i>klai</i>	<i>kai</i>	<i>shai</i> ?	<i>klāi</i>	<i>kwai</i>	<i>ɛai</i>	<i>ɛai</i>
middle (A1)	<i>klaaŋ</i>	<i>kaaŋ</i>	<i>kāŋ</i> (<i>klāŋ</i>)	<i>klaŋ</i>	<i>kjaaŋ</i>	<i>ɛaaŋ</i>	<i>ɛaaŋ</i>
purple, dark (B1)	<i>klam</i>	<i>kam</i>	-	<i>klām</i> 'plant used as dye'	-	<i>ɛam</i>	-
fish scale (D1)	<i>klet</i>	<i>ket</i>	(Shan <i>ket</i>)	<i>klip</i> ?	<i>kit</i>	<i>ɛet</i>	<i>ɛat</i>
tube (C1)	<i>klɔŋ</i>	<i>kɔŋ</i>	<i>kɔŋ</i> (<i>klɔŋ</i>)	<i>kloŋ</i>	-	(Dioi <i>kioŋ</i>)	-
drum (A1)	<i>klɔŋ</i>	<i>kɔŋ</i>	<i>kɔŋ</i> (<i>klɔŋ</i>)	<i>klōŋ</i>	<i>kjoŋ</i>	<i>ɛoŋ</i>	<i>ɛoŋ</i>
wicker hat (D1)	<i>klop</i> 'to cover'	<i>kup</i>	<i>kup</i>	<i>klop</i>	<i>kip</i> ?	<i>ɛop</i>	<i>ɛop</i>
banana (C1)	<i>kluai</i>	<i>kuai</i>	<i>kuā</i> (<i>kui</i>)	<i>klōi</i>	<i>kuui</i>	<i>ɛooi</i>	<i>ɛooi</i>
salt (A1)	<i>klia</i>	<i>kīa</i>	<i>kūw</i> (<i>klu</i>)	<i>klū</i>	<i>kə</i>	<i>ɛuu</i>	<i>kuə</i>
kind of basket (A1)	<i>kruai</i> ?	<i>kuai</i>	(Shan <i>kui</i>)	<i>klōi</i>	(Tho <i>kuei</i>)	<i>ɛooi</i>	-
near (C1)	<i>klai</i>	<i>kai</i> , <i>khēē</i> ?	<i>kāo</i> , <i>klāi</i>	<i>klai</i>	<i>khjaə</i> ?	<i>ɛaə</i>	<i>ɛai</i>
top of head, head (C1)	<i>klaui</i>	<i>kau</i>	(Shan <i>kāu</i>)	<i>rāu</i> ?	<i>kjau</i>	<i>ɛau</i>	<i>ɛau</i>

Lung-chow *kwai* comes probably from PT *klw*-, *khja* from PT *khl*-, similarly Lao *khēē*, apparently a dialect form; Wu-ming *rāu* may be from PT *kr*-, cf. §3.18.

3.18. PT *kr*-:

	S	L	A	W	L-c	P	T
husk of grain (D1)	<i>kleep</i> ?	<i>keep</i>	<i>kip</i>	<i>rip</i> (D2)	<i>keep</i>	<i>leep</i> (D2)	<i>leep</i> (D2)
smooth, to polish (A1)	<i>klau</i> ?	<i>kau</i>	-	-	-	<i>lau</i> (A2)	<i>lau</i>
cage (A1)	<i>kroŋ</i>	<i>kɔŋ</i> (C1)	-	<i>ruŋ</i> (A2), <i>kloŋ</i> (A2)	<i>huŋ</i> (B1)	<i>loŋ</i> (B1)	<i>loŋ</i> (B1)

Notice Siam. *kl*-, and the irregularities in tone.

3.19. PT *khl*-:

	S	L	A	W	L-c	P	T
to shut up, to imprison (A1)	<i>khay</i>	<i>khay</i>	<i>khāŋ</i> 'dam'	<i>klāŋ</i>	(Nung <i>hay</i>)	<i>ɛay</i>	-
hard (A1)	<i>khεεŋ</i>	<i>khεεŋ</i>	(Shan <i>khεy</i>)	<i>kleŋ</i>	<i>kheεŋ</i>	<i>ɛεεŋ</i>	-
kind of basket (C1)	<i>khɔŋ</i>	-	<i>khraŋ</i>	<i>klōŋ</i> (B1)	<i>khoŋ</i>	<i>ɛoŋ</i> (B1)	-

3.20. PT *kh-*:

	S	L	A	W	L-c	P	T
top (toy) (B1)	<i>khaaŋ</i>	<i>khaaŋ</i>	<i>khāŋ</i>	<i>raŋ</i>	<i>haaŋ</i>	<i>ɕaaŋ</i>	<i>ɕaaŋ</i>
egg (B1)	<i>khai</i>	<i>khai</i>	<i>khāi</i> (<i>khraī</i>)	<i>rāi</i>	<i>khjai</i>	<i>ɕai</i>	<i>ɕai</i>
turtle dove (A1)	<i>khaυ</i>	<i>khaυ</i>	-	<i>rāυ</i>	-	<i>lau?</i>	-

Wu-ming seems to be the only dialect here which distinguishes PT *khl-* and *kh-*.
Po-ai *lau* may be from PT *kr-* or *xr-* (§3.18; §3.26), Lung-chow *haaŋ* from PT *xr-*.

3.21. PT *gl-*:

	S	L	A	W	L-c	P	T
to catch with a rope (C2)	<i>khɽɔŋ</i>	<i>khɔŋ</i>	(Shan <i>kɔŋ</i>)	<i>klɔŋ</i>	<i>kjoŋ?</i>	(Dioi <i>kiŋ</i>)	-
to crawl (A2)	<i>khlaan</i>	<i>khaan</i>	<i>kān</i>	-	<i>kjaan</i>	-	-
nearly alike (D2)	<i>khlap-</i> <i>khlaai</i> (C2)	<i>khap-</i> <i>khaai</i>	-	-	-	<i>ɕap</i> 'to even up'	-

3.22. PT *gr-*:

	S	L	A	W	L-c	P	T
to cover (B2)	<i>khɽɔm</i>	<i>khɔm</i>	<i>khām</i>	-	-	<i>ɕum</i> (B1)	-
to desire (B2)	<i>khrai</i>	<i>khai</i>	<i>kheu</i>	<i>klai</i> (A2)	-	(Dioi <i>kiai</i> A2)	-
idle, lazy (C2)	<i>khraan</i>	<i>khaan</i>	<i>khān</i> 'to neglect'	-	<i>kjaan?</i>	-	-
utensil (B2)	<i>khriŋ</i>	<i>khīŋ</i>	<i>khing</i> (<i>khriŋ</i>)	-	(Tho <i>kīēŋ</i>)	-	-
mortar (D2)	<i>khrok</i>	<i>khok</i>	<i>kūk</i> (<i>kuk</i>) (Shan <i>khok</i>)	-	<i>kjuk</i>	-	-
half (B2)	<i>khriŋ</i>	-	<i>king</i> (<i>klīŋ</i>) (Shan <i>khīŋ</i>)	<i>klāŋ</i> 'to block the road half way'	-	<i>ɕaŋ</i>	-
kind of bug (A2)	<i>khɽeŋ</i>	<i>khɛŋ</i>	(Shan <i>kɛŋ</i>)	-	(Nung <i>keŋ</i>)	<i>keŋ</i>	<i>keŋ</i>

PR *gr-* > Ahom and Shan *kh-* is regular, but note the two cases of unaspirated *k-* in Ahom, where Shan has *kh-*.

3.23. PT *ɣl-*?

	S	L	A	W	L-c	P	T
to vacillate (A2)	<i>khɽɔn</i>	<i>khɔn</i>	<i>khān</i>	-	(Nung <i>hɔn</i>)	(Dioi <i>hon</i>)	<i>hoon</i>

3.24. PT *ɣr-*?

	S	L	A	W	L-c	P	T
indigo (A2)	<i>khraam</i>	<i>khaam</i>	-	-	<i>kjaam</i>	<i>ɕaam</i>	<i>ɕaam</i>

3.25. PT *xl-*:

	S	L	A	W	L-c	P	T
mud (B1)	<i>lom</i>	<i>lom</i>	(Shan <i>lom</i>)	<i>lom</i> 'stuck in mud'	<i>lum</i> 'to sink'	<i>lom</i> 'to sink in mud'	<i>lom</i>
liquor (C1)	<i>lau</i>	<i>lau</i>	<i>lāo</i>	<i>lāu</i>	<i>lau</i>	<i>lau</i>	<i>lau</i>
nephew (A1)	<i>laan</i>	<i>laan</i>	<i>lān</i>	<i>lan</i>	<i>laan</i>	<i>laan</i>	<i>laan</i>

	S	L	A	W	L-c	P	T
to close the eyes (D1)	<i>lap</i>	<i>lap</i>	<i>lāp</i> 'out of sight'	<i>lāp</i>	<i>lap</i>	<i>lap</i>	<i>lap</i>
to fear (A1)	-	-	-	<i>lau</i>	<i>laau</i>	<i>laau</i>	<i>laau</i>
iron (D1)	<i>lek</i>	<i>lek</i>	<i>lik</i>	-	<i>lik</i>	<i>lek</i>	-
surplus (A1)	<i>lia</i>	<i>lia</i>	(Shan <i>lē</i>)	<i>lī</i>	<i>ləə</i>	<i>ləə</i>	<i>liə</i>

All the modern Tai dialects show *l-*, but the tone indicates a voiceless initial. The Sui group of languages definitely indicates a guttural cluster; cf. Sui *khum*⁵ 'mud', *khau*³ 'liquor', *khan*¹ 'nephew', *khāp*⁵ 'to close the eyes', *kho*¹ 'to fear', *khət*⁵ 'iron', but *ka*¹ 'surplus'. One may assume that PT *xl-* early became *hl-* or simply a voiceless *l-* in most Tai dialects, but it is impossible to determine whether this took place in the PT period or later. Sui is not included in our narrowly defined Tai group. Cf. PT *xr-* in §3.26, where some Tai dialects still show a guttural initial.

3.26. PT *xr-*:

	S	L	A	W	L-c	P	T
shower (B1)	<i>haa</i>	<i>haa</i>	<i>rā</i>	<i>ra</i>	<i>haa</i> (TP <i>khjaa</i>)	<i>laa</i>	<i>laa</i>
to seek (A1)	<i>haa</i>	<i>haa</i>	<i>khā</i>	<i>ra</i>	<i>haa</i>	<i>laa</i>	-
six (D1)	<i>hok</i>	<i>hok</i>	<i>ruk</i>	<i>rok</i>	<i>huk</i> (TP <i>khjo</i> ?)	<i>lok</i>	<i>lok</i>
ear (A1)	<i>huu</i>	<i>huu</i>	-	<i>rī</i> (A2)	<i>huu</i> (TP <i>khjuu</i>)	<i>ləə</i> (A2)	<i>liə</i> (A2)
cast-net (A1)	<i>hee</i>	<i>hee</i>	<i>khe</i>	-	<i>hee</i> (Nung <i>khe</i>)	<i>lee</i>	-
to scorch (C1)	-	<i>heem</i>	-	-	-	<i>leem</i>	-
pimples (D1)	<i>hit</i>	<i>hit</i>	<i>rit</i>	-	(TP <i>khjet</i>)	<i>lat</i>	-
mountain stream (C1)	<i>huai</i>	<i>huai</i>	<i>rui</i>	<i>rui</i>	<i>huui</i> , <i>vuui</i> (Nung <i>khui</i>)	<i>vi</i>	-
to laugh (A1)	<i>hua-rə</i> ?	-	<i>khu</i> (<i>khru</i>)	<i>riäu</i>	<i>huu</i> (TP <i>khuu</i>)	<i>ljuu</i>	<i>liau</i>
to bark (B1)	<i>hau</i>	<i>hau</i>	<i>heu</i>	<i>räu</i>	<i>hau</i>	<i>lau</i>	<i>lau</i>

The T'ien-pao dialects (abbr. TP) in southwestern Kwangsi and Nung are quoted under Lung-chow, because they still show a guttural initial which is lost in most other dialects. There are also sporadic cases of *kh-* in Ahom.

3.27. PT *yr-*?

	S	L	A	W	L-c	P	T
sesame (A2)	<i>ɣaa</i>	<i>ɣaa</i>	<i>ngā</i>	<i>ra</i>	<i>ɣaa</i>	<i>ɣaa</i>	<i>laa</i>

3.28. PT *l/r-*?

	S	L	A	W	L-c	P	T
mark (A2)	<i>laai</i>	<i>laai</i>	<i>lāi</i>	<i>rai</i>	<i>laai</i>	<i>laai</i> (Dioi <i>đai</i>)	<i>laai</i>
to steal (D2)	<i>lak</i>	<i>lak</i>	<i>lāk</i>	-	<i>lak</i>	<i>lak</i> (Dioi <i>đak</i>)	<i>lak</i>
to pull (D2)	<i>laak</i>	<i>laak</i>	<i>lāk</i>	<i>rak</i>	<i>laak</i>	<i>laak</i> (Dioi <i>đa</i>)	<i>laak</i>
to rinse (C2)	<i>laaŋ</i>	<i>laaŋ</i>	<i>lāŋ</i>	<i>riǎŋ</i>	<i>laaŋ</i> ?	<i>ləŋ</i> (Dioi <i>đəŋ</i>)	<i>liŋ</i>
to sharpen (D2)	<i>lap</i>	<i>lap</i>	<i>lāp</i>	-	-	(Dioi <i>đap</i>)	-
little (D2)	<i>lek</i>	<i>lek</i>	<i>lik</i>	-	-	<i>lek</i> (Dioi <i>dek</i>)	<i>lek</i>
draught (C2)	<i>leəŋ</i>	<i>leəŋ</i>	(Shan <i>lēŋ</i>)	<i>reŋ</i>	<i>leəŋ</i> ?	<i>leəŋ</i> (Dioi <i>deŋ</i>)	<i>leəŋ</i>
lunch (A2)	-	<i>leəŋ</i>	-	<i>riŋ</i>	<i>leəŋ</i>	<i>liŋ</i> (Dioi <i>diŋ</i>)	<i>liŋ</i>
finger nail (D2)	<i>lep</i>	<i>lep</i>	<i>lip</i>	<i>rip</i>	<i>lip</i>	<i>lit</i> ? (Dioi <i>dip</i>)	<i>lit</i> ?

	S	L	A	W	L-c	P	T
to lick (A2)	<i>lia</i>	<i>lia</i>	<i>le</i>	<i>ri</i>	<i>lii</i>	<i>lii</i> (Dioi <i>die</i>)	<i>liə</i>
to roll (C2)	<i>lɔɔ</i>	<i>lɔɔ</i>	—	—	—	<i>luu</i> (Dioi <i>du</i>)	—
wind (A2)	<i>lom</i>	<i>lom</i>	<i>lum</i> , <i>lüm</i>	<i>rum</i>	<i>lum</i>	<i>lum</i> (Dioi <i>dum</i>)	<i>lum</i>
to descend (A2)	<i>loŋ</i>	<i>loŋ</i>	<i>lung</i>	<i>roŋ</i>	<i>nuŋ?</i>	<i>loŋ</i> (Dioi <i>doŋ</i>)	<i>loŋ</i>
room (D2)	—	—	<i>luk</i>	<i>ruk</i>	—	<i>luk</i> (Dioi <i>duk</i>)	<i>luk</i>
to touch, to caress (D2)	<i>luup</i>	<i>luup</i>	<i>lup</i>	—	—	<i>lup</i> (Dioi <i>dup</i>)	—

As Po-ai and T'ien-pao *l-* may represent *l-* or *r-*, the Dioi forms are quoted, which show *ɬ-*, a dental fricative regularly corresponding to Wu-ming *r-*. It is impossible to decide from that Tai dialects alone what this alternation of liquids means or what cluster or clusters it represents.

3.29. PT *zl-*?

	S	L	A	W	L-c	P	T
to nourish, to bring up (C2)	<i>liəŋ</i>	<i>liəŋ</i>	<i>liŋ</i>	<i>ɕiəŋ</i>	<i>ɕəəŋ?</i>	<i>ɕiiŋ</i>	<i>ɕiəŋ</i>

Cf. Chinese *yang*³ < Anc. Ch. *ɣaŋ* < Arc. Ch. *zɣiaŋ*?

4.1. The preceding correspondences are given mainly to illustrate one phase of the complicated problem of comparative Tai phonology. A rather large number of consonant clusters are suggested for PT, to supplement the comparative studies of the Tai languages. These have hitherto been conducted with the main emphasis on Siamese, Lao, Shan, and Ahom, languages in the south and west. The Tai languages are generally assumed to be rather close to each other,¹⁸ but this does not mean that any one language, however archaic it might seem in some respects, can be assumed to represent the original Tai system. Thus Siamese *t-*, *h-*, etc. have a number of correspondences in the other Tai languages, and therefore must go back to different sources.

Scores of forms found in various dialects have been omitted from these lists, because I have no available material from the key dialect or dialect groups. For example, the differentiation of §3.3 and §3.11, or of §3.13 and §3.26, depends on the evidence available in the Lung-chow group alone. Lack of material from this group would make reconstruction impossible.

4.2. Similarly the Proto-Tai vocalic system is perhaps more complicated than we generally assume it to be. There may be close relations between the development of the consonants and the vowels, as in many Chinese dialects. The study of the vowel system may eventually clarify some problems concerning the initial consonants. The material for a comparative study of the Tai languages is fortunately not too meager, though in some instances not very precisely recorded. It is my belief that a satisfactory phonological system can be worked out, although the reconstructions may remain uncertain for a long time to come.

¹⁸ For instance, H. Maspero, *La langue chinoise*, *Conf. Inst. Ling. Univ. Paris* 1933.87.

REVIEWS

Languages in contact: Findings and problems. By URIEL WEINREICH, with a preface by ANDRÉ MARTINET. (Publications of the Linguistic Circle of New York, No. 1.) Pp. xii, 148. New York, 1953.

Reviewed by EINAR HAUGEN, *University of Wisconsin*

1.1 Wherever speakers of different languages or dialects meet, there is language contact. Wherever there is contact, certain typical problems arise. The purpose of the present book is to set up a framework of theory which will enable the linguist to speak more precisely and clearly concerning these problems. It is thus a book about bilingualism, taken in the widest possible sense of this term. The study of bilingualism is essentially the study of the consequences of second-language learning. This has meant many things to many people, and connotes to most non-linguists the educational and psychological problems involved in childhood bilingualism. But anyone who has learned to understand a second language is a bilingual, and it is the linguist's task to provide the basic data concerning the linguistic situations involved in the process of language learning. Since linguists are bilingual by profession, the present book can hardly fail to intersect with every linguist's interests at some point. The subject is one that has been discussed by all books on general linguistics, under such heads as 'speech mixture', 'borrowing', 'substratum and superstratum influence', 'creolization', 'pidgin languages', and the like. But this appears to be the first time that the subject is treated at full length from the linguist's point of view. The author has been remarkably successful in compressing a great amount of information and suggestive speculation into a mere 148 readable pages.

1.2. The plan of the book is language-centered, its basic theme being to show the effects of bilingualism on the languages involved. As a general term for all such effects the author introduces the word 'interference', drawn from the writings of the Prague school, which he defines (1) as 'those instances of deviation from the norms of either language which occur in the speech of bilinguals as a result of their familiarity with more than one language.' He distinguishes between the interference that occurs in the speech of bilinguals and that which has been established in the language by diffusion to monolinguals. The chief interest of this book is its emphasis on the meaning of language contact to the individual who experiences it, rather than on the historical results of such contact. In spite of its concern with a topic which has ordinarily been thought of as historical in nature, it is thus a contribution to descriptive linguistics. At the same time it emphasizes the parallel between linguistic and cultural contact and pleads for a fruitful collaboration between linguists and other social scientists in the exploration of this field.

1.3. The order of treatment proceeds from the linguistic structure outward to the psychological and social factors involved in the bilingual situation. The author finds that the linguist's primary task is that of drawing up a 'differential description', or what this reviewer would rather call a BILINGUAL DESCRIPTION, of the two (or more) languages in contact. This means an exhaustive statement

of the 'differences and similarities between the languages' in every domain. Since any difference is a potential source of interference, this should enable the linguist to predict the behavior of bilinguals. The author divides this part of his discussion, entitled 'Mechanisms and structural causes of interference', into phonic, grammatical, and lexical interference. But he then goes on to show that in practice the amount of interference is limited by extra-linguistic factors inherent in the bilingual's relation to the languages he brings into contact. Those which might be called psycholinguistic are treated in a chapter on 'The bilingual individual', in which he discusses such problems as the speaker's facility of verbal expression, his ability to keep two languages apart, his relative proficiency in the languages, his specialization of the languages by topics and interlocutors, his manner of learning them (including the age and source of learning), and his attitude to each language. Those which are primarily ethnolinguistic he discusses in a chapter entitled 'The socio-cultural setting of language contact'. These include such topics as the size of the bilingual group and its relation to neighboring monolingual groups, the prestige of the language groups involved, attitudes toward bilingualism as such, tolerance with regard to mixed and incorrect speech in each language, and the symbolic values of the languages to their users. The reverse problems of the effects of bilingualism on the psychological and social status of the speakers are relegated to an appendix containing a brief survey of the literature on intelligence, group identification, character formation, and educational problems.

1.4. The terminology and theoretical framework is in part original and may be characterized as a generally happy combination of American and European practice. The influence of Saussure is probably stronger than that of Bloomfield, and one notes overtones of Prague and Copenhagen in the use of such terms as 'expression' and 'content', in his emphasis on the structural causes of diachronic change, and in his lack of squeamishness about statements on meaning (the unit being the 'semanteme'). But the author's care in defining his terms is such that one should have no difficulty in translating his statements into other systems of metalinguistic. As a research tool it will be of the greatest value because of its bird's-eye view of a widely discussed field, its numerous examples drawn from the most diverse languages, its suggestions (§5) for research methods and opportunities, and its impressive bibliography of some 658 items. The bibliography is especially rich in references to the German and Swiss literature, much of which is inaccessible in this country, and to Yiddish literature, which is generally unknown to non-specialists. The field is important, not only for its theoretical implications in linguistic analysis, but also for the practical problems of teaching foreign languages.¹ So far most descriptive linguists have been interested only in setting up the structures of single languages. Very little work has been done on the problem that arises in the strictly synchronic comparison of different linguistic structures.

¹ Cf. C. C. Fries, *Teaching and learning English as a foreign language* 9 (Ann Arbor, Mich., 1945): 'The most efficient materials are those that are based upon a scientific description of the language to be learned, carefully compared with a parallel description of the native language of the learner.'

2.1. The author begins his bilingual description by listing in tabular form the phonemes of two dialects in contact observed by himself, one Romansh (R) and one Schwyzertütsch (S). After some brief statements concerning their allophones, he makes two comparisons, one with R as the primary system, and one with S as primary. This amounts, in effect, to a study of an R-accent in S and an S-accent in R. The phonemic comparison leads one to expect certain kinds of interference which do in fact turn up in speech. In summarizing his results, the author finds four types of phonological interference. Three of these are phonemic (under-differentiation, over-differentiation, and reinterpretation), the fourth phonetic (phone substitution). He grants that these classifications do not emerge from the raw data directly, but from their phonemic analysis.

2.2. At this point the reviewer may be permitted to raise certain questions. It is evident that any bilingual description will bring out theoretical differences concerning description in general. Weinreich's approach impresses the reviewer as possibly giving undue weight to STRUCTURE as a factor in the bilingual situation without clarifying just what is meant by this term. The author maintains for instance that Russian /p/ cannot be regarded in principle as being the 'same' as English /p/, since they are parts of different structures. Russian /p/ is defined as nonpalatalized in opposition to /p'/, while in English this distinction is irrelevant. He specifically says that as structural units they are incommensurable, but that in spite of this, the bilingual identifies them 'astride the limits of the languages' because they have a 'physical resemblance'. In Copenhagen terms, it is their substance that is identified, and only indirectly their 'real' or structural selves. But if this be true, it is the substance and not the structure that should be compared. A list of allophones would presumably have been more to the point than a list of phonemes. The use of the same symbols for Russian /p/ and English /p/ is already a falsification, since it presupposes a phonetic identification which has not yet been made. In his table of R and S consonants, Weinreich uses different symbols for R stops /b d g/ and S stops /B D G/, apparently for phonetic reasons. But structurally they appear to be in precisely the same position, and there is no obvious phonemic reason for writing the capitals in S. The fortis series /p t k/ are written alike in the two languages.

2.3. The author's dichotomy between structure and substance goes so far as to make him declare that 'the actual sounds produced by the bilingual lie, as it were, in the structural no man's land between two phonemic systems', and that therefore 'their interpretation in functional, i.e. phonemic, terms is subject to special difficulties' (14).² But is a phonemic system, as set up in Weinreich's or any one else's formulation, a gestalt which exerts influence on the behavior of the speakers? More concretely: does a bilingual ever identify two phonemes because they occupy analogous places in the phonemic charts of the two languages? Weinreich does not put it quite as baldly as this, but under his 'structural causes of interference' this seems to be what he is saying. He regards R /ε/ and S /æ/ as somehow the 'same' phoneme because 'both are defined as front vowels

² Cf. Z. S. Harris, *Methods in structural linguistics* 72 fn. 28 (Chicago, 1951).

of maximum openness'. In practice this means that they both appear in the lower left-hand corner of his chart:

R	i	S	i
	ɪ		e
	ɛ		æ

When R speakers therefore identify the two, he considers this merely an allophonic change ('phone substitution'). But R speakers do not limit themselves to this identification; two pages earlier we have learned that they also identify their /ɛ/ with S /e/. While he does not specifically label this case of interference, he presumably regards it as an instance of 'reinterpretation of distinctions'. As the phonetic symbols indicate, the R /ɛ/ is phonetically intermediate between S /e/ and /æ/, with probably some overlapping of allophones, and the confusion might well be regarded as an 'under-differentiation of phonemes'. It is not easy to see why one of the two possible identifications should be allophonic, the other phonemic, if that is really what was intended.

2.4. This use of the abstracted pattern as a reality to be reckoned with is exemplified also in his assertion that the 'existence of suitable "holes in the pattern" or "empty cases" in the primary phonemic system' makes it easier for the learner to learn certain sounds (22). His example is the R /ʒ/, which the S speaker finds easier because he has /ʃ/ and /z/ already and can therefore 'pigeon-hole' the new one into the space where their qualities intersect. Interpreted in terms of phonetic components this means simply that muscular movements can be analyzed into components and recombined in new sequences. But even such recombination requires learning. Even phonemes that already occur in a language are difficult when they occur in new sequences, e.g. /ŋ/ initially or /h/ before consonants in English. Norwegian has /s/ and a contrast of voiced with voiceless consonants, but no /z/; in spite of this 'empty space' where voicing and sibilance cross, this is empirically the last English sound learned by a Norwegian.

2.5. Another line of explanation advanced by the author is the factor of phonemic burdening (he vacillates between the words 'yield' and 'burdening'). He finds that S /k^h/ is reproduced as R /k/ because S /k/ is a rare phoneme and therefore the distinction between /k/ and /k^h/ is not sufficiently important for S speakers to correct the error in R speech. He attributes the absence of the aspiration to the low functional burdening of the contrast /k/-/k^h/. This would seem to be putting the cart before the horse. The naive speaker who has no /k^h/ in his language will surely identify it with his own /k/ whether the other language has a distinction of /k/-/k^h/ or not. The fact that he may gradually learn to make some kind of distinction after repeated correction by the speakers of the other language is not a structural but a sociocultural fact. It may be doubted whether phonemic burdening is a structural fact at all; in any case its status is in doubt as long as no one has devised a method for quantifying its function in speech. The existence of minimal pairs in the lexicon or the high statistical frequency of a given phoneme do not prove that a given contrast would lead to confusion in actual utterances.

2.6. On the other hand, this reviewer would regard the sequences (distribu-

tion) of phonemes as having a greater structural significance than they have here been given. A list of phonemes like that for Romansh and Schyzertütsch in Weinreich's tabulation is of little value in predicting interference unless it is accompanied by a rather complete statement of their distribution. The occurrence of /s/ in both English and Finnish does not enable the naive Finnish speaker to produce initial s-clusters which do not occur in his language. This is not merely, as Weinreich puts it, a 'factor inhibiting phonic interference', but an essential part of the structuring of each phoneme.

2.7. It will perhaps be perceived that the reviewer finds other information of greater importance than a list of phonemes, on the one hand a full description of the PHONETIC COMPONENTS and on the other of the PHONEMIC DISTRIBUTIONS. These are the elements which the speaker hears in the other language, which he then reinterprets and organizes in terms of the structure which he knows, namely his own. For this reason also, the reviewer does not find Weinreich's fourfold division of interference phenomena entirely satisfactory. As an example we may take the author's treatment of vowel length, which is phonemic in S, allophonic in R. The consequence is that S length is distorted in R, remaining in those positions where it is required by the following consonants, disappearing elsewhere, so that e.g. S /zi·ba/ retains its long /i/, while /a·par/ shortens its /a/ (16). This is called a 'reinterpretation of distinctions.' But it could also be stated as a change from phonemic to allophonic length, which in turn could be regarded as an 'underdifferentiation of phonemes', since the distinction between long and short phonemes disappears. The dependence of such determinations on the author's phonemic practices is apparent when we note that there is an exactly parallel development in the relation between geminate and single consonants in R. This distinction, which is significant in R, is allophonic in S, being dependent on the preceding vowels, so that S /fili/ is reproduced by R speakers as /filli/, while R /messa/ is reproduced by S speakers as /mesa/. If the long vowels had been analyzed as geminate instead of long, the exact parallel between these two phenomena would have been brought out: gemination (or length, if one chooses) is phonemic in vowels, allophonic in consonants in S, but the reverse in R; speakers of each language turn the phonemic distinction of the other into an allophonic one, thus eliminating a significant distinction of the other language as surely as when they coalesce /y/ and /i/, or /e/ and /æ/.

2.8. This detailed consideration of one section in the book is not offered as a criticism of the author, but as a contribution to the discussion of the theoretical problem involved. No matter how well one analyzes the respective phonemic systems, the behavior of bilingual speakers will not be precisely predictable. While the author has enumerated the many factors that inhibit or promote the bilingual's tendency to interference, he has made no provision for what the reviewer called 'erratic substitution', or the factor of chance which can lead to individual mishearings that are not easily accounted for by any theory.³ Nor

³ *Norwegian language in America* 394 (Philadelphia, 1953; abbr. *NLA*). Cf. Christen Møller, *Zur Methodik der Fremdwortkunde* 33 (Aarhus, 1933): 'Es gibt hier für alle Entlehnungen, die durch das Ohr erfolgen, einen inkontrollablen störenden Faktor ...'

does he mention the fact that phonemic interference can be total, so that two languages may be spoken with identical phonemic systems, e.g. Latin and English as pronounced by Englishmen.

3.1. The chapter on grammatical interference is an admirable survey of a complex and highly controversial problem. The author shows that much of the disagreement concerning the borrowing of grammatical elements is due to a lack of adequate definitions of the terms used. He therefore explicitly distinguishes between grammatical functions, which are treated in this section, and designative (semantic) functions, which are treated in the following chapter on lexical interference. He anticipates criticism by directing a barbed footnote at 'those formalistically inclined readers who cannot conceive of linguistic meaning other than distribution' and therefore are likely to find his chapter on lexical interference 'either repetitious or linguistically irrelevant' (47). The distinction is not unlike that which emerges in C. C. Fries, *Structure of English*, between 'function words' and 'parts of speech', the former being words that signal structural meanings, the latter words that refer to the non-linguistic world. In Weinreich's treatment the function words are handled together with more or less bound morphemes as well as such grammatical relationships as order, agreement and dependence, and modulations of stress and pitch. He would probably have achieved a more coherent classification if he had regarded these latter as morphemes also. As it is, he is able to show that the morphemes of one language may be identified by bilinguals with either the morphemes or the relations of another language. If relations are regarded as morphemes, this statement could be simplified to a statement of simple morphemic equivalence between languages.

3.2. Following hints advanced by earlier linguists, the author offers material in support of the thesis that the likelihood of morphemic transfer from one language to another is in inverse proportion to its structural integration. In other words, a free morpheme is more likely to be transferred than a bound one. In discussing this thesis he quite rightly criticizes an inadequate formulation by this reviewer written in 1950, which left the impression that the small percentage of interjections (1.4%) in loanword lists meant that they were less transferable than (say) nouns. This implication was not intended and has been corrected in the reviewer's book, where the statistical method now suggested by Weinreich was independently adopted.⁴ When the proportion of borrowed interjections is compared to that of the language in general, they are seen to rank very high, as one would expect from their almost complete lack of integration into the syntactic structure.

3.3. In discussing the basis for identification among morphemes, the author finds that this is either a formal or a functional (for which one could also write distributional) similarity. Formal similarity led Hungarians to turn their deverbal adjectives in *-ando* into gerundives on the model of Latin *-andus*, while functional similarity caused Uzbeks 'to equate the Russian construction *iz* plus genitive with their native partitive and to use it even where idiomatic Russian requires other prepositions' (40). In any transfer or imitation of forms

⁴ NLA 407.

it is 'the more explicit pattern' that 'serves as the model for imitation'. This accounts for the trend in pidgin languages, where the bound morphemes of one language are given up in favor of free morphemes from the other; word order as the most explicit category of all takes the place of the subtler devices of structural signalling. On those relatively rare occasions when bound morphemes are actually transferred from one language to another, they are such as fit readily into a pre-existent category in the recipient language. When English *-s* is introduced into the Welsh plural system, it is hardly more than a new allomorph of the pre-existent plural morpheme.

4.1. The chapter on lexical interference, while valuable in itself, illustrates another fundamental terminological difficulty of current linguistics. While the author guards himself by noting the parallelism between the processes demonstrated for grammatical interference and those which are now presented for lexical interference, one cannot but wish that he had somehow tried to reduce redundancy by combining these. Surely there is no difference in principle between the borrowing of English *but* in the Yiddish sentences /nit er bat ix/ 'not he but I' (30) and that of *fence* in American Norwegian, or between the reproduction of English *who* by Yiddish *ver* as a relative pronoun in the sentence /der ments ver iz do/ 'the man who is here' (30) and that of *corn* by Norwegian *korn* in the sense of 'maize'. In both cases we have bilingual identification of morphemes due to similarity of function, resulting in the first case in transfer, in the second case in reproduction. The fact that the functions are 'grammatical' in one case, 'designative' (i.e. semantic) in the other, is at least not relevant to the bilingual process. Nor is it possible to maintain any absolute rigidity of distinction between grammatical and semantic functions; for the purposes of generalized statement, the term 'distribution' can cover both. It was to avoid any limitation to a particular level of analysis that this reviewer adopted the terms 'importation' and 'substitution,' which correspond in a general way to Weinreich's 'transfer' and 'reproduction,' but are used about phonemes as well as morphemes.⁵ The reviewer also used 'reproduction', but in a more general sense, similar to that of Weinreich's 'interference'. For the results of transfer we agree in using 'loanword', but Weinreich has regrettably not found the term 'loanshift' useful as a common designation for the results of substitution. Instead he classifies them more or less along traditional lines, which distinguish between extensions of meaning and loan translations. But examples of the former (like his Yiddish *brik* 'bridge' adding the meaning of 'floor') and of the latter (like Louisiana French *marchandises sèches* for 'dry goods') have much in common: in both cases native words have altered their usage in response to a foreign model.

4.2. Whatever the terminology, no one can quarrel about the basic importance of the two mechanisms of interference here described and richly illustrated from the vast literature on loanwords. Weinreich distinguishes between their application to simple words and to compound words and phrases. By 'simple' he really means 'unanalyzed by the borrower'. He has erroneously included the American Norw. *blakkvalnot* 'black walnut' among these, since it reveals analysis

⁵ NLA 402.

by the substitution of Norw. *not* for E *nut*, and by the back-formations to which it has given rise: *blakkval* and *blakkvaltre* 'black walnut tree'.⁶ One might also question whether Amer. Italian *pizza-paia* should be classed here, since the element *pizza* is recognizable as an Italian morpheme and *paia* undoubtedly had been borrowed in Amer. Italian before the compound. This reviewer found that in practice it was often impossible to be sure whether a given sequence was or was not analyzed by the borrowers. Amer. Norw. *kråssrød* 'cross road' consists of elements which have both been borrowed separately; the word shows complete adaptation to the Norw. phonemic and grammatical pattern. It thus fits Weinreich's definition of the transferred compound in which 'all the elements may be transferred, in analyzed form', yet he would apparently class it as 'simple'. One could only make this classification for sure if one could observe the loan at the moment of original borrowing, i.e. the process of interference itself. This is one of the theoretical comments one might make on Weinreich's approach: that while he here claims to be describing interference, his examples are largely drawn from established loans, which show adjustments to previously borrowed words of the same types. This is the reason for the reviewer's adoption of the point of view that a loanword description is not to be regarded as describing the actual historical process, but as a comparison between *MODEL* and *REPLICA* which reveal the sum of all changes during the process of interference.⁷

4.3. The further discussion of lexical interference contains many acute observations on the lexical integration of loanwords (i.e. their relation to the previous stock of words) and the reasons for borrowing: they are the same as for innovations in unilingual speech, plus a need for semantic differentiation and the social values of the model language. The author candidly admits the inability of the investigator to explain the resistance of some words to importation, and he is healthily skeptical about potential homonymy as an explanation of such resistance (61). There is an interesting discussion of the total amount of interference and of the attempts that have been made to quantify and classify the impact of one language on another. A section on interference and language shift brings up the problem of whether languages can be genuinely 'mixed' in the popular sense (68). The author here seems to contradict an earlier assertion (7, quoted from Lotz) that 'every speech event belongs to a definite language'. He admits here that a language switch is possible within a single sentence or phrase, and even that the speaker himself may be uncertain which language he is using at a given moment. The reviewer has discussed this problem in *NLA* 63-9 and cited many examples from his own observations of such confusion. Speakers not only switch in the midst of utterances, but they are frequently mistaken about the linguistic membership of given items in their own speech. Often, too, the criteria which satisfy the linguist that a borrowed word has been integrated will not disguise it to the consciousness of bilingual speakers, who will continue to call an English loanword English in spite of its adapted shape. There are thus definite (but so far unstated) limitations on Meillet's assertion that 'a speaker always knows that he is using the one system

⁶ *NLA* 562 et passim.

⁷ *NLA* 388.

or the other' (69). A section on pidgin languages, labelled 'crystallization of new languages from contact', concludes the study of interference from the linguistic point of view.

5.1. The chapters entitled 'The bilingual individual' and 'Socio-cultural setting of contact' are shorter than the preceding and will occasion less comment here. It is shown that the amount of interference can be correlated to the bilingual speaker's language aptitude, his switching facility, and the status he accords each language. Such factors as relative proficiency, mode of use, order of learning and age, usefulness in communication, emotional involvement, function in social advance, and literary-cultural value may add up to a dominance configuration for one language over the other. This configuration may be shared by other speakers, resulting in various typical language functions in bilingual groups. Linguistic effects are different according to the congruence of the mother-tongue groups with various other kinds of social groupings, resulting from geographic contiguity, migration, cultural-ethnic ties, race, sex, age, status, occupation, and urban-rural distinctions. But the author warns against 'tagging two languages in contact as respectively "upper" and "lower" at any cost'. Some may be surprised to find that a special section is devoted to 'the standardized language as a symbol', but Weinreich is entirely justified in considering the relation of patois to national language an example of bilingualism. In pointing out the importance of language loyalty in relation to the standardized language, he neglects to mention that the patois can excite an equally vigorous loyalty, though perhaps of a different kind.

5.2. The final chapter, on research methods and opportunities, is brief but stimulating. It suggests the need for an approach which will combine the socio-cultural with the structural method of analysis, and the importance of such areas of multiple language contact as the Balkans and the Americas, or such languages as Yiddish, as favorable fields for the study of interference. Concerning the studies which have so far appeared the author says that they are often not comparable because of differences in techniques and orientations. 'It is consequently a major task in research planning to promote the coordination of studies in this field by drawing up general canons of description.' Since the reviewer finds himself in hearty agreement on this point, he can only add his assent and hope that the author may continue his bilingual researches along the fruitful lines here suggested.

5.3. Misprints are blessedly few; the only ones that are not self-correcting and could be disturbing to the argument are the following: 16, line 6 from bottom: for [fili] read [fil-i]; 18, line 5 from bottom: for 'consonant' read 'vowel'; 18, line 19: for 'substitution of [a] for /a/' read 'substitution of [a] for /a/'; 45, line 19: for 'yoke' read 'jug'.

Subjunctive and optative: Their origin as futures. By E. ADELAIDE HAHN. (Philological monographs, No. 16.) Pp. xviii, 157. New York: American Philological Association, 1953.

Reviewed by FRED W. HOUSEHOLDER JR., *Indiana University*

Though the central thesis of this book is not wholly new (being a modification of the view of the great Greek grammarian W. W. Goodwin), it is unfamiliar, almost paradoxical, to anyone brought up in the main tradition of Indo-European and classical grammar. The books to which most of us were chiefly exposed present the Delbrückian view that the subjunctive is in origin a mood of will (supplementing the imperative), and the optative a mood of wish; and that other uses developed later from these primary ones. Now Miss Hahn, in a stimulating book, forces us to reconsider the evidence. She assumes that Indo-European once had two true future tenses, but no modal contrasts except for the imperative; that later one of these futures became the subjunctive, another the optative, beginning in the proto-language but with the shift from tense to mood still not complete in Latin; and that Greek and Sanskrit developed new futures, in part from special varieties of the old. The main body of the book is an attempt to show that there is no evidence which convincingly refutes this assumption.

In a discussion of tense, mood, and aspect it is easy to get confused. Before going on to summarize Miss Hahn's treatment, therefore, I shall offer the tentative definitions of these categories with which I shall try to operate in this review. They do not necessarily agree with Miss Hahn's implicit definitions, but they are intended to cover approximately the same ground. All are defined with reference to the main verbs of simple non-interrogative sentences; once established, they can be extended to other situations.

Mood has to do with contrastive truth value (in the logical sense). If in a given language one set of forms, when used as predicates of simple sentences, are characteristically incompatible with their contradictories, then these forms constitute what is traditionally called a DIRECT or INDICATIVE mood. If speaker A makes a non-interrogative utterance in this form, speaker B may say *You're a liar* or *That's not so* without talking nonsense or violating any rules other than the norms of polite behavior.

Other forms may be called MODAL (a bad name if the indicative is also a mood) or OBLIQUE. There are two subtypes. (1) If no form of contradiction is possible, if not even a probability value may be assigned, we have a PURE MODAL form, of which the commonest type is the imperative. Here speaker B may say *Don't say that* or *Take it back* or *I think you're making a mistake* or *You're being too hasty* or the like, but he cannot normally say *That's true* or *That's not true*. (2) If the utterance has what may be called probability value, we speak of SEMI-MODAL forms. Here speaker B may reply *I don't think so* or *That seems unlikely* (answers which are also possible for indicative utterances), or he may use forms which occur as replies to pure modal utterances.

The semi-modal category is in many languages formally combined with the indicative, in others with the pure modal, in others partly with one and partly with the other; there is usually a dichotomy rather than a clear-cut trichotomy

(though Turkish makes a close approach to trichotomy). Semi-modal forms may refer to future (futures, potentials, necessitatives), present (some potentials and unrels), or past (unrels and quotatives), but most often to the future.

One semi-modal form may be singled out as the most neutral or objective way of referring to the future, and may then fit into the system as a future indicative. This is the case in ancient Greek, where formally the future is a defective aspect system, and may be the case in Latin, treating the future as a tense—though analysis as a mood is equally possible.

TENSE in the strict sense refers to the possibility of contrastive occurrence of an indicative form as predicate of a simple sentence in combination with adverbs which relate the time of the predicate to the moment of speaking. Forms which regularly occur with such adverbs as *yesterday* and *last year*, but not with *now* and *at present*, or with *tomorrow* and *next year*, are called PAST. It is usual to find a contrast PAST VS. NONPAST (for which it is sometimes not misleading to substitute PRESENT), or PAST VS. UNDEFINED; in theory one might also find PRESENT VS. NONPRESENT, or a three-way PRESENT, PAST, UNDEFINED (though I cannot cite any examples). In some languages the semi-modal FUTURE may be fitted into this system, though the fit is seldom perfect in IE languages. The meaning of the past category is not often limited to time, but normally includes other functions, such as unreality.

ASPECT introduces another kind of reference to time, unrelated to the moment of speaking (except so far as the form also has tense). The primary dichotomy appears to be between forms in which tense and time-when references ('dates') apply to the subject (allowing certain inferences with regard to the relation of the act to this DATUM POINT), and those in which they apply primarily to the predicate (event, action, state, etc.). We may call the first kind RELATIVE aspects and the other NONRELATIVE (not necessarily 'absolute'). The most usual dichotomies in IE languages are nonrelative perfective vs. nonperfective, as in Greek (i.e. completive, not normally 'punctual' in the ordinary sense, vs. undetermined, by implication usually relative imperfective, incomplete, progressive, or the like), and relative imperfective vs. nonrelative non-imperfective, as in English (i.e. incomplete, progressive, 'durative', with datum point in the middle of an event of restricted length, vs. implied perfective). The application to repeated or habitual action is not usually a defining property; this use apparently belongs to the undetermined aspect, which in English is otherwise generally perfective but in Greek imperfective.

Frame tests for aspect present a difficult problem. It is true that the Kahanes in a recent review¹ speak of an 'adverb' in discussing aspect, but their adverb appears to be some sort of fictitious entity which I do not fully understand. The following points, however, are fairly clear: relative forms must be accompanied by (or carry) some indication of the datum point (*when I saw him, at five o'clock*), while perfective forms need not; perfective (nonrelative) forms may be accompanied by indications of duration, length of time (*for two years, all*

¹ Review of H. Seiler, *L'aspect et le temps dans le verbe néo-grec*, in *Lg.* 30.115-23 (1954).

day), while true relative imperfectives normally are not; expressions equivalent to our *while* clauses of time normally contain imperfective forms, while expressions equivalent to our clauses introduced by *after* and *until* normally contain perfective forms.

A second aspect contrast often found in IE languages is that between the (relative) PERFECTIVE STATE ('perfect', 'stative', often 'resultative') and the NONRESULTATIVE (by implication but not necessarily nonstative or eventive). This 'stative' form, like the relative imperfective, requires a datum point, expressed or implied, marking the end of a prior period of time within which the event[s] leading to the asserted state may be placed indefinitely; but it also admits expressions of duration, like the perfective (nonrelative). The English perfect phrase will nearly always allow the addition of a time-clause introduced by *since*, whereas other forms do not often permit that; but the ancient Greek perfect cannot be similarly tested, though it will usually (like English) permit the presence of a time phrase including both duration and date (*for a long time now*) or an expression of prior time within which (*in the past five minutes*). An assertion such as the English *John Smith has written a book* may be decomposed into three assertions: *John Smith wrote a book*, *John Smith is alive*, *The book is in existence*. When no other time indication is present, in both English and Greek, it is often assumed that the past event is very recent; the first sentence above might be extended to *John Smith finished writing a book recently*.

A perfect may nearly always be paraphrased by a proposition including the verb *to be* and some predicate adjective or noun phrase (though sometimes the subject will have to be the object of the original perfect); but this is not a wholly satisfactory test either, since many other propositions may be so paraphrased (*He teaches school* = *He's a schoolteacher*).

To reinterpret Miss Hahn's thesis in terms of these definitions, she apparently maintains that at the earliest period when ancestors of the Greek (etc.) subjunctive and optative existed in Proto-Indo-European (post-Indo-Hittite), the proto-subjunctive had as its principal or only use, when not combined with modal particles, that variety of semi-modal which I have termed maximally objective, that used for confident predictions (like our *is going to*), plus, perhaps (if I understand her correctly), a slightly weaker type of semi-modal use, found in promises and statements of intention (like some uses of *will*). At this time the optative, in her view, was a still weaker ('more remote') kind of semi-modal, a sort of potential (equivalent in some respects to our *may* and *would*, though never unreal). The later use which is called the subjunctive of will did not yet exist; nor did the later optative of wish.

Her presentation of these views is partially obscured by the reiterated assertion that the two proto-forms were 'future tenses and nothing more' or 'future tenses and nothing else'. Obviously no future tense can be that 'and nothing more', since the future is not a realm of undeniable fact. Any form (used as a main verb) referring to the future must imply a prediction, a promise, a probable guess, a hope, a reasoned expectation, an order, an intention, or some similarly colored assertion. Miss Hahn almost defeats her purpose by

insisting on such sweeping statements; but see her explanation in §§101-2 (74-5).

As corroborative evidence for her assumed ORIGINAL MEANING, she offers her analysis of the GENERAL MEANING (*Gesamtbedeutung*, common to all occurrences) in several of the daughter languages. She seems to take as axioms, at least part of the time, these two propositions: (1) no meaning that cannot be detected in every single occurrence of a linguistic form is 'really present' in any instance of that form—i.e. the only meaning is the *Gesamtbedeutung*, everything else is mere inference; and (2) the original meaning must be the ancestor of the various general meanings in the daughter languages. She violates these principles, of course, when she allows an IE distinction between proto-subjunctive and proto-optative, since elsewhere 'futurity and nothing else' is the only universal meaning she is able to establish for the Greek or Vedic optative, or for the Latin subjunctive, which in her view invariably continues an IE optative or a form used interchangeably with an optative. She sometimes attributes to the Greek optative, apparently as part of its general meaning, the notion of 'more remote', under which she includes 'less probable' or 'less vivid', 'further in the future', and 'shifted to the past'.

A corollary of her first axiom, which explains the fact that she rarely gives data on frequency, is that one counter-instance is sufficient to refute any generalization. At the beginning of Chapter 6 on the moods in Homer, for instance, she lists six ways available to Homer for the expression of futurity, with one example of each in independent clauses. The casual reader might suppose that all six were more or less equally common; but see her notes 175-7. In fact, the approximate frequencies in Homer are: future indicative alone, over 1000 (probably over 2000); future indicative with *ke* or *án*, about 20; subjunctive alone, 1 without *ou*, 6 with *ou*; subjunctive with *ke* or *án*, about 30 (20 being first person singular); optative alone, about 10 (most being debatable, two with *ou*); optative with *án* or *ke*, about 15 possibly nonpotential, of which 12 are first person singular (there are also about 450 normal potential optatives—'would, may, might', etc.).² Peculiar items in a traditional text may, of course, be archaisms, but they may also be innovations, blunders (pseudo-archaisms),

² I give here some more statistics, relevant to statements summarized on p. 145 (§§214-7) from Chapter 6, §§137-53. 'The sharp distinction between the potential [optative] ... and the optative of wish ... is not ... Homeric': wishes with *ke* or *án*, 2; wishes without these, 180; potential with *ke* or *án*, 440; potential without these, 9. — 'The optative is used to express ... will rather than wish': optative of will (often debatable), 24; subjunctive, 262. — 'An independent *mē* clause, expressing fear ... takes either the future or the subjunctive': first verb after *mē* future, none; first verb subjunctive, 36. — 'In some or all of these [purpose or object clauses] we again find the future, subjunctive, and optative (both with and without *án*) used in parallel ways': *hina* with subjunctive (always without *án*), 95; optative, 58; future, none; *ópōra* with subjunctive, 139 (12 with *án*); optative, 36; future, 5. — '[In] "fearing" clauses ... with *mē* [and] "hoping" clauses ... with *ei* ... we again find the future, subjunctive, and optative (both with and without *án*) used in parallel ways': *mē* with subjunctive (never with *án*), 99; with optative (never with *án*), 41; with future, 2; *ei* with subjunctive (always with *án*), 19; with optative (never with *án*), 35; with future, 1. These figures are no doubt subject to some correction for errors of observation, but probably not of an order greater than plus or minus 3 in any one case.

or simply errors of transmission (corruptions in the manuscript tradition). Miss Hahn takes the position that all are archaisms.

A real methodological problem exists here, however: how can cases of contrast be distinguished from cases of free variation in a text or corpus, in situations where no informant is available? Miss Hahn appears to postulate that two forms (specifically morphemes attached to verbs) are in free variation if they occur in parallel coordinate clauses (e.g. the modals in *He will come ... and he may stay*), unless some other feature of the context strongly supports the hypothesis of contrast. This view makes frequency irrelevant. I should like to cite here what is often called the redundancy principle (one aspect of which was offered by Martin Joos, in his talk to the Linguistic Society in December 1954, as a 'first theorem of semantics'), and then add a corollary. The principle may be stated in several forms; a brief one is: Minimal pairs are abnormal in natural languages. The most striking formulation, perhaps, and the one which I prefer, is this: If in a given text it can be shown that two forms belonging to the same small form-class are in complementary distribution in the majority or in all of their occurrences (considered not with reference to other morphemes of the same minimum phrase but to a context of sufficient length), then, by definition, the two forms contrast in meaning. For any IE language 'sufficient length' is arbitrarily taken to mean the immediate phrase (containing a verb form) and the preceding and following phrases (each containing a verb form). The corollary then is: Two forms of the same class in a given text may be equivalent in meaning if and only if they are found to be in complementary distribution, with respect to contexts of sufficient length, in less than half or in none of their occurrences. This does not, of course, exclude the possibility of morpheme alternants determined by complementary distribution with reference to the word or immediate phrase only.

If these definitions are accepted, none of the forms equated by Miss Hahn are semantically equivalent. All that her evidence is capable of showing is that some of the forms do in fact belong to the same small form-class. Her approach makes it abundantly clear (if anyone doubted the matter before) that the most hopeful approach to meaning is through analysis of contrasts, rather than exclusively of similarities.

A second corollary is, that in attempting to formulate the 'meaning' of the contrast, or the 'meaning' of any form entering a contrast, only those occurrences are usable which are in complementary distribution.

Before summarizing Miss Hahn's challenging book, I offer a few selected quotations, so that readers unfamiliar with her work may be better able—while bearing in mind the treacherous nature of quotation out of context—to form an idea of her style and method of presentation. The passages are presented in the order in which they occur in the book, preceded by references to page and section or note.

3 n. 5: The quite different type of duality played by the perfect as belonging to two separate temporal spheres, present and past, is of course due to the fact that it represents an amalgamation of two wholly different tenses, the one a preterite or past punctual, and the other a perfect or present perfective.

9 §10: The ... tendency in both Greek and Latin to use the subjunctive ... in generalizations ... can be accounted for, it seems to me, precisely on the assumption that these subjunctives were originally futures, and in no other way whatever.

26 n. 41: The force a verb bears when its clause stands alone is carried over when the clause becomes a part of a condition, whether as protasis or as apodasis.

59 §85: There were two of these [sc. new Indo-European formations], and I believe both originally denoted futurity and nothing else.

75 §103: Thus in English ... *will*, expressing volition, and *shall*, expressing obligation, have become merely signs of simple futurity, almost completely interchangeable in most cases ...

76 §104: Thus in Vedic Sanskrit the subjunctive and the optative got mixed and interchanged with each other as well as with many other forms such as the imperative ...

93 §127: The Greek optative in this use [sc. unreal apodosis] does not need a particle ... but ... has it ... without exception.

118 n. 292: The point is that all these subjunctives have in common the one essential detail that they refer to the future ..., and the precise category of meaning into which we elect to fit each of them is relatively unimportant.

147 §224: But none the less we find, so long as we steadfastly view both subjunctive and optative as originally and fundamentally tenses rather than moods, that a certain pattern ... does emerge and prevail.

149 §229: But it is significant that—at least according to my theory—*neither* of these uses is really the *fundamental* one of the mood with which it is associated.

The following is an attempt to summarize, as briefly as possible, the contents of Miss Hahn's book.

Part I (Chapters 1-3) discusses the views of earlier scholars, with reference to the Greek forms (1), Latin (2), and certain assumed IE forms with uses similar to subjunctive or optative (3). Chapter 1 introduces her thesis, attributing the prestige of the prevailing view (subjunctive a mood of will, optative of wish) to Delbrück, and arguing against the unscientific character of what she calls the metaphysical approach to defining the moods and against the psychological approach, as of W. G. Hale. By the former she means the use of terms such as subjective, objective, conditioned, possible, conceptual; by the latter, the use of terms like memory, perception, feeling, emotion. Her own approach she leaves uncharacterized, except that it is scientific and deals 'exclusively with language itself'. She singles out W. W. Goodwin as her own predecessor (giving partial approval also to Karl Hammerschmidt, Adolf Walter, and Eduard Schwyzer), and summarizes the views of various Greek grammarians up to 1950. Chapter 2 gives a similar summary of the views of Latin scholars, partially approving E. P. Morris and S. A. Handford, but again attacking Delbrück, and in part E. A. Sonnenschein, Kroll, and J. B. Hofmann. Chapter 3 summarizes earlier views on the so-called injunctive (modally used augmentless past) and the Latin subjunctive in *-ā-*, and gives her own views: (1) that the injunctive is not a mood, and should not be called by that name (but simply 'augmentless past indicative'), that its prohibitive use is due to the punctual aspect of certain forms, and that the other modal uses result partly from the prohibitive use, partly from a confusion with the optative in 'past iterative or generalizing expressions', thence extended to other parts of the optative domain;

(2) that the *-ā-* subjunctive is an old augmentless past (aorist) indicative formation, later misinterpreted by the Romans in the same manner as the so-called Sanskrit 'injunctive'. She suggests that Sanskrit subjunctives with secondary endings also come from this *-ā-* indicative. Nonclassical linguists will be interested to know that Trubetzkoy once dealt with this topic, proposing that the *-ā-* morpheme was in Indo-European merely an alternant of the ordinary optative morpheme *-yē- ~ -ī-*: a theory rejected by Miss Hahn, but possibly resuscitable in terms of some early conditioned alternation between *y* and a laryngeal.

Part II deals with mood in Indo-Hittite and Indo-European. Chapter 4 discusses the Hittite imperative, which has, apparently as an innovation, a first person singular, but no first person plural attested, and the modal particles *man* (1. 'if'; 2. with the present in ideal or potential, with the preterit in unreal protases and apodoses) and *le* (prohibitive negative). She concludes that Indo-Hittite also had modal particles: one equivalent to Hittite *man* and Greek *án*, one equivalent to Hittite *le*, Greek *mé*, and Sanskrit *mā*, and probably others, including one—perhaps *ne*—equivalent to Hittite *natta*, Greek *ou*, Sanskrit *na*, Latin *nōn*, which Miss Hahn does not consider modal, but which I would regard as a particle of the direct mood, contradicting a predicate. Except for the imperative, and possibly intonation or word order, Indo-Hittite had no other modal devices. Chapter 5 discusses the IE situation. She believes that the earliest proto-subjunctive (future) was the imperfective or 'iterative-durative' subjunctive; that the need for a 'punctual' future was filled by making an analogous form from the *s*-aorist, which is the ancestor of all *s*-futures in Greek, Italic, Sanskrit, and perhaps Celtic, as well as of the aorist subjunctives of Greek and Indic, and the *s*-subjunctives in Italic and Celtic; and that the earliest proto-optative was imperfective, the aorist (at least the *s*-aorist) optative coming later, perhaps after the break-up of Indo-European. The Latin subjunctive in *-sim*, the perfect subjunctive, the imperfect subjunctive, and the pluperfect subjunctive are all such *s*-aorist optatives. Early Latin had three futures, two of them 'iterative-durative' (*-ē-* and *-b-*), one 'punctual' (*-s-*), all from IE proto-subjunctives. The proto-subjunctive was used to make assertions about the very near future (say 'tomorrow'), the proto-optative about the more distant future (say 'next year'). These were the proto-meanings, all others being later. They were not clearly distinguished in IE, but were constantly confused and substituted for one another and for other forms such as the imperative. The distinction between the *s*-future (very immediate and vivid, punctual) and the *s*-subjunctive (less immediate and vivid, punctual) was already present in IE, but these forms were also frequently confused. Possible shades of meaning in the future are numerous, and easily confused. For this reason the proto-subjunctive entirely disappeared from most branches of IE, gave only future indicatives in Italic, almost disappeared in Sanskrit, and became clearly distinct in Greek only at a late date.

In Part III Miss Hahn turns to a detailed examination of crucial passages in Homer (Chapter 6) and Early Latin, mainly Plautus (Chapter 7). The general purpose of the argument in Chapter 6 is to show (a) that *ke* or *án* is meaningless in combination with optative or subjunctive; (b) that there are subjunctives

without any element of will, optatives without any element of wish, and that therefore these meanings are never in fact conveyed by the forms, but only by inference from the context, with either sense attaching to either form; (c) that there is no difference in meaning between future indicative and subjunctive, and that the great majority of all futures are indistinguishable in form from aorist subjunctives; (d) that, though there is often no difference between subjunctive and optative, we may still say that for Homer the optative has a tendency to represent a more remote or less vivid future. Incidentally she gives many fine intuitive interpretations of Homeric passages, discusses the expression of unreal propositions and wishes (where *ke* or *dn* is meaningful, the optative an innovation), and examines the development of the optative in secondary sequence, which proves that it is a tense, not a mood.

Chapter 7 contains much the same kind of analysis of Plautine passages, showing (a) the interchangeability of future indicative and present subjunctive, and (b) in particular the future context for so-called 'subjunctive by attraction' and 'subjunctive in subordinate clauses in indirect discourse'. Chapter 8 is a summary of the first seven, somewhat more detailed than I have given. Chapter 9 restates her original hypothesis in the form of a conclusion.

Now, first, is the question asked by Miss Hahn one to which a meaningful answer can be given? She appears to be asking, What were the earliest uses of the proto-subjunctive and proto-optative, and what were their most frequent uses? (I take 'fundamental' to include both of these points.) It certainly does seem possible, by comparing Homeric and Vedic uses, to arrive at a list of uses hypothetically present in the parent language; but this would be the latest stage of IE. Going further must be based either on what seems to be a 'plausible' or 'logical' development, or on comparison of similar developments in other languages. Miss Hahn relies exclusively on the former type of argument, without making even the Homeric-Vedic comparison. Instead, she separately establishes a *Gesamtbedeutung* for Homer and suggests complete confusion for Vedic, then pushes back her *Gesamtbedeutung* into the parent language.

Let us here briefly supply the comparison which she does not make—but which has, of course, been made before. The Homeric and Vedic subjunctives have in common the following uses. (A) In independent sentences, the subjunctive occurs with (1) first person singular in statements of future intention, exhortations, and so-called deliberative questions with a question word; (2) first person plural and dual similarly, but with hortatory as the commonest use; (3) third person in deliberative questions (with a question word), in sweeping general denials, i.e. denials containing words like 'never' and 'no one', with the truth-value negative particle, and in indefinite or immediate future statements, particularly when closely connected to a preceding verb of different form. It may be accompanied by the adverb **nu* or **nūn* 'now', but not apparently by references to a definite point of future time. It is likely, but not certain, that the second person also had some of the uses attributed here to the third person, and perhaps, in echoed questions ('Are you to go? Is that what you wonder?'), some of those attributed to the first person. (B) In dependent sentences (often paratactic in form), the subjunctive occurs with all persons alike: (1) as indirect

representative of an imperative; (2) in purpose clauses, including relative clauses of purpose, particularly if dependent on a first-person form or an imperative; (3) in general (including future general) conditions and relative and temporal clauses, especially with imperative or subjunctive in the main clause.

This pattern certainly does not look like that of an ordinary 'future tense and nothing more'. Conspicuous is the absence of unconditioned particular propositions referring to a definite future time, like *The train will arrive at 8 o'clock*. Homeric and Vedic agree in using other forms in such sentences, among them those traditionally called future indicative. The Vedic 'injunctive' (augmentless aorist or imperfect when not used simply as a past or present indicative) shares most of the above-mentioned subjunctive uses, but (a) not imperative or hortatory in the first person positive, (b) often imperative in all other forms, (c) regularly prohibitive with *mā* (a use attached in Greek to the aorist subjunctive and all first-person subjunctives).

In all these uses the aorist subjunctives far outnumber the imperfectives in Homer; in Vedic only the root-aorist active subjunctives are especially frequent, though aorist 'injunctives' outnumber imperfects.

I think most linguists would agree that this evidence suggests a modal form; if any tense value is to be assigned it would be present or non-past rather than future. The frequency of aorists suggests that possibly the form originated in the proto-aorist, specifically perhaps proto-root-aorist, and was at the outset a perfective present indicative. The analogy of Slavic and of such modern Indo-Iranian languages as Hindi and Pashtu shows the possibility of this development. The English simple present is also parallel in some ways, regardless of the number of cases where it has replaced an earlier distinct 'subjunctive' form. The 'injunctive' is perhaps merely an earlier aspectless or perfective non-past, dating from a time before the development of primary endings, which was only partly superseded by the new formation in Indic, completely so in Greek. Delbrück's observation (Hahn n. 64) about the direct contrast and mutual exclusiveness of **e* (augment) and **mé* (prohibitive) seems sound; the augment is perhaps originally an indicative modal ('It is a fact') particle, whence the natural development to past meaning.

Optatives in Homeric and Vedic share the following uses. (A) In independent sentences, the optative occurs with (1) first and second person in hopes or possible wishes, chiefly in prayers to gods; (2) third person also in hopes or wishes, and in indefinite or general indications of desirability or will ('any such person should', 'someone had better'), and as a conditioned potential, most frequently with the condition expressed. With question words and with the direct modal negative **ne*, all persons may have the potential force. Negative wishes (or hopes) take *na* in Vedic, *mé* in Homeric, but occur in all persons; Greek is generally taken to be the innovator here. (B) In subordinate clauses, the optative occurs (1) in general conditions, usually unreal, and relative clauses if the main verb is optative; (2) in purpose clauses, including relative clauses of purpose, where the purpose is indicated as a hope rather than an intention, most often when the main verb is optative.

The big majority of Homeric uses seem to be later developments, though the

agreement on unreal conditions suggests (contrary to Miss Hahn) that this occurred in late IE. Again aorists predominate in Homer, though not in Vedic, where, however, the number of root-aorist forms is high. Again we should suppose an aorist or (in view of the secondary endings) aspectless origin. If it is in origin a special proto-aorist formation (augmentless, i.e. 'injunctive'), perhaps containing the root meaning 'go', as Miss Hahn supposes, it must antedate the contrast between **e* and **mē* which occurs with ordinary aorists, but not with this—unless Greek is more archaic than Vedic in using *mē* with the optative. The lack of primary endings merely gives a clue to the date; it is presumably older than the subjunctive, but younger than the 'injunctive'.

The common pattern of Homeric and Vedic again looks modal, not temporal, and Miss Hahn may be right in assigning priority to the potential rather than the wish use, though more probably both are original. The same two uses, besides some apparently neutral future uses, can be observed in the Old Turkish forms in *-gay* etc., often called optatives, from their earliest occurrences, as well as in their modern descendants, e.g. Azerbaijani forms in *-[y]a-*. The greater approximation of subjunctives and optatives to futures in the first person singular noted by Miss Hahn in Homer and Plautus is also paralleled in the Turkic use of first person singular imperatives where we might use 'will' in English; and the Hittite forms cited by Miss Hahn (n. 93) are again similar. When speaker and subject are the same, will or intention largely coincides with prediction or expectation, except with verbs indicating events partially beyond the subject's control, where hope or wish is more likely; cf. Macdonell, *Vedic grammar* 352, 215C.

As for Miss Hahn's theory that Greek *s*-futures are all in origin aorist proto-subjunctives, the objections which she dismisses in note 115 seem to me pretty strong. Consider that the twelve most frequent futures in Homer are *es(s)o-* 'be', which has no corresponding aorist, *ereo-* 'say', *dōso-* 'give', *hexo-* or *schēso-* 'have', *eleuso-* 'come', *thēso-* 'put', *opso-* 'see', *teleo-* 'accomplish', *oiso-* 'carry', *peiso-* 'persuade', *hēso-* 'cast', and *axo-* 'bring'. In none of these is the normal corresponding aorist formed with the same stem as the future. In one case (*teles[s]a-*) the normal aorist is an *s*-aorist, but does not correspond to the future, though there is one occurrence of a future *telesso-*. In one case (*peisa-*) there are four instances of a sigmatic aorist, against 64 of the normal *pepitho-*. In two cases (*axo-* and *oiso-*) there are a few instances (9 and 13 respectively, mainly imperatives and infinitives) of what seem to be sigmatic THEMATIC aorists as against the normal aorists (*agago-* 105, *eneiko-eneika-* 42). It looks as if the aorists are induced by the futures here, not vice versa. Much more plausible is that these *s*-futures are originally a derivative conjugation, perhaps desiderative, whose spread and grammaticalization were hastened by the partial coincidence of their forms with aorist subjunctives.

Miss Hahn has made no attempt to compare remoter languages in support of her proposed semantic analyses, but a few further facts might be mentioned in this connection. (1) Use of the same form in wishes and in the protases of conditions is found in Hindi, Brahui, Turkic, and Pashtu. (2) Use of the same form as potential (and in conditional apodoses) and for wishes, particularly in

the unreal case, is found in Kutenai, Brahui, and Hindi. (3) The combination of two morphemes, one future, the other past, for potential forms is very widespread. (4) Association of past-tense morphemes with the unreal (regardless of time, present or past) is also very widespread, in spite of Miss Hahn's skepticism in n. 97, n. 209, and elsewhere, where she attempts to relate the unreal meaning to aspect or to particles, rather than to past tense. (5) Use of the same form for volitive-imperative and for conditional protases (either future or general) is not unique either, being found in Acooli and Pashtu. In short, her reliance on logic or a-priori probabilities in semantic reconstruction seems to be unjustified. Unfortunately we do not yet know the histories of enough languages through a long enough time span to say (for instance) whether the shift from wish to potential is as common as the shift from potential to wish, but there seems to be little doubt that both shifts have occurred. It is perhaps wiser to be skeptical than dogmatic in such cases. The only relevant shift of which I am unable to find any clear example is the shift from a maximally neutral future indicative to a primarily volitive modal form; but I will not be surprised if an instance turns up. Miss Hahn's assumption may be true, but, without new documentary evidence, it can never be proved.

To return now to the general issue raised by this book. Is it profitable to ask such questions about 'original meaning'? The answer seems to be that it does not directly contribute to the advancement of knowledge. Even accepting Miss Hahn's data as being all the data, a competent linguist may freely disagree with her or with me or with anyone else who attempts to answer such a question. But to the extent that such speculation stimulates workers to reopen closed issues and re-examine their presuppositions and prejudices, it may be very profitable. In that sense, Miss Hahn's book may well prove a beneficent gadfly to many more linguists.

Indogermanisches etymologisches Wörterbuch. By JULIUS POKORNY. Fascicle 8, *lek- to neus-*, pp. 672-768. Bern: A. Francke AG. Verlag, 1954.

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Unless the number of Indo-European 'roots' beginning with *n* to *y* (I suppose there were none that began with *x*, *y*, or *z*) is less than the number of such words in Indo-European languages, it does not seem likely that two more parts will complete the work, as still stated on the paper cover of this eighth part, for a complete index was announced in 1950 with the publication of the fourth part.

Everyone knows that Latin *locusta* has the meaning 'lobster' more frequently than 'grasshopper' (which should therefore come second, 673). Fr. *langouste* (in some dialects) has both meanings, and also Greek *κάραβος*, whence Liddell and Scott mistranslate *κηραφίς*, which occurs only once, 'locust' in a context where the meaning can be nothing but 'lobster'. As for the famous lady poisoner *Locusta*, the *ō* of her name (Juvenal 1.171) appears to be genuine, and not merely poetic license. She came from Gaul, where there was also a variant **lacusta* that is best explained in connexion with the large number of cases of

o : a recorded in Gaul (positional variants?), not restricted to *yo* : *ya* (Pedersen, *VKG* 1.34). The twofold meaning perhaps comes from an older 'jointed' (of the limbs) and not from any supposed similarity of lobsters and locusts. In any event Pokorny properly ignores Osthoff's etymology, which compared Gothic *bluahan*, OE *flēon* 'flee'.

Pokorny suggests (678) that Greek *λέπας* 'stone' and *λεπάς* 'limpet' may be related, but such a relationship would bring in the Greek *δέπας* 'beaker', *δέπαστρον* 'pitcher, dipper' and Latin *lepista* 'wine cup', which brings up the old problem of *l* : *d*. Since his root 3. *lep-* 'stone, rock' is set up solely to accommodate *λέπας* and Latin *lapis* 'stone' (with the Umbrian forms in *u-*) I think his suggestion plausible, whatever the Greek forms in *δ-* may mean (*λοπάς* beside *λέπας* also occurs).

684 s.v. *leudh-*: add Venetic *louzera*, a divine name 'Libera'. 686 s.v. *leug-*: for *Τεργεστε* read *Τεργέστε*, since Strabo is quoted, whatever his testimony may be worth; in view of the modern form he was probably correct in this case. This *leug-* Pokorny rightly connects with *leu-* (681), and Zeuss was, I believe, correct in seeing it in *Lutetia* (Parisiorum), for that ancient fortification, on the Île de la Cité, is described by Caesar (*BG* 7.57.4) as surrounded by swamps (*λοίγεον* 'swamp' Strabo 7.5.2, 214 C). I had made this suggestion (in *DAG* 484) independently before I discovered (from P-W 13A, 2097) that it had been proposed by Zeuss. The underlying meaning appears to be 'black' not only in *λοίγεον* 'swamp', but also in *λοῦγος* 'raven' and Ir. *luch* 'mouse', on which cf. Buck, *Synonyms* 3.63 (182). It is, however, inconceivable that *lucius* 'pike', if that also is 'black' (Pokorny 689), comes from *leuk-* 'light'. The word is known only from Ausonius (*Mos.* 120) and later writers (Anthimus, Polemius Silvius, Ruodlieb). Walde-Pokorny 2.411 make it Celtic, but Walde-Hofmann 1.825 deny this. The same fish was called *lupus* in Narbonensis, and we have a personal name *Leuponus* in Lugdunensis. The fact is that the contrast *p* : *q* (and also *b* : *g*) is more marked in Gaul than is commonly realized, but not at all distributed in such a way as to accord with the usual notion of dialect—the variants are scattered indiscriminately all over the map. It is an extreme case of co-existent phonematic systems, which regularly appears just as a language is dying out.

691: *lino-* 'flax, linen', which Walde-Hofmann insist is non-Indo-European, like *lepus* 'hare', is admitted by Pokorny; *lepus* (I think) is not. Perhaps there is no inconsistency. Flax was cultivated in Gaul, where there was an active trade in linen. The fragment of Plautus quoted by Isidore, *Etym.* 19.23.3, reads *linnam* ('cloak, mantle') *operatast tatrix Gallica* (Loewe), and stamps *linna* as Gaulish; it is commonly connected with Latin *pellis* 'hide', but hides are not woven (*tatrix*), and the alternation long vowel + consonant : short vowel + geminate is common all over Gaul.

692: The distinction between *lautus* and *lōtus* is 'elegant' and 'scrubbed'. Pokorny seems to take it for granted that *a* in *lauo* beside Greek *λῶω*, *λοβῶω* is res indicata. 695: On *mattus* see *Lg.* 25.391 (1949): not 'drunk' but 'dizzy'. 697 s.v. *magh-*: Cf. perhaps Venetic *maxellon*? 700 s.v. *māter-*: add East Italic *materešo*. As for *māterišs*, its meaning is 'stuff, matter' (as in Lucretius), es-

pecially 'building timber' contrasted with *lignum* 'firewood', which favors Osthoff's derivation (**dmā-ter-ǵə*). 708 s.v. *meġ(h)*:- The Greek forms in *μη-* (explained by Hammerich, *Laryngeal before sonant* 16, as *meġh-*) are passed over in silence, and also the discussion of *mactus* by Rose, Skutsch, and Palmer, CQ 32.57 and 220 (1937). Greek *ἀμέλω* (722) suggests that a laryngeal may be the clue to a solution of the difficult Germanic forms (Gothic *miluks*, OE *mioluc*). 723: Rhys Carpenter, *Folk-tale, fiction and saga* 124 (1946), has shown that the Thracian divine name Βελεῖς is that of a bear-god, and following Kretschmer's showing (*Einleitung* 236) of *μ : β* in Thracian, interprets Βελεῖς as *μελ-*, so that *Beleizis* would be the 'honey-eater'. This same deity was also called *Zalmoxis* (*Sa-*), of which a variant is *Zamolxis*; but I do not think we have either 'honey' or 'milk' here, for *Zalmoxis* is actually better attested and *ζαλμός* is given as a Thracian word for 'bear-skin' (cf. Skt. *śārman* 'hide'). The series of animal-names which Pokorny lists s.v. *mend-* (729) must certainly go with the *mand-* at 699; in fact Pokorny himself gives the cross reference in the former place, but it should have been added also at 729. *Menzana*, the epithet of Jupiter among the Sallentini, is admittedly 'the horse-god' (so Pokorny), and perhaps (as Skutsch suggests to me) *Messius* (with *-ss-* from *-ns-*, *-ndī-*) at Horace, *Serm.* 1.5.52, whom Sarmmentus (ib. 56) compared with a wild horse, contains the same element.

731: It has never been shown that *mangō* represents a supposed Greek **μάγων*. According to CGL 5.620.8 *mangō* meant 'gallodromus', and according to Su[il]das *μαγγάνα* was an Italic name for 'wine cask'. A dedication to Iuppiter Poeninus of the Great St. Bernhard (*NdSc.* 1892.68), inscriptions from Brescia and the Benacenses in North Italy and from Cologne show that the word *mangō* was current in Cisalpina and in Germania Inferior, *Mangius* and *Mangandius* are personal names in Narbonensis and Germania Superior, OE *mangere* 'merchant' (now *fish-*, *cheese-*, *iron-monger*) and *mangian* 'to sell' all throw doubt on the wild etymology of the dictionaries that dies so hard, the wilder the harder, of a difficult word. 735 s.v. *mer-* 'die': Here the Latin *mortuus* is explained as conflation of *mṛ-ṷó-* and *mór-to*, which is likely enough; hence the meaning, constant for example in Lucretius, not 'dead', but 'destined to die, mortal'. 763 s.v. *neku-t-*: Pokorny naturally puts Hittite *neku-* and *ne-ku-uz* here, with Latin *nox* 'night', not with *nūdus* 'naked', which is too absurd. 764 s.v. *nepōt-*: What is (*δηση*) after *νέποδες*? Can it be a garbled reference to *Od.* 4.404?

Hethitisches Wörterbuch: Kurzgefasste kritische Sammlung der Deutungen hethitischer Wörter. By JOHANNES FRIEDRICH. (Indogermanische Bibliothek; 2. Reihe, Wörterbücher.) Pp. 16, 344. Heidelberg: Carl Winter, Universitätsverlag, 1952-54.

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For many years students of Hittite who wanted to read the original texts had to content themselves with Sturtevant's *Hittite glossary*.¹ It was the only

¹ E. H. Sturtevant, *A Hittite glossary*² (1936); *Supplement to A Hittite glossary* (1939).

tool—and a very useful one—for finding the meaning of words not known to them. However, the *Glossary* had its limitations; it was never meant to be a dictionary, but simply an index to Hittitological literature; it led to places where—in commentaries or periodical articles—certain words were discussed. Friedrich, one of those who have worked in the Hittite field from the very beginning, now gives us in his *Helitisches Wörterbuch* a more complex tool, which is up-to-date and leads a step closer to a final dictionary. It still does not give the full vocabulary of the language as it is known to us, since, like Sturtevant, Friedrich limits himself to words of which some treatment has been offered either by himself or by others. However, it adds to each lemma, besides the pertinent literature, also such inflectional forms as are actually citable from the texts, and in particular the 'deviating' and rare ones. The grade of certainty which individual interpretations can claim is expressed by the way in which the lemma is printed (spaced or unspaced) and also by the use of question marks. At the end of many articles cognates from other Anatolian languages are cited, and in clear cases etymological notes are added.

I have dealt elsewhere² with the philological side of Friedrich's work, which is of very high quality and has further benefited from the advice of scholars like H. Otten, H. G. Güterbock, and, indirectly, B. Landsberger (particularly noticeable in the Akkadian part). Convinced that the readers of *LANGUAGE* will be especially interested in the problem of how much inherited material the Hittite vocabulary contains, let me concentrate here on the etymological notes.

It is well known that really good etymologies are few and far between. But they have steadily increased. No doubt they will further increase when the readings of common ideograms like 'brother', 'sister', 'slave', 'ox, cow', 'sheep', 'goat', 'bread', which so far escape us, become known. In this respect the establishment of equations like GIŠ 'wood, tree' = *daru-*, LĀL 'honey' = *melit-*, GIR 'foot' = *pat(a)-*³ augurs well for the future. In the present book, to my knowledge, the following etymologies appear for the first time: *hartagga-* 'bear(?)': Gk. *ἄρκτος* (I heard it first from W. Drohla); *hulana-* 'wool', Luw. **hulani-* : **wlna-*; both are dependent on the correctness of the interpretation. Sommer contributed the following combinations: *iya-* middle 'to go': Skt. *yā-mi* 'I go' (80) (see already Hrozný, *Sprache der Hethiter* 152 f.); *karp-* 'lift': Skt. *grabh-* 'seize'? (101); *kartā(i)-* 'cut off': Skt. *kart-* (103); *peruna-* 'rock': Skt. *parvata-* 'mountain' with reference to *pirwa-*.⁴

It stands to reason that archaic formations should yield good etymologies. Such formations are, among the nouns, the *r/n*-stems (e.g. *watar*, g. *wetenaš* 'water'), the nouns in *-an*, mostly denominative (e.g. *linkan* 'oath', cf. *li(n)k-* 'swear an oath'), and, among the verbs, those with ablaut (e.g. *kuen-* 'hit, kill', pr. pl. 3 *kunanzi*) and those with infixed *n* (e.g. *harni(n)k-* 'perdere': *hark-* 'perire').

² In a review to be published in *JAOS*.

³ This last one was only recently established by H. Otten, *ZA* NF 16.230 f. (1952).

⁴ For the latter see also a forthcoming article on 'Cappadocian' proper names of Neshite character.

I venture to offer here some etymologies—most of them new—which seem to me attractive.⁵

ḥappar, g. *ḥapparaš* 'business, purchasing price': Lat. *ops*, *opis* 'possessions, fortune'.⁶

kallar, g. *kallaraš* 'awesome, monstrous': Lat. *calvor* 'to scheme, deceive', Gk. *κηλέω* 'charm, bewitch'.

wešur- to be inferred from *wešuriya-* middle 'waste away': OIcel. *visna*, OHG *wēsanēn* 'wither'.

partawar, g. *partaunaš* 'wing' belongs to a verb **parta-* (so far not citable) the relationship of which to **per-* in OCS *paritb* 'fly'—is analogous to that which prevails between **ker-* 'cut' (Gk. *κείρω*) and **kert-* (Skt. *krntāti*, Hitt. *karta(i)-* 'cut off').

kuššan, g. *kuš(ša)naš* 'hire': OE *hȳr*, MHG *hûr* 'hire' < **kūsā-*; cf. *kušata* neuter pl. 'bride price', Goth. *huzd*, OHG *hort* 'hoard' < **kuzdho-* < **kusdho-*; cf. Goth. *mizdo*, OE *meord* 'wages' < **mizdho-* < **misdho-*. In Hittite one would expect, besides the noun, a verb *kuš-*; so far only middle pret. sg. 1 is citable (*Ḫatt.* iii 24), which seems to mean 'I repaid'.

ḥuek-, *ḥuk-* 'slaughter (animals)': Goth. *weihañ* 'fight', OIcel. *vegandi* 'killer'; Lat. *vincō* 'gain the victory', *victima* 'sacrificial animal'; cf. Hitt. *ḥunink-* 'damage, injure'.⁷

ištark- 'fall ill', *ištarnink-* 'make sick': OIcel. *starkr*, *sterkr*, OE *stearc* 'stiff, stark, strong' (< **starku-*⁸); Goth. *gastaúrknan* 'dry up'; Lat. *consternāre*.

**manink-* to be inferred from *maninkuwant-* 'close, short': OHG *mangōn* 'lack', MHG *manc* 'deficiency'; Lat. *mancus* 'bodily deficient'; Gmc. **magro-* (OIcel. *magr*, OE *mæger*, OHG *magar*).

maru-š-, *mum(m)iya-* 'drop, fall': Lat. *moveō* 'move'.

**šešar-*⁹ to be inferred from *šešariya-* 'strain, filter' and *šešarul* 'strainer': Skt. *śisarti*, *sarati* 'flow, run'; Lat. *serum* 'whey'. Compare OIcel. *śia*, OE *sēon*, OHG *sihan* 'strain, filter' with OE *siġan*, OHG *siġen* 'flow down'.

duwarna- 'break to pieces': Skt. *dhvāratī* 'harm, destroy'.

pašila- 'pebble', probably an original *l*-stem like *šuwil* 'cord, thread': Gk. *ψῆφος* 'pebble', Lat. *sabulum* 'gravel'; cf. *ψῶ* 'reduce to small particles'.¹⁰

In the following I deal with three additional etymologies which have a rather significant implication.

Once it is recognized, with Friedrich,¹¹ that the middle of *šuwa*¹² means

⁵ I am grateful to my colleagues Konstantin Reichardt and Paul Thieme for kindly answering numerous questions of detail and for furnishing valuable remarks and suggestions.

⁶ Related *ḥappina-* 'rich'; cf. Laroche, *RHA* 11.41 f.

⁷ See, above all, *Hittite code* §9 f.: 'if anyone inflicts a head injury upon a man' (SAG.DU-ZU *ḥu-u-ni-ik-zi*).

⁸ Cf. A. Noreen, *Altisländische Grammatik*⁴ §424 Anm. 2 (1923).

⁹ The formation seems analogous to *wewak-*: *wek-* 'demand'.

¹⁰ See Sturtevant, *HG*² 64; Pisani, *Jahrb. f. kleinasiatische Forschung* 2.217 (1952), and cf. E. Benveniste, *Origines de la formation des noms* 42.

¹¹ *HW* 200.

¹² *uttar*=*kan šu-u-wa-at-ta-at* 'the wording thereof became (proved) too voluminous' XXX 39 rev. 10 (end of a colophon); *man*=*kan DI-NU-ma šu-u-wa-at-ta-ri nat MA-ḪAR*

'swell up; become (too) voluminous or involved',¹³ one begins to wonder whether the verb should not be etymologically combined with Skt. *śvadyati* 'he swells up' and its cognates *śavas-* 'strength' and *śūra-* 'strong, hero'. There will be some hesitation; for the Sanskrit words of course contain *ś* < IE *k'*, as shown by Gk. *κνός* 'fetus', *κνέω* 'become or be pregnant', *κῦμα* 'wave'. This would contradict the equation IE *k'* = Hitt. *k*, evidenced by such cases as *kēr*, *kard-* 'heart' (cf. Skt. *śrad-* in *śrad-dādhāti* 'he believes', Lat. *cor*, *cordis*), *kitta(ri)* 'he lies' (cf. Skt. *śete*, Gk. *κείται*), *wekzi* 'he demands' (cf. Skt. *vaṣṭi*, Gk. *ἐκών*), *gim-*, *gimant-* 'winter' (Skt. *him-*, *hemantā-*, Lat. *hiems*), *genu-* 'knee', and others.¹⁴ However, there are some additional examples citable for the equation IE *k'* = Hitt. *š*.

There is first *šuppi-* 'pure, (ritually) clean, holy'.¹⁵ It combines readily with Skt. *śu-* in *śubh-*, *śubha-* 'good luck, beauty', *śubhrā-* 'bright, beautiful' (cf. Arm. *surb* 'clean, holy') and *śudh-* (*śundhati*) 'purify, clean'. Compare also *śundm* 'hail!', Av. *spənta-*, OCS *svetъ* 'holy' < IE *k'wento-*.

Furthermore we have *šup(p)ala-* '(herd of) animals'.¹⁶ The initial part of the word very probably goes back to **pšu-*,¹⁷ cf. Av. *fšu-*, which contains the zero grade of IE *pek'u-* (Lat. *pecus*, Goth. *faihu*). The second part is reminiscent of Lat. *po-p(u)lus* 'people', in which *po-* is unexplained¹⁸ and *-p(u)lus* is usually connected with Gk. *πλῆθος* 'multitude' (IE **p(e)lē-*).¹⁹

¹³ UTU-ŠI *uppau* 'if the case becomes too involved, let him refer it to My Majesty' XIII 2 iii 23 f.; *naš=šan* ŠAG-ŠU *šu-ut-ta-ti nu=za* *šarḫuwandan QA-TI-ŠU piran* UGU-a *karpan ḫarzi* 'his inner parts have become so swollen up that he has to hold up his entrail with his hand' KBo VI 34 iii 18 f.

¹⁴ Apparently related *šuwu-* 'full' (IX 28 i 12 f., missing in *HW*); *šuwu-* act. 'fill (trans.)', *šunna-* 'make full, fill'.

¹⁵ Hittite is, therefore, called a centum language; see most recently F. Sommer, *Hethiter und Hethitisch* 96 (1947); H. Otten, *Wissenschaftliche Annalen* 2.322 (1953).

¹⁶ *HW* 199.

¹⁷ Parallelism 'dog and swine, *šuppala-*, man' XXXI 127 i 42 ff. (cf. 'mankind, dog (and) swine, creatures of the field' VI 45 iii 15 ff.); furthermore 'sheepcote, corral, *šupla-*' KBo VI 34 iv 13 ff. — §163 of the *HC* deals with removing the brand of animals (differently *HW* 195 s.v. *šuniyabḫ-*).

¹⁸ Cf. Hitt. (*i*)*špant-* 'night'; Skt. *kṣap-*; see *Lg.* 27.475 (1951). — For *śu-* in Skt. see Thieme, *ZDMG* 95.343 ff. (1941) and *KZ* 69.172 (1951). He finds this development of **pšu-* in Vedic words like *śurūdḥ-* 'das Vieh mehrend' > 'Reichtum', *śūghanā-* 'das Vieh tötend', *śuṣā-* 'Vieh gewinnend'. He also asks whether Gk. *κίχλωψ* may not originally mean 'Vieh stehlend'.

¹⁹ Cf. Lat. *puer*, *pūbēs*?

¹⁹ How should Lat. *būbulcus* (**būfulcus*), *sūbulcus*, *aubulcus* be analyzed? More specifically: is their nature as nomina professionis occasioned by *-bulcus*, or only by *-cus* (cf. Skt. *pācaka-* 'cook', *khanaka-* 'digger')? The latter seems more likely, since *bū-*, *sū-* (with short vowel!) is the form typical in old compounds; cf. J. Wackernagel, *Altindische Grammatik* 2.1.§22a. If so, we would have to posit **būbulus*, **sūbulus*, and **aubulus*. — A word of similar formation from Hittite texts is *ḫupala-*, marked by a 'Glossenkeil' as non-Hittite. The context of the main occurrence, KBo VI 29 ii 33 f. (: *apun=ma=kan*. *KU₆-un GIM-an ḫupalaza appa ištaptā* 'she (the goddess) isolated that man like a fish from its school(?)', in identical context *nan=kan* ŠAH GIM-an *ḫumma appa ištaptā* 'I isolated him ... like a pig in the sty' *Ḫatt.* iv 25 f.), shows that the word has to do with fish. One is led to believe that the word for 'fish', occurring in the sentence in ideographic spelling (*KU₆-un*, a *u*-stem!), is also contained in *ḫupala-*, perhaps 'school of fish' (rather than 'net' as

In the case of *šuwa-* and *šuppi-* it is certain that the sibilant is present also in other Anatolian languages. For *šuwa-* this is suggested—if (as is probable) *šunna-* 'fill' is pertinent—by Palaic *šu-ú-na-at*²⁰ and by Hier. Hitt. *suwa-* 'fill';²¹ perhaps Luwian *šu-u-wa-at-ta* (*KUB XXXV 88 iii 5*) also belongs here. As for *šuppi-*, it should first be remarked that the name of the great Hittite king *Šuppiluliumaš* is duplicated by *Sapalulme*, king of the Syrian kingdom of Hattina under Shalmaneser III of Assur. Moreover, the Karatepe bilingual text provides the equation *sa-pi-sa-t/di?-a+r* = Phoen. *šlm* 'good fortune, good health'.²²

Unfortunately we do not possess in Cuneiform Hittite any correspondences of the vocables for 'horn', 'dog', and 'horse' that appear in the hieroglyphs as *šurni*, *šūwan-*, and *ašūwa-*, in terms of IE **k'rni-*, **k'(u)won-*, **ek'(u)wo-*; either Cuneiform Hittite did not possess these words²³ or they are hidden for us behind consistent ideographic spellings. The reading of the sign here marked by *šu* with a sibilant, posed by Gelb as early as 1942²⁴ and questioned by me in *JAOS* 64.85 (1944), can today be considered assured. The sign 'horn' is used in the name of a country *A-x-ra*, which by the nature of things can only be Assur.²⁵ This does not make Hieroglyphic Hittite a satəm language, however, as has been asserted.²⁶ The three examples for Hieroglyphic Hittite share with those assembled above the peculiarity that, in every clear case, the *k'* is followed by the vowel *u*. Since Cuneiform Hittite is doubtless a centum language, the shift from *k'* to *š* must be ascribed to the combinatory effect of the *u*, and the same must then also be assumed for the other Anatolian languages.²⁷ It would indeed be perplexing if Hieroglyphic Hittite were a satəm language but Cuneiform Hittite a centum language.

Friedrich's *Wörterbuch* will give scholars specializing in the comparative grammar of the Indo-European languages easy access to the Hittite vocabulary; it is to be hoped that they will make use of the opportunities thus offered. The decipherment of the Minoan inscriptions, which now seems to have entered a decisive phase, opens up new possibilities of research for the near future.

has so far been assumed). One is therefore inclined to think of IE **g'hu-/g'hju-* in Arm. *jukn*, Old Pruss. *suckis* (cf. Gk. *ιχθῆς*), on which see now Thieme, *Die Heimat der indogermanischen Ursprache* 48 (Mainzer Akademie, Abh. der geistes- und sozialwiss. Klasse 1953, Nr. 11).

²⁰ *Bo* 1551/c etc. (*ZA NF* 14.141) rev. 22.

²¹ I. J. Gelb, *Bibl. Or.* 7.136 (1950).

²² *Karatepe* 276; see H. Th. Bossert, *Jahrb. f. kleinasiat. Forsch.* 2.320 (1953).

²³ *SI-ar* 'horn' is to be read *karawar*; see Friedrich, *HW* 291 (with literature), and cf. Gk. *κέρας*.

²⁴ *Hittite hieroglyphs* 3.19 ff.

²⁵ Gelb, The contribution of the new Cilician bilinguals to the decipherment of Hieroglyphic Hittite 18 fn. 1 = 135 fn. 30 (mimeographed ed. 1950 = *Bibl. Or.* 7); Bossert, *Arch. Or.* 18:3.20 f. (1950); P. Meriggi, *Athenaeum* 29.25 fn. 1 (1951); Friedrich, *Arch. Or.* 21.134 (1953).

²⁶ Bonfante and Gelb, *JAOS* 65.173 ff. (1945). Cf. the debate during the 22nd Congress of Orientalists at Istanbul (*Proceedings* 146). Friedrich, *Arch. Or.* 21.134 (1953), wisely reserves judgment.

²⁷ Note the effect of ypsilon on a preceding IE *y* or *t* in Greek.

Armenisch-deutsches Wörterbuch. By DIRAIR FROUNDJIAN. Pp. xvi, 505. München: Verlag R. Oldenbourg, 1952.

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The information concerning the present stage of Armenian to be gained from most Western dictionaries of that language is largely vitiated by the attempt to combine materials from Modern and from Classical Armenian. Froundjian's dictionary clearly concentrates on the modern language; but here also the author seems to think it necessary to include a few words about the older stage. Such mixture may not be a very serious impairment of the descriptive function of a dictionary; but we cannot take so light a view of the fact that an undisclosed number of words were expressly made up (vii) for the purpose of being included in this dictionary.

The phonetic explanations in this work are very poor: the phoneme *t'* is described as [th] (163), but *p'* and *k'* as [p] and [k] (474, 485); the terms 'stummes *h'*' (321) and 'weiches R' (464) are at least rather odd. To list *kə* (without mentioning the variant *ku*) as 'Zeitwortpartikel' is of no use as long as its function is not explained. Some of the German expressions ought to have been discussed with a competent native speaker: *Buchstabenzahl* (71 etc.) will certainly be understood only as 'number of letters', not as 'numerical value of a letter'; *Nachbuchstabe* (402) is a rather prehistoric term (and the statement about *s*—'Altarmenisch als Nachbuchstabe Zeichen für Mehrzahl an Stelle von *k*'—is not only misleading but plainly wrong); words like *Jungmädel*, (*arenuš*, 42), *Volksgenosse* (*azgakiç*, 2), and *Parteigenosse* (*kusakçakan anker*, 160) are hardly respectable in present-day German usage.

Numerous misprints on the Armenian as well as on the German side contribute to the failure of this work to live up to the author's claims for it as 'grundlegend'. To succeed, the author would have needed an understanding of basic linguistic principles and methods.

Die Oxytonierung der griechischen Substantiva auf -iā. By MEINRAD SCHELLER. (Abhandlung zur Erlangung der Doktorwürde der philosophischen Fakultät I der Universität Zürich.) Pp. [vi], 146. Zürich: Dissertationsdruckerei Leemann A.G., 1951

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Scheller's main points are these: (1) that there is no essential difference in function between the suffixes -iā and -iā́; (2) that the accentuation -iā́ replaced -iā in the more popular, in the household words; (3) that this change of accent marked ancient Greek as it does modern; and (4) that the accentuation -iā́ replaced -iā upon the consonantization of the accented *i*.

His examples and analyses demonstrate convincingly that there is no difference in function to conform with the difference in accent. In opposition to earlier classifications he shows that, although nouns of collective and concrete meaning dominate the -iā́ group, they are found also in the -iā group; and that abstracts form part of both groups.

An examination of the 52 common and 20 proper oxytone nouns in *-iā* listed by Scheller might well justify the conviction that these are the popular words. Their applicability to everyday life, despite some doubtful cases, seems as striking a common characteristic as any other which has been proposed for them. Scheller could hardly be expected to take into account what may be equally popular words among the thousands accented *-iā*. Some of these (common enough to be listed in Buck's *Dictionary of selected synonyms* under both ancient and modern Greek—*ἐστία* and *ἐργασία*, for instance) have retained the accent *-iā* from classical times to the present.

The consonantal pronunciation of antevocalic *ι* is an outstanding feature of modern Greek; that many common words in modern Greek have acquired the oxytone accent cannot be denied. As for antiquity, the warnings of Greek grammarians against the oxytone accentuation of some words in *-iā* are Scheller's best evidence that there was even in antiquity a tendency toward that accentuation.

Scheller quotes (95) Pernot's explanation of the consonantization of *ι* (i.e. that first the accent shifts, then the *ι* becomes consonantal), and objects that by this analysis an isolated and unusual process is used to prepare the way for a common sound-change. He would reverse the process and persuade us that the common tendency toward consonantal pronunciation of the *ι* gave rise to the accentuation *-iā*. Herein (and in his application of the principles to antiquity) lie the most delicate of the questions which he treats.

The reader might be tempted here to turn to the poets and point out that in the Palatine Anthology, for instance, the *ι* of *-iā* words is metrically ambiguous 6 times, necessarily vocalic 7 times, and necessarily consonantal only once. The one consonantal *ι* is in *ἀχρυσιαί* (9.384.15), a word which appears as hapax legomenon in Homer (*E* 502). This seems evidence rather against than for consonantal *ι*. Actually, however, this is not evidence of any kind. According to Scheller the vocalic metrical value of the *ι* belongs to an earlier age, the written accent *-iā* to a later age which produced it by modernizing under the influence of contemporary speech.

If dissyllabic *-iā* in the old poets was modernized by scribes to look exactly like later spoken monosyllabic *-iā*, then what of the later poets in whose time some words had already acquired consonantal *ι* and accented *ā*? Did they vocalize the *ι* not merely with the freedom with which their predecessors had consonantized it, but consistently? The circumstances which Scheller supposes provide an answer to such an objection: the later poets preserved the traditional metrical value despite contemporary speech.

It is natural, one must admit, for scribes to modernize and for poets to archaize, but between them they have destroyed what might have been incontrovertible proof of Scheller's most novel and striking thesis. For those scholars of antiquity who need to be reminded that writing is not by any means the same thing as language this volume would be salutary reading.

Any reader, whether he is convinced or not, will be inclined to admire the reasoned and novel conjecture, the minute and scholarly care which Scheller brings to the problem.

Recherches sur XPH, XPHΣΘAI: Étude sémantique. By G. REDARD. (Bibliothèque de l'École des Hautes Études, Sciences historiques et philologiques, Fasc. 303.) Pp. 122. Paris: Librairie Ancienne Honoré Champion, 1953.

Reviewed by JAMES W. POULTNEY, *Johns Hopkins University*

The words treated in this monograph form an interesting group not only because of the curious adoption of verbal functions by *χρή*, originally a substantive, but also because of the wide semantic divergence between forms meaning 'want' or 'need' and those relating to the consultation and responses of oracles.

The forms from other languages which are recognized in the etymological dictionaries as cognates of *χρή*, *χρησθαι* (for example Skt. *hrasati* 'decrease', Ml. *gerr* 'short') are dismissed on the first page as without value for the determination of the central meaning, and quite rightly so, for the relationship of these forms to *χρή* is by no means assured. The problem then is a purely Greek one, and the basis for its treatment is an examination of the early literature, especially Homer, Pindar, Herodotus, and the tragedians, together with a classification of the uses of *χρησθαι* adopted from the dictionary of Liddell-Scott-Jones. The argument which occupies the first half of the book, preceding the treatment of individual words of the family of *χρή*, may be very briefly summarized. *χρησθαι* is a denominative to the originally nominal *χρή*, as *τιμᾶν* is to *τιμή* (< *τιμᾶ*). The middle and not the active forms must be taken as the starting point in the study; not only is the middle decidedly more frequent in the Homeric poems (35-6) but the usage of *χρησθαι* is fully in accord with the normal Greek use of the middle voice for actions in which the subject is a person having a special interest in the action, while the effect on the object is of minor importance. The active uses can be explained as secondary developments from the middle uses, after the model of similar contrasts in other verbs. Thus, if we adopt 'rechercher l'utilisation de quelque chose' as the central meaning of *χρησθαι*, we may view *ἐχρησα* as formed in order to express the meaning 'j'ai donné à utiliser, j'ai prêté', whence was developed in the reverse direction *ἐχρησάμην* with the special meaning 'j'ai emprunté' (38, 44). The meaning 'consult an oracle' is easily derived from other instrumental uses, for the person actually makes use of the god or the priest or the ghost, as *Od.* 10.492 = 23.323 *ψυχῇ χρησόμενος Θηβαίου Τειρεΐαο/μάντιος ἁλαοῦ* plainly shows. Thus are developed four principal classes of meanings: 'consult an oracle', 'borrow', 'have need of', 'make use of', sometimes with active and passive meanings contrasting with these middle meanings. This is a condensation of Redard's listing (34); it must be noted that the order in which he gives the meanings at this point is the standard dictionary order rather than the order in which the various meanings were developed, and further that the separate meanings had already developed before the time of the earliest documents, as he points out (45). This prompts the reviewer to raise the question whether any new light can be expected on the word family under consideration from the Linear B texts from Crete and the Greek mainland. It seems quite conceivable that such forms

as *χρήματα* might appear, but unlikely that these documents will furnish anything significant to add to the argument in Redard's monograph.

Several pages (55-61) are devoted to a discussion of the difference between *χρή* and *δεῖ*, their equivalence when signifying that which is destined by fate, and the use of both with a dependent genitive, which the author takes to be a cause rather than a result of the partial concurrence between them. The monograph then passes on to a treatment of the various nouns, adjectives, and denominative verbs ultimately derived from *χρή*: *χρηίζω*, *χρειώ*, *χρημα*, etc., making over twenty in all. Here the separate sections are in part detailed listings, which extend far beyond what is essential in supporting an argument, but which are useful for those seeking a more comprehensive treatment than can be found in the dictionaries. The omission of *χρήμη* from a list of this sort is probably an oversight; it is an infrequent form, but LSJ list it as an Ionic equivalent of *χρεία* I and give citations from Archil. 56.5, Ps.-Hdt., *Vit. Hom.* 13, 14, together with two doubtful instances.

The book as a whole deserves to be recognized as a sound and careful treatment, and to my knowledge the most detailed treatment, of the special topic to which it is devoted. Minor blemishes appear to be few in number: 30, in Xen., *An.* 1.3.18 add *τί* at the beginning of the passage; 49 fn., *enterpon*, cited as Lithuanian, is Old Prussian; 51, in *Il.* 9.613 transpose the text to read *οὐδέ τί σε χρή/τόν φιλέειν*; 105, in Thuc. 2.54 read *τοῖς εἰδόσιν* for *τῆς εἰδόσιν*.

Handbuch des Gotischen. By WOLFGANG KRAUSE. (Handbücher für das germanistische Studium.) Pp. xx, 306. München: C. H. Beck'sche Verlagsbuchhandlung, 1953.

Reviewed by HERBERT PENZL, *University of Michigan*

Wolfgang Krause, Professor of Comparative Linguistics at the University of Göttingen, characterizes this book in the preface as an introduction to Gothic on a historical basis. His brief bibliography (xv-xviii) excludes older reference books like E. Schulze's *Glossar* and v. d. Gabelentz's *Grammatik*, but mentions LANGUAGE and the study by W. P. Lehmann. In the first part Krause discusses the history of the Goths, Wulfila's biography, the Gothic manuscripts, and Crimean Gothic (3-26). The second part has two major divisions: Allgemeine Kennzeichnung und Verwandtschaft des Gotischen (29-58), and Systematische Grammatik des Gotischen—the latter consisting of Lautlehre (59-126) and Formenlehre (126-248). The third part consists of Gothic specimens (251-65); the fourth is a Gothic-German glossary (269-306), where the definitions sometimes differ slightly from those given in the text.

The whole arrangement of Krause's book indicates that it is primarily intended as a textbook for students at German universities. It is therefore not to be expected that we will encounter many startling new interpretations or discoveries in it. The phonology is a more or less traditional account of the development of vowels and consonants from Indo-European to Proto-Germanic to Gothic, dealing with the 'auslautsgesetze', the ablaut series, the Germanic

consonant shift, and Verner's law. In the morphology the Gothic forms are accompanied by Proto-Germanic and Indo-European reconstructions, of the type found in the grammars by Kieckers (1928), Krahe (1948), and even Wright (1910). Krause sometimes fails to explain his specific reconstructions; thus he apparently assumes a loss of IE *e* in the inflectional endings *-es(o)* (gen. sg.) and *-es* (nom. pl.) in polysyllabic forms before it becomes PGc. *i*, but does not discuss this. In his reconstructions he alternates between **(e)z* and **iz*: **gumin-(e)z*, **guman-(e)z*, **tungon-(e)z*, but **burg-iz*, **mann-iz*, **broþ-r-iz*, **dag-izo* (sic, with final *o*). Apparently following somewhat uncritically Wiklund's argument (1924) from the values in Finnish loanwords, Krause assigns to PGmc. *ē*¹ 'eine geschlossene Aussprache', which is also assumed for *e* in Gothic *gemun* and *mena*. PGmc. *ē* is said to occur in weak-stressed syllables, e.g. *-æn* and *-ær* (89). Krause fails to discuss the phonemic split postulated by him, which must confuse his readers.

Krause states in the preface (vi) that he wants to call his readers' attention to 'die Methoden der modernen, besonders im Ausland eifrig betriebenen Sprachwissenschaft ..., vor allem ... die schier unbegrenzten Möglichkeiten der amerikanischen Laryngalforschung'. But he discusses the laryngeal theory and its interpretation of the IE ablaut only briefly (69 f.); although sceptical of its explanation of the Gmc. *Verschärfung* (104), he states that IE *tenues aspiratae* are now mostly explained as clusters of *tenues* and *laryngeals* (70, 108 f.). He seems to accept the explanation advanced by Chr. Stang (*À quoi correspondent en germanique le th sancrit, NTS 15.335 ff.*) to account for *t* in Gothic *namt* (IE *-tha*) and *bairats* (IE *-thes*) but *þ* in *bairiþ* (IE *-the*). Stang suggests that instead of IE *th* we should assume *t* followed by the first laryngeal (*t*₁) in the first two cases, *t* followed by the second laryngeal (*t*₂) in the third case. In answering Stang, I. Dal (*NST 16.328 ff.*), although sympathetic to the laryngeal theory herself, found the usual explanation of *t* in forms like *namt* as an analogical transfer from *gaft*, *falht*, *wast* and of *t* in *bairats* as due to a pre-Gothic change *þs* > *ts* more convincing. But Krause considers the 'Zauberstab der Laryngaltheorie' indispensable here (vi). Unfortunately, such eclectic and sporadic adoption of laryngeals for Indo-European cannot give his readers the right introduction to the laryngeal theory. They are not told about the use of Kurylowicz's notation nor about Sturtevant and his Indo-Hittite hypothesis.

Krause agrees (112) with Hirt that Proto-Indo-European originally possessed only two 'guttural' series, simple velars and labiovelars; he thus boldly departs from the assumption of the traditional three series, palatals, velars, and labiovelars. Obviously the composition of the PIE consonantal pattern is still a controversial topic, even if the laryngeals are not considered. A treatment of Germanic comparative phonology may not be the best place to settle any such argument. PGmc. phonology cannot establish the contrast between palatals and velars; and even the labiovelars can be historically analyzed as clusters of 'palato-velars' plus nonsyllabic *u* (*w*), since there is no contrast between Gmc. reflexes of assumed IE labiovelars *kʷ* and of IE clusters *k̑w* or *kw*. Consistent differentiation between Common-Indo-European (CIE) and Proto-Indo-European (PIE) and perhaps Proto-Proto-Indo-European (PPIE) patterns

would clarify the whole argument considerably. A threefold series consisting of palatal (*k̑*), velar (*k*), labiovelar (*kʷ*) is justified for CIE on the basis of the values in all IE languages and their correspondences, even though there are no more than two 'guttural' series in any one IE branch. Another question is whether the distribution of *k̑ k kʷ* in the reconstructed CIE forms provides evidence for reconstructing a PIE phonemic split of the allophones of /*k*/ into /*k̑*/ and /*k*/, so that we can perhaps postulate an earlier PIE pattern consisting of only the phonemes /*k*/ and /*kʷ*/. Relevant also is Meillet's argument from dialect geography, that the centum languages, which are found in the extreme east and west of the IE area, are apt to have preserved the original PIE pattern. Thus, the argument whether we have to assume three or two 'gutturals' may resolve itself into merely a terminological disagreement: are we willing to equate CIE (with a series of three) with PIE, or should we reserve the latter term for a stage that can be internally reconstructed from our reconstruction (a PPIE really), and which had only a series of two? The recognition of several layers of reconstruction is not just a matter of terminology, of course, but an important question of procedure and method. It is obvious that most recent attempts to increase the IE consonant inventory beyond Brugmann's pattern (by Benveniste, Martinet, Lehmann, and Kurylowicz), which are occasionally matched by the reducing of the IE vowel pattern to little more than one solitary 'Urvokal', refer to PPIE (Krause's *Urindogermanisch*?) reconstructed from CIE (Krause's *Indogermanisch*), not to CIE itself. But, as Krause seems also to believe, the Hittite evidence demands an inclusion of laryngeals into CIE, though only to the extent that they are found in that language.

Krause assumes (50) that PGc. *þl-* became *fl-* in North and West Germanic: Go. *þliuhan* 'flee', OIc. *flýja*, OE *flēon*, OHG *flīohan*. He is apparently not familiar with George Nordmeyer's article (*Lg.* 11.216-9; also Prokosch, *Comparative Germanic grammar* 87; R. A. Fowkes, *Gothic etymological studies* 18), which convincingly proved that PGmc. *fl-* (IE *pl-*) became *þl-* in Gothic. The few existing *fl-* forms in Wulfila's text (*flodus*, *flahtom*, *flautai*, *flauteiþ*, *faiþflokun*) were probably Ostrogothic forms introduced by the scribes.

It is clear that in matters of prehistoric phonology Krause has endeavored to be open-minded and up-to-date. The same desirable attitude characterizes his description of the Gothic pattern itself, or at least his programmatic statement (52), which sounds very promising: 'Während für eine tote Sprache wie das Gotische eine phonetische Beschreibung der Laute ausgeschlossen ist, lässt sich das phonologische System mit einiger Wahrscheinlichkeit aufstellen.' But in spite of this statement, he fails to deal with the major phonemic problems of Gothic, which are of course linked to an interpretation of Wulfila's orthography. Krause does not seem to know W. G. Moulton's article on the phonemes of Gothic, *Lg.* 24.76-86 (1948), or mine in *JEGP* (1950), nor F. Mossé's treatment of *ai* and *au*, although he mentions Mossé's *Manuel* in his bibliography. Wulfila's symbols *ai* and *au* are to Krause 'fallende Diphthonge', and 'kurze, offene Monophthonge' (62); he does not even see any problem in the acceptance of the comparative evidence for phonetic identifications. Krause does not go so far as to differentiate between the phonetic values of *i* in Go. *giba* (Gmc. *e*)

and *i* in Go. *fiskans* (Gmc. *i*), though he would have been equally justified in doing that too. He adheres more closely to his stated objectives when he discusses consonantal values. He interprets the alternation between *f* and *b*, *þ* and *d* (in *hlaiþs hlaiba*, *staps stada*) as indicating such medial allophones as [b] and [d] but adds, perhaps under the influence of the Streitberg-Sievers analysis: 'ob dafür zu Ulfilas Zeit bereits stimmhafte Verschlusslaute eingetreten waren, ist nicht entscheidbar' (63, 123). He interprets phonemically the fact that Wulfila writes *g* in *dagis* and also in *dags dag*, where he assumes 'Auslautsverhärtung', i.e. [x]: 'so lässt sich das so verstehen, dass der Laut x im Gotischen Ulfilas kein selbständiges Phonem war, sondern nur in festgelegter Korrelation zu *γ* vorkam'. He comments further (125): 'im Bibelgotischen scheint der Buchstabe *h* in allen Stellungen dem reinen Hauchlaut *h* zumindest sehr nahegestanden zu haben.' Like all phonetic identifications based on purely phonemic evidence, Krause's may be open to question, but they resemble Moulton's recent interpretation (*Lg.* 30.1 ff.) of Gothic /h/ as a 'glottal spirant' in all positions. The alternation between *h* and *g* in such forms as *aih aigun*, *juggs juhiza*, *huhrus huggrjan*, *stainahs wulpags* makes it necessary to interpret *h* as a velar spirant in Pre-Gothic and in PGmc.: Verner's law certainly cannot be called 'Pre'-Germanic.

Unless we first consider the orthography of Gothic vowels as a reflection of the phonemic pattern, we cannot achieve a satisfactory reconstruction for earlier periods. All the handbooks have uncritically assumed that *e* and *o* must be long vowels even in inflectional endings (e.g. in *dagos*, *dage*); *ai* and *au* in final morphemes have been interpreted as diphthongs belonging to the same long quantity class. Can we actually prove that quantity is phonemic in Wulfila's Gothic? The distinctions between *a* and *ā*, *u* and *ū* are only assumed on the basis of comparative evidence. The contrast between *i* and *ei* may be one of quality rather than quantity. The inflectional allomorphs *-jis -jip* and *-eis -eip* cannot be called conclusive. If we assume *ai* and *au* to be monophthongs and quantity to be nonphonemic in Gothic, our historical interpretation of the final morphemes in *gibai*, *genais*, *genai*, *bairais*, *bairai*, *sunaus*, *sunau*, *bairau*, *atsteigadau*, *liugandau*, *hailaidau* is apt to differ considerably from the one offered by the handbooks. As already Hirt pointed out, the optative and imperative forms ending in *-au* can be easily explained historically if we assume a value [ɔ]; the other alternative is to invoke what Streitberg called 'die einst vielfach in Anspruch genommene Partikel *u*'. The phonemic interpretation of vocalic values in Gothic inflectional endings is apt to endanger the whole assumption of PGmc. long vowels with 'schleifton' (three morae) and long vowels with 'stosston' (two morae), and should lead to a restatement of most of the 'auslautgesetze' of Germanic. It will strengthen the argument of Kurylowicz, who believes that circumflected long vowels in Lithuanian and Greek are innovations there, and thus probably contribute to making the theory of two types of IE long vowels quite untenable. We must carefully study the evidence of each language, and must not hesitate to label any material inconclusive if the label fits; certainly we cannot base elaborate reconstructions on such material.

All this is linked to a careful consideration of Wulfila's orthography, which is highly relevant to historical phonology.

Krause's treatment of morphology agrees, on the whole, with that of other handbooks. He labels the long stems like *hairdeis* 'ia-stems', the short stems like *harjis* 'ja-stems'. G. Must's explanation (*Lg.* 28.218 ff.) of the genitive plural ending *e* in *dage* he rightly calls (140) 'nicht überzeugend'; Must seems to assume a phonemic confusion between /i/ and /e/ in Wulfila's own dialect, although the evidence points only to a coalescence in the dialect of some of the scribes. Krause believes (211 f.), obviously following the hypothesis of Wackernagel and Behaghel, that the Germanic weak preterit was derived from an IE medial aorist form ending in *-thēs (2nd sg.), and that the plural of the Gothic weak preterit, e.g. *nasidedum*, contains the characteristic morpheme *de* and the ending *um* of the strong verbs.

Krause announces in the preface (vi) that he wants to inform the reader 'über den derzeitigen Stand der Erforschung von Aspekt und Aktionsart'. Streitberg's famous article Perfective und imperfective Aktionsart im Germanischen, *PBB* 15.70-177, has had the unfortunate result, in spite of isolated protests (by A. Beer, A. Mirowicz, B. Trnka, and others), of leading to a fairly general acceptance of aspect as a grammatical category for Gothic and for Germanic in general. This has gone so far that Senn in 1949 (*Lg.* 25.409) could seriously advance the opinion that the grammatical category of aspect in Slavic was an importation from Germanic. There is no doubt that a variety of aspectual meanings expressed in various formational and lexical ways can be found in all Germanic languages. Thus Sapir says that English *cry* 'is indefinite as to aspect, *be crying* is durative, *cry out* is momentaneous, *burst into tears* is inceptive, *keep crying* is continuative, *start in crying* is durative-inceptive, *cry now and then* is iterative, *cry out every now and then* or *cry in fits and starts* is momentaneous-iterative' (*Language* 114, fn. 22). Publications by S. Agrell, E. Koschmieder, E. Hermann, and others have introduced certain refinements into the semantic analysis of alleged aspectual forms in Germanic, particularly a distinction between the objective 'Aktionsart', as found in the lexical meaning of *finþan* 'find' or *briggan* 'bring' (nondurative), and the subjective 'aspect', which expresses 'die subjektive Anschauung des Sprechenden von dem jeweiligen Handlungsverlauf', e.g. *gahausjan*, *gasaihwan* (perfective), *hausjan* 'hear', *saihwan* 'see' (imperfective). Semantic and stylistic comparison of idiomatic Gothic simple verb forms, compounded forms with the prefix *ga-*, and the forms of the Greek original is a legitimate and useful study. Verbal word formation by means of the prefix *ga-* in Gothic does not constitute a sufficient morphological basis for the recognition of a grammatical dichotomy between an 'imperfective' and a 'perfective aspect', especially since the category of tense is not affected. Neither the simple nor the compounded verbal forms have a simple future or any regular future phrase in Gothic. [Cf. now Scherer's article on aspect in Gothic, *Lg.* 30. 211-23.]

Krause uses the form *Ulfila* throughout the book, because that is the form given in the Latin epistle of Auxentius, Wulfila's personal friend and disciple.

The form of Wulfila's name has been often discussed because conflicting forms are recorded in the sources: e.g. *Θύρφιλας* (Philostorgios), *Θύρφιλα* (gen.) (Wulfila's seal), *Vulfila* (Jordanes), *Gulfila* (Isidorus); K. K. Klein (Der Name Wulfilas, *Zschr. f. vgl. Spr.* 70.154-76) has recently reviewed the pertinent material in detail. There is no evidence of any kind that in Wulfila's Gothic a *w* before a back vowel was ever dropped, even in 'lässiger Aussprache'. The Greco-Latin form *Ulfila*, with its Greek loss of initial *w*, certainly cannot provide another isogloss linking North and East Germanic; Krause seems to have that in mind when he writes (103), 'Nur in Personennamen konnte auch im Gotischen *w* vor *u* ausfallen.' We must assume that Wulfila when he spoke Gothic called himself *Wulfila*.

Krause proposes as new terms for North, East, and West Germanic respectively the *dagaz*-, the *dags*-, and the *dag*-group (41 ff.). This is obviously the result of the recent attacks by German scholars on the alleged ethnical unity of West Germanic. Krause apologizes for still using the old terms too, and assures us that they are 'rein geographisch orientierende und nur für den fraglichen Zeitraum gültige Benennungen, die keinen ethnographischen Wert besitzen'. This 'ethnographischer Wert' is hardly a requirement for the names of reconstructed proto-languages. Krause's new terms are based on the nominative singular of the Germanic *a*-stems; they emphasize the loss or preservation of final *s*. Unfortunately, this is the feature that in another case form, the nominative plural of the same class, would provide us with a puzzling isogloss within West Germanic: e.g. OE *dagas*, OS *dagos*, OFris *dega(r)*, but OHG *taga*. The term for North Germanic is the Runic form, which is on a different time level from *dags* or *dag*, and is identical with the proto-form PGmc. **dagaz*. It is not likely, therefore, that Krause's new labels will be widely adopted.

For the outward appearance and typography of the book there can be nothing but praise. Printing errors are few. The book will serve the expressed purpose of its author very well. Since the old traditional approach in linguistic studies is bound to prevail in Germany for a long time, a foreign reviewer can only signalize with pleasure the introduction of laryngeals and phonemes into a textbook. We must recognize that such men as Krause, Krahe, and some others constitute the small minority among German scholars who still keep up the distinguished tradition of German comparative linguistics, which at one time led the world.

An introduction to a survey of Scottish dialects. By ANGUS McINTOSH. (University of Edinburgh: Linguistic survey of Scotland, Monographs, No. 1.) Pp. xii, 122. Edinburgh: Thomas Nelson and Sons Ltd., 1952 (1953).

Reviewed by RAVEN I. McDAVID JR., *Western Reserve University*

Scholars have many reasons to welcome McIntosh's book, the first in a series of monographs based on the current investigation of Scottish dialects. The significance of the book, be it said at the outset, is not in the raw data it contains; actually it discusses only a few speech-forms and their distribution. But it

augurs well for our future understanding of the regional and social varieties of Scottish speech. We can be sure that the survey is in good hands; it will be vigorously pursued; it will provide the study of Scots speech with valuable material to complement the work of Craigie's *Dictionary of the older Scottish tongue* and Grant and Murison's *Scottish national dictionary*; and the friendly relations between McIntosh and American dialect geographers should lead to the gathering of data that will help us to interpret the Scottish contribution to American speech. In broader terms, McIntosh's *Introduction* offers a sound critical appraisal of the aims and methods of dialect geography in the light of recent developments in linguistics. What is perhaps most important, McIntosh writes well, presenting undiluted linguistic truth so effectively and so clearly that the specialist need not feel insulted or the layman overwhelmed.

The need for a new survey of British dialects, making use of modern techniques, has been recognized for some time. No one should underestimate the work of the English Dialect Society, or the invaluable data in Ellis's *Early English pronunciation* and in Wright's *English dialect dictionary* and *Grammar*. But as with many pioneer works, the techniques by which these studies were prepared have been greatly improved in the past two generations. The volunteer work of the enthusiastic but untrained amateur needs the direction of a specialist; for the haphazard collection of anything that strikes the investigator's fancy, it has been necessary to substitute systematic investigation of selected items, to provide comparable data on grammar, pronunciation, and vocabulary;¹ and the assembling of data from whatever communities amateurs could be persuaded to work in, has been replaced by surveys over a network of communities reflecting the economic, social, and cultural history of the area. It is no reflection on Ellis and Wright that the Edinburgh group should have begun a new investigation of Scotland, or that in England a similar investigation (though differing somewhat in method) is being inaugurated under Harold Orton of Leeds and Eugen Dieth of Zurich.²

Perhaps even more important than the new techniques of dialect investigation is the changing estimate of what constitutes dialect. Rarely nowadays do investigators limit their inquiries to the old-fashioned informant and the speech of the uneducated; rarely do they assume a polar opposition between a uniform standard language (Kurath's CULTIVATED SPEECH) and diverse local dialects (Kurath's FOLK SPEECH). Instead, they generally recognize that cultivated

¹ To take one small example, the Atlantic Seaboard states have a variety of terms for the earthworm, distributed in regional patterns that reflect settlement history and suggest that most of them must have come from the British Isles. Yet of these terms the *English dialect dictionary* includes only the Narragansett Bay *eaceworm* and the Windsor (Conn.) *angledog*. See Hans Kurath, *A word geography of the eastern United States* 74-5, figs. 139 and 140 (Ann Arbor, 1949).

² The English questionnaire was published in *Proceedings of the Leeds Philosophical and Literary Society, Literary and historical section* 6:9.605-700 (1952). The method and the underlying theory of dialect are discussed in the preface; by Dieth, *Linguistic geography in New England*, *English studies* 29.65-79 (1949); and by Orton and Dieth, *The new survey of dialectal English*, *English studies today* 63-73 (Oxford, 1951).

speech as well as folk speech may have regional varieties, and that between cultivated speech and folk speech is a vast body (largely uninvestigated) of **COMMON SPEECH**—the habitual vehicle of communication for the yeoman or independent small farmer, the small storekeeper, the skilled craftsman—with distinct geographical varieties centering around regional or local commercial centers. Not only do all three varieties—folk speech, common speech, and cultivated speech—vary from place to place, but the relationship of the three is not everywhere the same. In many places the common speech, in some the cultivated speech, clings to characteristically local pronunciations, words, and grammatical forms; in other places, only the oldest and least sophisticated folk speakers retain the old forms. Moreover, dialect forms are not just relics of older usage that persist in relic areas; equally important are the innovations that have arisen or spread in focal areas. A true picture of dialect usage must take account of both the preservation of relics and the spread of innovations.

The Scottish survey is of course important for an understanding of the relationship between British and American dialects. Speakers of Scots constituted an important part of the American colonial population. Highlanders settled the Cape Fear Valley in North Carolina, and in Georgia formed the military colony of Darien at the mouth of the Altamaha, established to protect Savannah against the Spaniards. Ulster Scots set up independent frontier communities in New Hampshire and in northeastern South Carolina; the main body of their migration settled the Pittsburgh area in western Pennsylvania, from which they spread south through the Shenandoah Valley into the piedmont and mountain areas of the Carolinas and Georgia. The important role of the Scots in Canadian history is apparent to even a casual student; it is possible there are more speakers of Scots Gaelic in Nova Scotia than in Scotland. In view of these facts, the student who would see American dialects in their true perspective will find the distribution of dialect features in Scotland and Northern Ireland only slightly less important than that in England itself.

The Scottish survey, a joint project of the departments of English and Celtic at the University of Edinburgh, will record both Scots (Scottish English) and Gaelic usage. It is concerned not only with the dialects of Scotland proper, but also with those of Northern Ireland, the Isle of Man, and the northernmost counties of England—regions long affiliated with Scotland, both culturally and linguistically. So far, the evidence has been assembled only through two correspondence questionnaires, distributed to some 4100 communities; at the time the *Introduction* went to press, not all of the first questionnaires had as yet been returned, much less analyzed. The second questionnaire was distributed only last year; field investigations, as well as further questionnaires, lie in the future. As anyone familiar with dialect work can appreciate, it will be some time before the survey is completed and the results are published. In the meantime, since the success of the project depends on continued public interest and public cooperation, McIntosh and his colleagues are bringing out a series of monographs on related topics, the first one naturally being the explanation of the

project.³ The ultimate publication of the Scottish atlas itself will modify the conclusions of some of the volumes, but will not supersede them.⁴

The *Introduction* explains to the scholar and the educated layman the purposes of the survey, the problems that such investigations meet, and the solutions here proposed. It appeals to national pride by properly identifying Scottish speech as an aspect of Scottish culture. It avoids all pretense to omniscience or finality by pointing out that the study of regional and local speech is never completed as long as the language endures, but must be undertaken many times and with the help of many techniques.

At the outset McIntosh delimits the scope of the Scottish survey by showing how the investigation of the living language at a particular time requires a different procedure from that of a historical dictionary (such as Craigie's or Grant's), a study of place names, or the compilation of local glossaries. He is careful to explain that a collection of local glossaries would not be equivalent to a linguistic survey, since the compiler of such a glossary seldom attempts to provide a complete local vocabulary but concentrates upon the oddities—the things that he feels are 'dialect'—and so usually misses many distinctive items. McIntosh shows how dialect studies may be either DESCRIPTIVE⁵ or COMPARATIVE; comparative studies in turn may be either DIACHRONIC, comparing different historical stages of the same dialect, or DIATOPIC, comparing different dialects at the same time. Diatopic studies are therefore the immediate business of the linguistic geographer, regardless of the descriptive or historical uses to which his data may subsequently be put.

McIntosh wisely indicates that the investigator of dialects cannot work in a vacuum, but must cooperate with other social scientists: historians, social anthropologists, geographers, and students of material culture. Population history is always important in interpreting the distribution of linguistic forms; in Scotland the significant movements of population largely took place before

³ A second monograph, by Kenneth H. Jackson, *Contributions to the study of Manx phonology*, is to appear this year; a third, by J. S. Woolley, *Bibliography of Scottish linguistic studies*, has already come out.

⁴ As McIntosh points out (96), the monumental appearance of such supposedly definitive works as the *English dialect dictionary* may lead some scholars to assume that it is unnecessary either to preserve the original research materials or to supplement, corroborate, or correct it by later studies. Many British scholars have reported difficulty in enlisting support for a linguistic atlas, since a widespread opinion holds that Ellis and Wright have recorded everything.

⁵ McIntosh reminds us that good pedagogical prescriptive treatments are essentially descriptive, since they limit themselves to the features of the single dialect (upper-class Parisian French, British Received Standard, or middle-class Chicago English) which the instructor feels will best serve his students' needs. What linguists criticize in many prescriptive treatments is not their prescriptiveness, but the choice of an inappropriate model (such as British pronunciation as a standard for Americans), an erroneous description (as when the preterit *dove* is deplored as 'colloquial' or 'substandard', though in American communities where both *dived* and *dove* occur, the latter is generally favored by younegr and more sophisticated speakers), or the implication that the choice between linguistic forms is an ethical choice between good and evil rather than a social choice between appropriate and inappropriate.

there were documents to record them, but they can be traced and must be reckoned with. As a force altering the number and nature of local dialects—operating on all, though differently on each one—the linguistic geographer must take account of the standard language. For McIntosh this is not equivalent to British Received Standard, but something like the working definition which Fries uses: that variety of the language (itself with many local variations) used by those in positions of respect who conduct the important affairs of the English-speaking world. He rejects the notion of a 'pure dialect' as something that has never existed. Moreover, he points out, dialect boundaries—even in Scotland—are rarely very sharp, so that it is often difficult to determine where one local variety leaves off and another begins. On these questions, though he has said nothing really new or startling, he has said it more deftly and more intelligibly than it is often said.

On questions of method, the book is more controversial. McIntosh takes sharp issue with some of the traditional methods in linguistic geography, especially some of the theories of Gilliéron (or rather, their rigid application by Gilliéron's disciples): the use of a single uniform questionnaire throughout the area, and of a single fieldworker whose job it is to record automatically—and to submit without editing—a minute impressionistic transcription of the responses of each informant. McIntosh suggests that for the Scottish survey it might be profitable to reexamine the techniques of investigation, to determine whether all types of evidence need to be collected by the fieldworker. Utilizing not only recent experimental dialect studies but lessons from statistics, demography, and phonemic theory, he concludes that the fieldworker ought to be used only for those purposes for which he has no substitute—in the main, phonetic recording⁶—and that the bulk of the lexical evidence might well be gathered by correspondence questionnaires, such as those which the Scottish survey is already using.

The arguments of McIntosh against the traditional procedures are plausible, though I do not find them completely convincing in principle, and though I doubt whether the Scottish methods would be applicable to North America. (1) Good fieldworkers are hard to find, hard to train, and hard to keep enthusiastic about field work for more than a few years. (2) The typical European 'portmanteau questionnaire' of 2000 items or more is bound to yield many blanks if there are marked differences in local culture.⁷ (3) Field work is an

⁶ Even for phonetic studies, portable tape recorders can supplement the fieldworker's notes. Though recorders can be used only where electric current is available, and must be carefully tended during the interview, the recording can be played repeatedly, will catch many more conversational responses than even the best fieldworker can jot down, and provide better evidence than impressionistic transcriptions for such problems as stress, intonation, and juncture. On the other hand, the interviewer who relies solely on the tape will spend much more time transcribing the data than he would spend in a field interview, and may find important material obscured by noise or not on the tape at all. The best solution is to conduct the interview in the way that one has found most effective, transcribe impressionistically during the interview, and use the tape as a check.

⁷ The questionnaire published by Orton and Dieth contains about 1125 items, heavily weighted in favor of relics to be found only in relatively isolated communities. See my review in *JEGP* 52.563-8 (1953).

It is obvious that one can spend too much time attempting to devise a fool-proof ques-

expensive means of collecting data, which should be used only when no other satisfactory means is available. (4) For most of the lexical items fine impressionistic transcriptions (or even phonemic transcriptions) are superfluous, since—to take a specific example—the comparison between the cow calls *co-boss* and *sookie* does not involve a single potential phonemic contrast in analogous environments.⁸ Allowing a week for interviewing each informant with the long questionnaire (a reasonable estimate in the light of American experience with somewhat shorter ones), McIntosh shows that in five years a good fieldworker would be able to complete little more than two hundred fifty interviews, a far from adequate sample for Scotland. Therefore the fieldworker should collect data on fewer items—most of them dealing with matters of pronunciation—and visit more communities.

Since the fieldworker will be recording chiefly pronunciation, his questionnaire should be designed to record as many known or suspected phonetic differences as possible. McIntosh recognizes three types of pronunciation differences, as Kurath and I do in our forthcoming volume on American pronunciation: (1) differences in the incidence of the phoneme—is the syllabic of *stone* /o/ or /e/ or /i/? (2) differences in the pronunciation of the phoneme or one of its allophones—what is the phonetic quality of /r/ or of intervocalic /t/? (3) differences in the phonemic system—have OE /hw-/ and /f-/ fallen together? In discussing attempts to answer these questions, McIntosh is not afraid to use technical concepts, but explains how linguists operate in terms of the phonemic principle, which assumes there are fixed structure-points in the sound system of every informant. Since no one list of words can anticipate all contrasts, it may be necessary to add items to the questionnaire in order to handle local problems.⁹ Furthermore, the field-

tionnaire for field investigations. Orton and Dieth spent five years on their questionnaire before they published it; within five years after the American atlas was first proposed, the fieldworkers had completed the survey of New England and Lowman had undertaken his preliminary survey of the South Atlantic states. The most carefully prepared questionnaire must allow for modification in the light of field experience: as Gillieron himself recognized, a perfect questionnaire can be made only after the dialects have been exhaustively investigated.

⁸ Of course, a lexical variant sometimes yields valuable information on the phonemic structure of the dialect. In American English there is lexical variation between *toadstool*, *frogstool*, and other terms. In New England, many informants preserve the old-fashioned monophthongal or in-gliding 'New England short o' [œ] only in the initial syllable of *toadstool*, while (for these informants) the up-gliding diphthong [ou] occurs in the simplex *toad*. Thus the complete picture of the phonemic system of such dialects depends on the pronunciation of *toadstool*, which had been included in the questionnaire primarily as a lexical item. See the forthcoming dissertation of W. S. Avis, *The mid vowels as in take and road* (University of Michigan, in progress).

⁹ *Cot* 'small bed' is not a part of the questionnaire for any of the American regional atlases. But in areas where the field worker suspects that some informants lack the contrast between unrounded and rounded low-back vowels (western Pennsylvania, northern Michigan, Minnesota, western Canada) the fieldworker may use *cot* for comparison with *caught* or *tot* with *taught*. Other potential contrasts may elude even the most alert fieldworker and require independent investigation; see Sumner Ives, *American pronunciation in the Linguistic atlas, Tulane studies in English* 3.179-91 (1952); H. A. Gleason, *Introduction to descriptive linguistics* 28-34, 213-4 [mimeographed edition] (Hartford, 1953).

worker should be allowed to comment on his practice and to point out where he may have failed to adjust adequately to the differences between two closely related neighboring dialects.¹⁰ The fieldworker thus assumes a more important role than that which Gilliéron assigned him.

As the phonetic investigation depends on fixed points in the sound system of the informant, the investigation of vocabulary depends on the choice of fixed points in the informant's experience of the external world. Although no informant will know everything that one might care to ask about, the lexical items chosen should be common to the experience of most. (Sometimes, it is true, a culture-item of limited distribution, such as the Charleston *joggling board*, may be critical for determining the extent of a dialect area.) Assuming a skilful fieldworker, the lack of a response may have one of two causes: the item may not occur, as coasting is unknown in the flat and snowless South Carolina coastal plain; or the item, though it occurs, may lack a name, as the familiar strip of grass between the sidewalk and the street—a *treelawn* in Cleveland, a *boulevard* in Minneapolis, a *parking* in northern Illinois—has no name in the South Carolina piedmont. A useful lexical item should have a number of regional or social variants; *caboose* would be unprofitable in the United States, since all railroads—and all people familiar with railroads—use only that term. On the other hand, a term may have such an overwhelming number of variants, with very slight connotative differences, that they reflect no pattern of regional or social distribution. Early in the New England field investigation, the questions calling for synonyms of *prostitute* and *intoxicated* were dropped; and in the Rocky Mountain area, contrary to local expectations, *gun* and *silver dollar* proved unprofitable.

Besides making sure that the lexical items in his questionnaire provide useful information on the distribution of linguistic forms in Scotland (the correspondence questionnaires were first tried on a group of Edinburgh undergraduates), McIntosh is concerned with the problem of making his data comparable with findings in other parts of the English-speaking world. Many Scots terms will not appear in the United States; conversely, neither the peanut nor the poor white is a normal part of Scottish culture. But there are so many valid comparisons in such activities as housekeeping, cooking, animal husbandry, and children's games, that since his last visit to the United States in 1949, McIntosh has often sought the advice of American linguistic geographers on the makeup of his questionnaires. One of these questionnaires consists largely of lexical items; but it includes a few items of morphology and some dealing with problems of phoneme-incidence and phonemic contrast.¹¹

Since McIntosh is writing for the layman as well as for the specialist in linguistic geography, he is careful to answer the questions which an amateur might

¹⁰ The fieldworker can avoid some of these problems by zig-zagging across suspected dialect boundaries rather than working parallel to them.

¹¹ Lest this seem a retrogression to the discarded techniques of the German atlas, we should remember that Wenker asked for PHONETIC data from what amounted to 40,000 miscellaneous fieldworkers at a time when the phonemic principle was not yet an axiom of linguistics. If the test words are carefully chosen, most informants can furnish PHONEMIC data—as whether *creek* has the syllabic of *leek* or *lick* or *lake*—without having to use any phonetic symbols.

raise. Though he is particularly concerned with getting records from the relatively old-fashioned informants, since their type of speech is disappearing, he is well aware that 'old-fashioned' and 'modern' are relative terms—that resistance to innovations varies not only from place to place (being much stronger, say, in the Outer Hebrides than in a suburb of Edinburgh) but from speaker to speaker, and even from occasion to occasion in the same speaker. One need not be distressed by this variability: it is a part of the normal linguistic situation in any period, though of course it is accentuated in times of rapid linguistic change. The question whether men or women make better informants—a question that seems to worry a number of linguists¹²—McIntosh dismisses as irrelevant. Toothlessness, too, he feels need not disqualify an otherwise satisfactory informant; it may be a normal characteristic of elderly uneducated informants in isolated rural communities, just as continual tobacco-chewing is a normal part of the conversational situation for some American social groups. McIntosh recognizes, furthermore, that an interview is not a mechanical process. Though an experienced fieldworker tries to control the contexts in which questions are asked and answered, and though he knows the cues that are most likely to set up satisfactory responses, he does not hold the interviews to rigid patterns, but manipulates the sequence of questions according to the informant; the less formality the better, especially if the investigator wants to get trustworthy grammatical data. As American students have found out, the fieldworker (or the person through whom the questionnaires are distributed) is likely to obtain the highest degree of cooperation if he appeals to local or regional pride; McIntosh, by stressing the importance of the data as an aspect of Scottish history and Scottish culture, has had excellent press relations and full cooperation from the Scottish Office of Education. As for publication, McIntosh rejects as too expensive the older method of hand-lettered maps, like those that appeared in the French, the Italian, and the New England atlases. Instead, he favors publication in the form of tables.

Lest some of his readers be disappointed at obvious omissions in the final survey, McIntosh is careful to explain that he does not expect to say the last word or get anything approaching all the data. This observation is particularly applicable to the study of grammatical variations. With care, one may assemble comparative morphological evidence as easily as lexical, but linguists have not yet devised satisfactory procedures for a thorough and systematic investigation of differences in syntax. Before undertaking such an investigation on a national scale, McIntosh advocates local pilot studies to experiment with different techniques. As methods develop, many kinds of future dialect studies may be undertaken: comparative studies of intonation and voice quality, detailed analyses of the sources and levels of usage in the speech of a region, and systematic descriptions of local dialects. (Phonological descriptions are few enough, and in their descriptions of English dialects linguists have rarely gone further.)

The most important question to raise at this point is the validity of evidence gathered by correspondence, since so much of the Scottish material will be of this sort. Any method, obviously, can fail if handled clumsily; the correspondence

¹² See the discussion in *Orbis* 1.10-86 (1952), and my review in *IJAL* 19.246-9 (1953).

method, also obviously, is less effective for some kinds of vocabulary items than others, and would probably be less effective in the United States than in Scotland. But even American experience has shown that a carefully prepared questionnaire, with careful indoctrination of those through whom it is distributed, enables the investigator to derive useful data through correspondence. In the Great Lakes area, A. L. Davis found that a selected multiple-choice questionnaire distributed to a limited number of communities gave essentially the same picture of dialect patterns that the preliminary field work had revealed.¹³ In the upper Midwest, H. B. Allen has supported his two hundred field interviews with a thousand multiple-choice checklists; they have made the evidence from field records much more meaningful than it would otherwise have been.¹⁴ D. W. Reed has had equal success with checklists in California; he has found them particularly useful in setting up the network of communities for field work. In Wisconsin, F. G. Cassidy has relied almost exclusively on correspondence for his study of the folk vocabulary.¹⁵ Of course, one must not lump together correspondence data with field material, but present each type separately. In this way the reader can see for himself if the two techniques yield different results.

Perhaps because McIntosh wished to simplify his statements for the benefit of the layman, a few of his observations need qualification. No matter how carefully devised, a correspondence questionnaire cannot probe for lexical variants in the way that a fieldworker can; if the first response (as often in the Middle West) is a general term, the fieldworker can modify the form of his question. Even in Scotland, I suspect that the correspondence method will prove only partially successful in probing for grammatical variants; though their tradition of 'correctness' is less vigorous than the American (and meets a stronger resistance from pride in local usage), notions of propriety and the prestige of the standard language may still inhibit informants from writing down what they actually say.¹⁶ No matter how hard one tries to focus a questionnaire on pronunciation alone, some items will turn out to have lexical variants in some localities. Thus the word *goal* ('place to which children run in certain games') was included in the New England atlas for its pronunciation; but in other parts of the Atlantic Seaboard the same question usually called forth *base*, *bye*, *hunk*, *line*, or *den*. A minor point: McIntosh's use of *informer* (74) where Americans would use *informant* is an unpleasant reminder of the public honors recently rendered those who have made profitable careers out of testifying against their neighbors; but since the Scottish law term for professional informing is *delation*, perhaps

¹³ *A word atlas of the Great Lakes region*, diss. University of Michigan (microfilm; Ann Arbor, 1949).

¹⁴ The linguistic atlas of the upper Midwest of the United States, *Orbis* 1.89-94 (1952).

¹⁵ See Cassidy, On collecting American dialect, *American speech* 23.185-93 (1948); Cassidy and Audrey R. Duckert, *A method for collecting dialect* (Publications of the American Dialect Society, No. 20; Gainesville, Fla., 1953).

¹⁶ Informants are reluctant to write a linguistic form which they have been taught is incorrect, no matter how often they use it in conversation. A striking example is the complete absence of *ain't* in the Vulgar English materials examined by Fries for his *American English grammar* (New York, 1940). In field interviews I have often heard informants deny using, and stigmatize as illiterate, forms which I had recorded several times in their conversation.

informer does not have the associations for McIntosh that it has for Americans and Irishmen. Finally, the repeated use of Scottish place names makes me wish that McIntosh had included a map showing the location of every place mentioned in his book; though such names may be no problem for Scotsmen, Peebles and the Black Isle are as strange to most Americans as Orangeburg and the Juniata would be to the average Scot.

But these are small objections. The book as a whole is intelligent and lucid. Readers interested in Scottish dialects, scholars and amateurs alike, will look forward to the appearance of the Scottish atlas and the other monographs in this series. And American linguists writing for a popular audience could profit by McIntosh's example.

Words and ways of American English. By THOMAS PYLES. Pp. ix, 310. New York: Random House, 1952.

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The lack of sound popular treatments of linguistics is all too well known. The most important descriptions of our science—for example those by Sapir, Bloomfield, Jespersen, and Sturtevant—do not attract the general reader, or even the general scholar; the books on language written expressly for a popular audience, like those by Bodmer, Pei, and Laird, distort the methods and achievements of linguistics even if they do not deny outright that linguistics is a science. For this reason Pyles's treatment of American English should be of interest, as dealing soundly and interestingly with linguistic materials.

To his task Pyles brings not only training in English and Germanic linguistics and awareness of current research (he is the present secretary of the American Dialect Society) but an admiration for the work of H. L. Mencken in promoting popular interest in American English. His acceptance of the methods and aims of linguistics is attested by adverse criticism from Harry Warfel, one of his colleagues at the University of Florida: Warfel's *Who killed grammar?* (Gainesville, Fla., 1952) lists Pyles with Fries and Hall as an arch-enemy of traditional normative grammar. Pyles has drawn freely on major research of the last two decades, such as the *Dictionary of American English*, M. M. Mathews' *Dictionary of Americanisms*, and the group of studies derived from the *Linguistic atlas*. Like any good popularizer, he makes no attempt to promote theories of his own; he keeps himself in the background, weighing and digesting and synthesizing what others have done. What he has produced will not be new to linguists, but should be interesting to the layman and very useful to English teachers, who need to be made aware of what scholars have found out about the language.

The arrangement of the book is not what I would personally have chosen, but is understandable in view of the general public's ignorance of linguistics. Pyles treats grammar incidentally, devotes one chapter primarily to pronunciation, and lays his greatest emphasis on matters of vocabulary. But even in this treatment Pyles avoids the common fault of presenting an Old Curiosity Shop of quaint and curious items. Instead, he repeatedly emphasizes that American

English, whatever we may think of it, is a product of the economic, social, and cultural history of the American people.

The treatment of the vocabulary contains little that would be unfamiliar to the reader of Mencken, except as recent publications like the *Dictionary of Americanisms* and Kurath's *Word geography* have made possible more complete and more exact treatments of familiar themes. Pyles again reminds the reader of the debt the American vocabulary owes to various other languages, both aboriginal and immigrant. He cites numerous examples of frontier and political tall talk, of slang and trade names, of differences between British and American usage, and of the well-known attempts of some Americans to achieve social status by dignifying humble occupations with high-sounding names. His statements are generally briefer and more direct than Mencken's, and reflect more accurately the findings of recent scholarship; it is gratifying to see one book that does not perpetuate the myth of a 'General American' dialect, but accepts the position of the Midland speech-area as Kurath and others have pointed it out.

In contrast with the entertaining but uncontroversial treatment of the vocabulary, Pyles's discussion of grammar and pronunciation is almost certain to provoke some lay dissent, though linguists will recognize its essential accuracy. Unlike the vigorous, independent, imaginative, and adventurous American spirit of popular myth, the typical American attitude on linguistic questions is presented unflatteringly as timid, conservative, pedantic, and normative. This attitude is manifest, Pyles contends, in the popular judgment of education—a judgment which highly values enrollments, building programs, and financially remunerative skills but has little concern with stimulating the intellect; a judgment which encourages not the inculcation of new ideas and attitudes but the more elegant expression of what is time-honored, safe, and respectable. It is understandable, therefore, that many Americans have an almost superstitious veneration of linguistic authority, especially as manifested in dictionaries—a veneration not discouraged by dictionary publishers, two of whom now advertise their respective wares as 'the supreme authority' and 'the new authority'. On a higher level the U. S. Board of Geographic Names shows the same timidity in its practice of discouraging the use of Anglicized foreign names and promoting the use of the German, Italian, Russian or other 'local' form, no matter how familiar and well established the Anglicized form may be; whereas French geographers, for instance, freely adapt non-French names to French conventions. The same timidity, with overtones of snobbery, is found in the practice of radio announcers in attempting to seem cultured by pronouncing foreign names as little as possible like English names, even if the resulting pronunciation has no resemblance to the way such names are pronounced in any language—as [šta'lin] for *Stalin*. A particularly widespread manifestation of this timidity is the trend toward spelling-pronunciations, far stronger in the United States than in Great Britain. This trend is characterized as typical of an age of widespread partial literacy, when a misguided pride in spelling as a social accomplishment is most likely to flourish (246). In sum, it appears that in linguistic as in political and moral problems, the American tends toward a sharply drawn dichotomy between

correct and incorrect, between good and evil, however little his precepts may be reflected in his actual practice.

The villain of Pyles's book is Noah Webster, 'smug, self-assured, and pugnacious in his pedantry as in his Puritanism and his patriotism' (94). Webster, Pyles reminds us, had little knowledge of linguistics, even for his day, but his air of authority and his skill in definition enabled him to exert a strong influence on American attitudes toward grammar and pronunciation. Basing his judgments on analogy rather than usage or linguistic knowledge, Webster favored the practices and appealed to the prejudices of the middle class rather than of 'the cultured and well washed' (107). To his influence may be at least partially ascribed the American retention of secondary stresses where British English has largely lost them, in such words as *dictionary* and *secretary*, and such over-careful pronunciations as *Tuesday* and *borrow* with full diphthongs in the weak-stressed syllables (103-4). This rough though not unjust treatment of Webster could understandably make Warfel, a biographer of Webster, look upon Pyles as linguistically subversive. But Pyles is careful to point out that whatever Webster's faults were, he did not create but only epitomized an authoritarian tradition that would have grown up even if Webster had never lived, a tradition that could not have become established unless Americans had been willing to accept it. Language attitudes reflect the mores of the community which uses the language.

The consistent emphasis on the social nature of language, and on the relationship between dialect differences and cultural differences, is one of the strong points of the book. Pyles rightly attributes the preservation of Africanisms and older dialect forms in Southern Negro speech, not to any peculiar Negro qualities of mind but to the cultural isolation of the Southern Negro. He emphasizes the geographical and social mobility of American cultural groups, and attributes the prevalence of authoritarian linguistic attitudes to the social insecurity of newly risen groups. Most important, he stresses, like Fries, the fact that in linguistic matters (as often in political matters) the so-called 'liberal' is the true conservative, defending a long and honorable tradition (here, the fact that standards of cultured speech are based on actual cultured usage) which another group is wilfully trying to subvert. Better than any other recent popularizer, Pyles reflects the considered judgment of linguists on questions of usage.

One can find flaws, of course. *Half after four* (one of the few items which is not entered in the very good word index) is far from 'common in America' (25). *Firewater* is not 'supposedly' but actually the literal translation of the name for hard liquor in some Algonquian languages, as well as in some languages of Southeast Asia (175). In most parts of the Midland and South, the syllabic of *hog*, *fog*, *frog* is not that of *law* 'cut short', but often even more diphthongized (267). *Mackinaw City* is spelled to reflect the pronunciation, though *Mackinac Island* is not (38). *Jazz* 'copulate with' is not confined to a 'low social level', but is a common middle-class euphemism in the South (47). The most widespread regional synonym for *haycock* is not *haystack*, which usually describes a larger pile of hay, but the Southern and South Midland *hay shock*; it is doubtful if this

is to be considered a euphemism. The evidence of Kurath's *Word geography* (80 and fig. 162) would suggest that *belly-buster* is far older than the 1890's (189). As applied to Americans of Japanese descent, *Issei* and *Nisei* designate only the generation of American residence, with no imputations of loyalty or disloyalty (213). Far from being a Britishism, *sitting room* is common American, though perhaps a bit old-fashioned (213). As a Marylander, Pyles is understandably reluctant to include Mencken's *Baltimoron* in his list of blends, though he includes *Chicagorilla*. I question if Americans today are given to an 'uninhibited use of profanity' (139); two pages later Pyles concludes that American profanity is relatively colorless. Though Bishop Lowth was one of the authoritarians, he at least recognized that the English noun has only two case-forms.¹ A reading of Olmstead's *Cotton kingdom* would dispel the myth that the pre-Civil-War Southern plantation provided 'the most charming form of life that our country has ever known' (62). A few footnotes and at least a skeleton bibliography would have been helpful; a popularization should not be an end in itself but a stimulus to investigate further. The discussion of pronunciation could be more systematic, and would be much better if a phonemic or phonetic transcription had been used: for English, respelling is not enough, and phonetic symbols are not difficult to understand. And though Pyles's style is generally easy and cultivated, a reader could be spared such Menckenisms as the gloss on *bowie knife*: 'the principal instrument of non-surgical phlebotomy in the American Southwest' (167).

But these faults are only incidental, not organic, in an honest and generally successful attempt to introduce the general reader to American English as a mirror of American life. The specialist in American English will find little new, but the ordinary teacher and the layman will find ample matter to teach and delight.

A survey of verb forms in the eastern United States. By E. BAGBY ATWOOD. (Studies in American English, No. 2.) Pp. viii, 53, with 31 maps. [Ann Arbor]: University of Michigan Press, 1953.

Reviewed by ALBERT H. MARCKWARDT, *University of Michigan*

This book is a companion volume to Hans Kurath's *Word geography of the eastern United States*; both monographs are based on the materials of the *Linguistic atlas of New England* plus those of the unedited atlases of the Middle and South Atlantic States, a corpus of some 1,400 field records in all. The two studies together give us a survey of the distribution of pertinent lexical and morphological items from Maine to Georgia, and from the Atlantic coast to the Appalachians and even beyond. It is fortunate indeed that the Atlas materials are being made available for scholarly analysis and interpretation. A monograph such as Atwood's constitutes one of the most compelling arguments for finding the funds necessary to complete a regional survey of the United States.

From another point of view, Atwood's work may be considered complementary

¹ For the reappraisal of Lowth, see Priscilla Tyler, *Grammars of the English language to 1850* (Western Reserve University diss., 1953).

to the *American English grammar* of C. C. Fries. Many of the nonstandard items which turn up in Fries's grammar are also to be found in Atwood's survey, but the reader soon discovers that they are distributed on a regional as well as a social basis. Atwood carefully makes the point that regional factors must be taken into consideration in dealing with the American vulgate.

The techniques of collecting the materials upon which this survey is based are so well known that they need not be described here in detail. The 1,400 informants fall into three groups: I, the older generation, generally poorly educated; II, the middle-aged generation, somewhat better educated; III, the 'cultured' informant, of superior education. In interpreting his data, Atwood wisely takes into account the varying practices of different fieldworkers in the selection of informants. Recognizing that the records also 'reflect differences in temperament, interest, and interviewing practices' of the fieldworkers, he has carefully limited his evidence to those forms which appear from the records to be spontaneous responses. In short, due caution has been observed in the selection and presentation of material.

In general Atwood finds that the distribution of verb forms follows that of the lexical items, namely into Northern, Midland, and Southern regional types, although, as he is careful to add, 'The geographical lines formed by many (if not most) of the verb forms are not at all clear and sharp. Often we are dealing with recessive forms that tend to thin out or disappear in certain areas and to be preserved more fully in others.' There is ample support here for Kurath's recognition of Midland as a major unified speech area despite the fact that 'The area is more clearly marked by the absence of popular verb forms than by characteristic usages of its own.'

On the whole the subdialect areas are not clearly marked. The past-tense form *waked* offers about the only instance of a well-defined division between eastern and western New England. There are but two forms which coincide with the outlines of the German area of Pennsylvania. In the South, subsidiary areas emerge somewhat more clearly, even though the number of isoglosses setting off the various districts is not large. The results would seem to emphasize the wisdom of the course that has been followed here, of first getting at the major dialect areas through an analysis of the lexical items and then considering the phonetic and morphological materials in the light of these findings. Nevertheless, despite the somewhat unsatisfactory nature of the sub-regional divisions, the author is amply justified in concluding that 'the idea that there is a uniform grammar of the American "vulgate" must be abandoned. What we actually have is a variety of regional dialects, each with its own set of grammatical forms, as well as its peculiarities of pronunciation and vocabulary.'

Atwood finds verb usage rather sharply divided along social lines, more so than vocabulary and pronunciation. Yet there are no items among the approximately ninety analyzed in this monograph which are exclusive in their distribution—that is, used by only a single class of informant. There are a considerable number which are primarily rustic and an even larger group which may be classified as 'popular regional', extensively used by types I and II but relatively uncommon among the cultured informants. In this connection it is worth noting that all

forms widely current among the Negroes were 'also in use among the more rustic of the white informants'.

Although Atwood disclaims any attempt at an extensive historical interpretation of his findings, he does make the point that a large number of the non-standard forms are archaic retentions of what was once standard English, and that many of them are still current in British dialect usage. There are few innovations, and virtually none of demonstrably American origin.

In some instances it is possible among the members of the various verb classes to observe particular tendencies which are not pointed out in the study itself. Reflexes of the Old English 7th class of strong verbs (*blow, grow, know, throw*) tend to adopt the regular weak inflections, particularly among informants of class I. Verbs which belonged to the Old English 4th class show a loss of *-n* in the past participle, a development of particular interest when it is remembered that the stressed vowel in the preterit of such verbs as *steal, tear, and wear* was also transferred from the past participle. Verbs of the 5th class all show a tendency toward the same form in all principal parts. Indeterminate verbs (*fit, knit, sweat*) seem, to some extent, to be going over to the regular weak conjugation.

A few of Atwood's findings are somewhat unexpected. It is surprising to see *broken* predominant among type-II speakers. So also is the rarity of *caught* in western New England and New York State. In the discussion of the forms of *drink* there is no mention of the taboo against *drunk*, which many believe to have been partially responsible for the development of *drank* as a past participle. *Hanged* is somewhat less frequent even among cultured informants than might have been anticipated, and the predominance of *don't* in the 3d singular of the present indicative is equally surprising.

The cartographical presentation is excellent throughout. Whether or not the alphabetic order is most satisfactory is an open question. Verbs belonging to the same conjugation, historically considered, could have been placed together. Indicating the work-sheet page number after the verb headings is probably of less help to the average reader than would have been a reference to the map on which the item is charted. But these are minor matters, which do not detract from an excellent presentation.

Chūgoku gogaku kenkyūkai ronshū [Papers of the Society for Chinese linguistic studies]. Pp. 113. Nara, Japan: Chūgokugo kenkyūkai, 1953.

Reviewed by ROY ANDREW MILLER, *University of California*

Sinologists and others concerned with studies in Chinese history and culture have long realized the danger of ignoring the considerable contributions to these fields by the Japanese. The present volume of studies in Chinese linguistics by Japanese scholars shows that the linguist too must take notice of the Japanese work relating to his discipline. A few comments on each of these studies in turn (except for one that has no linguistic interest) will serve to introduce the contents of this volume, which offers a fair sample of the high quality work being done in this area in Japan today.

Ôta Tatsuo, *Rôkittai no gengo ni tsuite* (On the language of the Lǎo-ch'ī-tà, 1-14) is an introduction to a new literary source for the Chinese language of the Yüan-Ming periods. The Lǎo-ch'ī-tà [3833, 564, 5943]¹ is a textbook on Chinese widely used in Korea, and published there in the period 1423-34. After a philological introduction to the problems of the transmission of the text, Ôta discusses its language in detail, comparing it with the Chinese translation of the *Secret history*, and with the Yüan theatrical pieces. Ten points are dealt with: (1) pronouns: in contrast with the 33 different pronouns used in the translation of the *Secret history*, and the 12 in the theatrical pieces, the Lǎo-ch'ī-tà uses only eight: *wǒ* [4778], *tsá* [6645], *nǐ* [4649], *t'a* [5961], and their plurals with *.men* [4419], i.e. *wǒ.men* etc.; (2) *kěi* [482]; (3) *.le* [3958]; (4) *hsüdn ... hsüdn* [2894, 2894]; (5) the pluralizers *.men* [4419] and *.mei* [4401]; (6) *jih-t'ou* [3124, 6489]; (7) *yǔ* [7533] as a phrase-final auxiliary; (8) *shàng-t'ou* [5669, 6489]; (9) *pù-chien* [5379, 846]; (10) *pù ... nà shén.ma* [5379, 4604, 5724, 4540]. Ôta concludes that the language of the Lǎo-ch'ī-tà, especially in its grammar, is on many points identical with that of the Yüan, but in addition has some features closer to the Peiping colloquial of the Ch'ing than to Yüan usage. His point that the style of such texts as the Chinese translation of the *Secret history* can hardly be attributed to any influence upon Chinese by foreign languages, but rather simply represents the appearance in our texts of far-reaching changes inside Chinese itself, is an important one.

Ogawa Tamaki, *Tôinto to Inkai Kyôgen: Tôdai on'inshi no ichisokumen* (Rime tables and the Yün-hai Ching-yüan [7757, 2014, 1137, 7728]: One aspect of the history of phonological studies in the T'ang, 25-34) is a study of the earliest appearance in China of rime tables, as opposed to rime books, of linguistic interest since the materials with which it deals, and their dating, are vital for the study of Chinese historical phonology. The author, in a well documented paper which shows a wide familiarity with the literature, attempts to demonstrate that rime tables were in use in China by the 8th century.

Osada Natsuki, *Soshûgo on'in taikêi no shotokuchô ni tsuite* (On some characteristics of the phonological system of the Soochow language, 35-50) utilizes mainly the data of Y. R. Chao's *Hsien-tai Wu-yü te yen-chiu*,² together with other older sources, to present a classification of the Soochow dialects, and examples of the sound correspondences between them. The paper is to form part of a projected series of studies in Soochow literature and language, with a dictionary of the Soochow language, underwritten by the Mombushô.

Tanaka Seiichirô, *Hei ji ni tsuite* (On the character *pîng*, 51-8) attempts the description of the semantic range of *pîng* [5292] in modern Mandarin in such expressions as *chên.te, chê pîng.pu.shih hsiao.hua* 'That's hardly anything to laugh about!' [297, 6213, 264, 5292, 5379, 5794, 2615, 2215]. A novel feature of the paper is the utilization of modern Chinese translations of Japanese literature as materials for the investigation. Tanaka is almost exclusively concerned here with problems of precise translation, in both directions. Though it is admittedly a

¹ Numbers in square brackets after romanized Chinese forms refer to graphs in R. H. Mathews, *A Chinese-English dictionary* (revised ed.; Cambridge, Mass., 1943).

² *Ch'ing-hua hsüeh-hsiao yen-chiu-yüan ts'ung-shu*, No. 4 (Peiping, 1928).

small matter, the author's constant employment of *ji* (= *tzü* [6942]), as for example in his title, to mean morph, morpheme, or even syntactic word, is hardly good usage; there is quite enough confusion between language and writing in his part of the world as it is, without adding to it in scholarly publication by this type of careless conformity to the popular idiom.

Tôdô Akiyasu, *Hyôgenron teki on'inron no kokoromi*: [kl], [tk], [pl] rui no arawasu keitai eishô (An attempt at phonology as expressionism: Morphological impressionism as expressed by the compound initials [kl], [tk], [pl], 59-78) is the forbidding title which introduces what is easily the most important single collection of materials on the reconstruction of the initial consonant clusters of Archaic Chinese to appear since Boodberg's Proleptical remarks, of which, unfortunately, Tôdô seems not to have heard. Tôdô has collected 106 binomial or larger expressions from modern literary texts, and 139 from early Han and pre-Han sources, which indicate the existence of consonant clusters in the archaic language. These he has classified according to the chief semantic patterns relating to shape or status of matter about which they appear to cluster, such as 'round', 'bent', 'empty', 'linked together', 'saw-toothed'. Except for this classification by rather vague semantic categories, it is a collection of materials rather than a proper study, but a collection for which every student of Archaic Chinese will be grateful. Boodberg, for example, long ago pointed out that in certain cases we have to deal with a metathesis of the components of these initial clusters, a phenomenon of great interest to historical investigations involving Chinese and Tibetan. When we reconstruct initial clusters in Archaic Chinese solely by means of the phonetic-compound graphs, we can never be sure of the sequence of the components of these clusters: for the phonetic-compound graphs, ***lk-* is as acceptable as ***kl-*. The only control is the binom: **kanglang* points to ***kl-*, but **langkang* to ***lk-*. Tôdô's collection of these binom controls, each carefully documented as to source, should do much to resolve some of the problems in these reconstructions. A hasty inspection of his materials shows that, in binoms from the old texts, the possible sequences appear as follows: (1) **Gl-* 32, **lG-* 8; (2) **GD-* 12, **DG-* 10; (3) **Bl-* 52, **lB-* 10. (This last includes **Bl-* 36, **lB-* 7, and **Ml-* 16, **lM-* 3.)³ Thus the sequence with **l* in first position in the cluster was a comparatively rare but still well attested feature of the archaic language. The sources of these clusters of the order **lG-* can also be exactly determined with Tôdô's materials; five are from the *Ch'u Tz'u*, two are from *Huai-nan-tzu*, and one from the *Chuang-tzu*; none are from any of the pre-Ch'in canonical works.⁴ The **GD-* clusters are from scattered sources: one from the *Ch'u Tz'u*, one from the *Shih-ching*, the rest from works with which it is difficult to associate any special geographical connection. But in the case of **lB-*, the sources are again suggestive: *Ch'u Tz'u*, *Mo-Tzu*, and *Chuang-tzu* one each, and *Huai-nan* two;

³ In addition, under (1) Tôdô includes 14 **ll* reduplication forms, and under (2) one reduplicated form of the shape **GGDD*.

⁴ For comparison, the expressions involving **Gl-* are from the following texts: *Shih-ching* 2, *Shu-ching* 1, *Tso-chuan* 1, *Chou-li* 1, *Huai-nan* 5, *Chuang-tzu* 5, *Ch'u Tz'u* 3, *Hsün-tzu* 1, *Kuan-tzu* 1, *Lieh-tzu* 1, *Erh-ya* 3, *Shih-chi* 2, *Han Shu* 1, *Fang Yen* 1, *Wen-hsüan* 2, no source given for 2; total 32. For the importance of the *Ch'u Tz'u* here, cf. E. Erkes, *Die Sprache des alten Ch'u*, TP 27.1-11.

of the remaining five, three are from *Shuo-wen* and two from *Erh-ya* and its commentaries. With the publication of Tôdô's materials, we have come much closer to putting the reconstruction of the initial clusters of Archaic Chinese on a firm methodological basis.

Nishida Ta'ichirô, *Kambun joji no sha to sho to ni tsuite* (On the auxiliaries *chê* [263] and *sô* [5465] in literary Chinese, 79-94) is another attempt at description of semantic range, using texts up to and including the Han. Japan's long tradition of scholarship in the field of literary Chinese gives the Japanese linguist an impressive advantage in studies of this kind.

Yamagishi Tomoni, *Keitai wo shû to shite mita Chûgokugo bumpô* (A Chinese grammar stressing morphological considerations, 96-106) is a short but remarkable paper, in which the author establishes ten form-classes for the description of modern Chinese. Yamagishi's strict adherence to methods of formal analysis, and the skill with which he utilises the principles of analysis by immediate constituents, make his contribution unique. The cornerstone of his analysis is occurrence in construction with *pû* [5379]; working from this he sets up his ten form classes of what he gratifyingly calls *go* (= *yü* [7651]) or *shi* (= *tz'ü* [6971]), not *ji*: (1) nouns (*meishi*), for example *jén* [3097] 'man', *wô* [4778] 'I', *hsien-tsai* [2684, 6657] 'now', *hûo-ch'e* [2395, 280] 'train', further subdivided into (1a) ordinary nouns (*futsû meishi*) and (1b) quasi-nouns (*jün meishi*): *wô* and *hsien-tsai* are 1b, *jén* and *hûo-ch'e* are 1a; (2) secondary nouns (*fuku meishi*): *chê.ke* [265, 3366] 'this', *ch'i hui* [409, 2309] 'how many times'; (3) epithets (*jusshi*): *hào* [2062] 'good', *chü-i* [1340, 2960] 'notice'; (4) secondary epithets (*fuku jusshi*): *kao-kao.r* [3290, 3290, 1759] 'very lofty', *i-p'ien-p'ien* [3016, 5256, 5256] 'in layers'; (5) negatives (*hiteishi*): *pû* [5379] 'not', the only member of this class; (6) intermediaries (*kaishi*): *te* [6213] '-s', *kuan-yü* [3571, 7643] 'concerning'; (7) secondaries (*fukushi*): *hên* [2094] 'very', *ch'áng-ch'áng* [221, 221] 'always'; (8) connectives (*renshi*): *sui-ján* [5519, 3072] 'notwithstanding', *chü.shih* [1210, 5794] 'namely'; (9) auxiliaries (*joshi*): *le* [3958], *pà.le* [4841, 3958]; (10) exclamatories (*kantanshi*): *ai* [4], *a* [2]. Essentially, these form classes are established by the following syntactic criteria. (5) has only one member; (3) and (4) appear in construction with (5), whereas (1) and (2) do not; (1) may precede *te*, (1a) appears in construction before (2) but (1b) does not; (3) appears in construction after (5); (4) appears in construction before (3), (6) similarly before (1) and (2), (7) similarly before (3) and (4); (8) appears in construction before (3) or before larger units with a member of (3) as head; (9) follows a sequence and is in construction with the last of its members, signifying conclusion of the utterance; and (10) never appears in construction with anything else.

The chief shortcomings of Yamagishi's system of description, of which the above is only a brief synopsis, is that he has based it upon a notation system somewhat less than phonemic, namely on character texts, and thus has not had at his disposal all the significant features of the language with which he is dealing. This is a serious handicap, and an unnecessary one, all the more unfortunate because his analysis is in so many other respects impressively thoroughgoing.

The concluding paper in the volume, Rokkaku Tsunehiro, *Baihanshi no*

bunrui ni tsuite (On the classification of numerary adjuncts, 107-13), is disappointing; by 'classification', Rokkaku understands the arrangement of forms used as numerary adjuncts in modern Chinese according to their semantic relation to the nouns with which they are employed. He sets up three main categories: (1) concrete descriptions, *k'ou* [3434], *p'ü* [4829]; (2) quantity measures, *ch'ih* [1045], *p'ei* [4996]; (3) aspect descriptives, *ch'en* [322], *ch'ang* [218].

With this first volume of its *Ronshü*, the Chügokugo kenkyūkai has made an impressive beginning, which augurs well for the future of linguistic studies in Japan, both Chinese and general.

Linguistic bibliography for the year 1950 and supplement for previous years.

Published by the Permanent International Committee of Linguists with a grant from the United Nations Educational Scientific and Cultural Organization. (Published under the auspices of the International Council for Philosophy and Humanistic Studies.) Pp. xxviii, 275. Utrecht and Antwerp: Spectrum, 1952.

Reviewed by GEORGE L. TRAGER, *Georgetown University*

This publication follows three previous volumes—one for 1939-47 (in two parts), one for 1948 with supplement for 1939-47, and one for 1949 with supplement for previous years. It is edited by Christine Mohrmann at Nimeguen; the collaborators were J. J. Beylsmit (Amersfoort), Ludwig Bieler (Dublin), Paul Burguière (Istanbul), R. Lagas (The Hague), Veikko Lehtiranta (Helsinki), Emilio G. Peruzzi (Florence), and Václav Polák (Prague). Others who sent in data were W. Brandenstein, Lokesh Chandra, Manfred Mayrhofer, B. Pottier, J. B. Rudnyčkyj, and T. A. Sebeok. Unesco, as before, gave 'moral and financial support'.

There is a list of abbreviations of periodicals (viii-xxvii) which might well come to serve as the basis for a standard list; for such a purpose it would probably be desirable to remove from the abbreviations all letters with the less usual diacritical marks, to transliterate all Greek titles instead of just some, to agree on a uniform transliteration for cyrillic alphabets, and to devise abbreviations for titles now unabbreviated.

The material is listed under some general headings, and then by language families, with subdivisions under each. In the table of contents and in all the headings, everything is in both French and English; this results in such sequences as 'Onomastique—Onomastics A. Généralités—General Works B. Europe—Europe ... 2. Toponymie—The Study of Place-names 3. Anthroponymie—The Study of Personal Names ...' This is not only wasteful, since surely all users of such a work can handle both French and English, but it is also on occasion irritating. Why not 'Toponymics' as the English for 'Toponymie'; why 'Groupe iranien' with only one capital letter and no article, but 'The Iranian Group' with capital *g* and definite article?

On most of the pages there are 20 to 25 entries. This means that some 5000 to 6000 articles and monographs are mentioned, not counting the reviews that are indicated in many cases. Such a long list is obviously not one to be read.

All the user can do is to run down the pages dealing with works on topics he is interested in (which he can find in the table of contents), and note what catches his eye. If he knows an author's name, he can look in the index and then see what the author has written, if anything. But if he knows only a title, there is no way to find it. And if he wants to find works on a particular and narrowly specific subject, he can only look through long lists under general headings, hoping to find what he seeks.

It is the reviewer's belief that traditional bibliographies such as this one are minimally useful and maximally inefficient. Too much time has to be wasted to find anything, mere titles (even with translations or one-line comments) are very often of little use as a guide to contents, and all kinds of clutter is in the way when one wants to find a specialized article on a specialized subject. When a bibliography for a single year is this big, and still incomplete, it is clear that publication in the field of linguistics is much too extensive already to be handled in this way. These comments are not to be taken as blaming the compilers: given the resources at their command, they have done a wonderful job. But some way must be found to do the much better job that needs to be done: if the linguistic scientist is to keep abreast of the work in his field, he must have a real bibliographical and abstracting service. All listings should be accompanied by abstracts (standardized as to length for various kinds of works) prepared preferably by the authors. However these are listed—alphabetically or by subject—there should be an index listing them in the alternative way. Moreover, an adequate bibliographical service should be on cards that can be filed, with only the indexes printed in the ordinary way. The subject listing should be provided with a detailed and closely subdivided system of headings—or, better yet, a number-and/or-letter notation with an index of headings.

The reviewer strongly urges that before too many more volumes like this one appear, concerted action be taken to supply the kind of service just described. He knows that, as far as he himself is concerned, he will regretfully put this book away in an appropriate place on his shelves, and continue to make his own classified card file for what he reads and comes across in linguistics and related fields. This is a great waste of time, but it is the only way he knows to find what he wants when he wants it. Many of his colleagues must share his reactions to printed books of bibliographies, for few really use them; they would probably welcome something different.

Of its kind, then, the *Linguistic bibliography* is excellent. But its kind is not very up-to-date or efficient.

NOTES

MILES LAWRENCE HANLEY died 4 February 1954, aged 60. He was a member of the Linguistic Society of America from 1929 to 1950.

He was born in Xenia, Ohio, in 1893. His undergraduate study was at Wittenberg College (B.A. 1914); his graduate work was done at Ohio State University (M.A. 1916) and Harvard University (M.A. 1927). During World War I he served in the U. S. Army as second lieutenant. His teaching appointments were at Kansas, Northwestern, Texas, and Harvard-Radcliffe, and from 1927 to his death at the University of Wisconsin. From 1931 to 1934 Hanley was Associate Director of the Linguistic Atlas, serving as Lecturer at Yale and Harvard. He was Secretary-Treasurer of the American Dialect Society from 1928 to 1940.

Hanley's professional energies went into his teaching and the assembling of data in large collections. During the gathering of the Atlas material, he made some 1500 phonograph recordings of American speech at a time when such a task was even more laborious than it would be today with modern equipment. (It was in connection with this enterprise that he suffered injuries in an automobile accident, the effects of which increasingly hampered him to the end of his life.) Another work of compilation (with Martin Joos) was the word-index to Joyce's *Ulysses* (1937, 1951). The most important collection of his last years was an index of English rimes from 1500 on.

Beside these depots of research materials, Hanley's monument is his stimulation of students and colleagues. His information was uncommonly extensive; his understanding of implications was quick and subtle; his expositions—whether from the podium or in relaxed conversation—were exhaustive, trenchant, and memorably phrased. Many American linguists have had their professional directions profoundly changed and enriched by what they learned from Miles Hanley; indeed, but for him, many of them would never have become linguists at all. A list of the publications which exist because of Hanley's stimulation, or are what they are because of his teaching and comments, would be an impressive bibliography.

R-M. S. HEFFNER

W. FREEMAN TWADDELL

DIE INDOGERMANISCHE GESELLSCHAFT, founded in 1912 and suspended with all other German societies by order of the occupation authorities in 1945, was re-established on 2 September 1953 at a meeting of Indo-Europeanists in Munich. A set of provisional bylaws was adopted, and two executive committees, an inner and an outer, were elected. The inner committee consists of the following officers: President, M. Leumann (Zurich); Vice-President, W. Brandenstein (Graz); Secretary, G. Redard (Bern); and Treasurer, A. Scherer (Heidelberg). The outer committee consists of twelve scholars from ten European countries. The Society hopes to establish a new journal devoted to critical bibliography; but this must wait until the Society has enough members to assure financial support to the undertaking. Linguists who want further information may address the Secretary

(Professor G. Redard, Kramgasse 17, Bern, Switzerland) or any other member of the inner committee.

ROBERT A. HALL JR., on leave of absence from Cornell University to study the pidgin languages of Australasia, has written the following letter, dated from Wewak, Territory of New Guinea, 23 May 1954, with the request that it be published for the information of linguists.

Recently the problem of Pidgin English in the Territory of New Guinea has been the subject of public discussion, especially as a consequence of the condemnation of Pidgin by the United Nations Trusteeship Council in July, 1953. Misunderstanding of the situation is widespread. In many quarters it is not realized that Melanesian Pidgin is a true language, with a grammatical structure of its own—as I have shown in my *Melanesian Pidgin English: Grammar, texts, vocabulary*, (Baltimore, 1943), nor that it fulfills an essential function in Melanesia, by facilitating communication between natives who would otherwise have no language in common, since there are hundreds of different languages in the Territory.

This failure to analyze the situation accurately is due, at least in part, to the name of the language. Both parts of the name *Pidgin English* are misleading. The word *pidgin* suggests to most people a kind of linguistic hash, a formless medley or broken jargon; it is also often confused with 'Pig Latin'. The inclusion of *English* in the title leads one to expect a similarity to English, and occasions disappointment and condemnation when it is discovered that the language is really very different. In fact, Melanesian Pidgin is no closer to English than French is to Latin.

This being the case, a change of name would seem appropriate. In other instances, the name of a language has been a major factor in influencing public attitudes. In Papua, a pidginized variety of a local language, Motu, is used as a lingua franca; but it is called Police Motu, and as such is not the object of opprobrium. In Indonesia, the pidginized Bazaar Malay has become the basis of the new national language, which, under the name Bahasa Indonesia, is widely respected.

For Melanesian Pidgin, likewise, a new name should be conducive to an objective attitude and to appreciation of the language's merits. The most suitable name that has been suggested so far as *Neo-Melanesian*, a name which reflects the closeness of the language to the structure of Melanesian tongues, and the fact that it is primarily a means of communication among the Melanesians themselves. In the language itself, it might be well to use the term *Tok Melanijin*, rather than the current names *tok pijin* or *tok boi*.

If the public can be persuaded to use the name *Neo-Melanesian* instead of the misleading *Pidgin English*, it will be a step towards recognition of the language and hence of the contribution it can make to the welfare of the Territory, not only in everyday communication but also in education, medicine, government, and other fields.

FRANK R. BLAKE of the Johns Hopkins University asks that the following comment be published in order to put in its proper light the review of his book that recently appeared in this journal.

In view of the unfavorable criticism of my monograph, *A resurvey of Hebrew tenses*, by Carleton T. Hodge in *Lg.* 30.177-80 (1954), I should like to make a few comments on the review. The substance of the review is that the reviewer agrees with Driver and not with the author as to the fundamental theory of verb forms, and for one scholar to disagree with another is of course normal. A reviewer of a grammatical treatise of any extent, however, may be expected to pass some judgment, favorable or unfavorable, on the numerous facts presented (as in this case) in the study; but the reviewer here confines himself to a single point, elaborated in the three pages of his review. The point criticized is discussed in the first three pages of the book and in the summation of results (77-80); in the complete absence of any mention of anything treated in pages 4-76, the question naturally arises whether the reviewer read them before preparing his review.

The points (some of them based on inaccurate quotation) which the reviewer advances in support of his single topic call for some discussion.

He speaks (178) of 'two false assumptions' that I make. First, 'that tense distinctions are fundamental in language'. No mathematical proof of this is possible, but on the other hand neither is mathematical disproof. I repeat the statement here: 'It is inconceivable to me that the differences between "today," "yesterday," and "tomorrow" would not be among the earliest concepts of primitive man and be expressed in some way by the words that he used.' Second, that I assume that 'the only approach to the problem is the historical one' (178). I nowhere make such a statement; also it should have been evident to any competent reader that the apparent history of the forms is employed only to explain the reason for meaning already determined by context.

His statement, 'We are here going from omnitemporality to omnitemporality' (179), shows a lack of understanding of the situation. The omnitemporal character of the ancient imperfect represents a primitive condition, and the apparent omnitemporality of the later perfect and imperfect is the result of the historical development and of the fact that the forms belong to different periods.

The reviewer asks (179), 'What did the omnitemporal imperfect mean?' This should be clear to anyone with any knowledge of a language with such a verb form. This imperfect had the general verbal meaning (not necessarily the infinitive, but one expressed in English by the infinitive with its sign *to*, e.g. the verb *to go*). Tense ideas in such verbs are expressed by the context, e.g. by adverbial expressions of time denoting past, present, and future.

His dismissal of the evidence adduced as proof of the original present meaning of the perfect as 'tenuous' (179; he gives only a part of the points advanced) shows a failure to grasp the importance of this evidence. He does not attempt to explain what he means by 'tenuous', nor does he attempt the 'hopeless' task of conforming these uses to the aspect theory.

The method suggested (179-80) by the reviewer for a further, more extensive survey on the basis of the whole Biblical text (a suggestion which is not new), separated so far as possible into units of approximately the same period, is one that would naturally come to the mind of any linguistic student. Such a study, however, would, I believe, simply result in a more extensive collection of material similar to that compiled by Driver, would be accompanied by great if not insuperable difficulties in determining the period of the verb forms discussed, and would in all probability add nothing of special value to what we know already of the use of the various verb forms, whether we explain them in terms of aspect or of tense.

With regard to aspect, that feature of the verb which deals with the character of the continuance of time (beginning, continuing, completed, or iterative) and not with time point, those who adopt the aspect theory have to consider the following facts. (a) There is no case in any language, so far as I know, in which verb forms denote aspect alone; aspect is always associated with tense. This, while not absolutely conclusive, is a strong argument against any pure aspect theory. There is also no obvious empirical basis for the development of a pure aspectual system, as there is for a temporal system. (b) The question, 'What is a completed present or a completed future act?' must be answered. Such ideas, involving contradictions as they do, are to me inconceivable.

On the whole, aside from presenting a picture of somewhat fervid disagreement with the author, the reviewer can hardly be said to prove or disprove anything, and he certainly adds nothing of value to the discussion of the important problem of the Hebrew tenses.

R. A. CROSSLAND of King's College, University of Durham, Newcastle upon Tyne, asks that the following note be printed as a correction to W. P. Lehmann's review of *Studien zur indogermanischen Grundsprache* in *Lg.* 30.99-104 (1954).

My discussion of the laryngeal theory, referred to twice in Lehmann's review, was published not in the *Transactions of the Philological Association* but in *Transactions of the*

Philological Society 1951 (Oxford, 1951).¹ Further, the observation cited in *Lg.* 30.103 applied only to the 'Anatolian' languages other than Hittite. Since 1951, incidentally, B. Rosenkranz' *Beiträge zur Erforschung des Luvischen* and H. Otten's *Zur grammatikalischen und lexikalischen Bestimmung des Luvischen* have put us in a rather better position where Luwian is concerned.

CORRECTION: Joshua Whatmough's review of Vetter, *Handbuch der italischen Dialekte*, Vol. 1, begins as follows (*Lg.* 30.108-9): 'A pamphlet of additions and corrections to Buck's *Grammar of Oscan and Umbrian* (1904) was printed at Boston in 1928. But the book is all but unobtainable. A new edition is in preparation.' Carl D. Buck has written to the reviewer, telling him that a revision of this grammar is not contemplated.

¹ The blame for inadvertently replacing *TPS* by *TAPA* is the Editor's. He apologizes both to the author of the article cited and to the reviewer.

The first of these is the fact that the
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JUNCTURE AND SYLLABLE DIVISION IN LATIN

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There have been a number of successful attempts, such as Kent's,¹ to explain variations in scansion or in the development of Vulgar Latin forms in terms of variations in syllable division. It is also rather commonly accepted that Latin had some means of marking word boundaries—that is to say, had a juncture phoneme. There has not been (to my knowledge at least) any attempt to put syllable division and juncture together in a systematic fashion, to see what each can tell us about the other and what both can tell us about the phonemic structure of the language. Such a synthesis is the task here attempted. It is the contention of this paper that when the two are put together, three situation types emerge: normal syllable division, normal syllable division accompanied by juncture, and syllable division of a type that requires juncture.

In syllable division we have evidence from scansion, from certain inscriptions where syllables are divided by points, and from direct statements by the grammarians; these several kinds of evidence establish the following facts. (1) A syllable is long if it contains a diphthong or a long vowel, or ends in one or more consonants. (2) The sequence /VCV/² is normally divided /V-CV/, not /VC-V/, so that the prior syllable is short. (3) The sequence /VCCV/, at least with non-initial clusters, is normally divided between the consonants, so that the prior syllable is long. (4) If there are more than two consonants in medial position, there is likely to be a division somewhere within the cluster, but scansion evidence will not tell us where, since scansion does not distinguish between syllables which end in one and those which end in more than one consonant.

There is evidence that word boundaries could affect syllable division. Thus Hoenigswald, after a study of metrical practice, explains the 'rule of avoidance' ('In post-scenic verse, words ending in a short vowel are not permitted before words beginning with *s* impure') in these terms:³ 'To keep the scansion short was to be barbarous by the standards introduced with the new meters; to use long scansion was to violate Roman tradition and, probably, a Latin pronunciation in which word boundary was somehow marked.' But there is also evidence that variation in syllable division could occur without the intervention of a word boundary. A striking example of variation (even though possibly artificial) occurs in the *Aeneid* 2.663,⁴ where the first syllable of *patrem* is scanned long, indicating a division *pat-rem*, while that of *patris* is scanned short, indicating a

¹ Roland G. Kent, *The sounds of Latin*² §62 (Baltimore, 1932).

² Unfamiliar symbols used in this paper are /L/ for length and /+/ for juncture. A hyphen marks the point of syllable division; it appears in some otherwise phonemic transcriptions, but is not a phoneme.

³ Henry M. Hoenigswald, A note on Latin prosody: Initial *s* impure after short vowel, *Proceedings of the American Philological Association* 80.272, 276 (1949).

⁴ Kent, loc.cit.

division *pa-tris*. A typical and convincing modern statement is the following:⁵ 'as is well known, clusters of this type [stop plus liquid] do not make position (*impetro, re-trahit*) in Plautus and Terence ... except where there is a compounding seam after the stop (*ob-ruo*). In later, classical verse, however, positional length becomes optional in such words as *impetro* as well.' The standard handbooks, such as Sommer's *Handbuch der lateinischen Laut- und Formenlehre* §§162-3, give many examples of such variation.

These facts are enough to establish that a juncture phoneme existed, that it could determine syllable division, and that it could determine word boundaries but was not necessarily in one-to-one correspondence with them. A sequence marked by a juncture must have been different from normal transition; and one of the ways in which it could differ appears in syllable division, at least when a single consonant is assigned to the preceding syllable. This juncture, it must be emphasized, was both a physical event and a member of a phoneme, audible just as a member of any other phoneme was audible. On the other hand, while it is true that a syllable division of the type /V-CV/ would not be revealed by scansion as containing a juncture, the consistent spelling of word boundaries can be relied on to point out the junctures which occurred in this sequence.

The best evidence as to the phonetic nature of juncture in Latin comes from its history. No explanation of forms like Italian *labbra* and *acqua* is as simple as postulating the sequences /-b+r-/ and /-k+w-/ , where the juncture resulted in a lengthening of the preceding consonant. Similarly, as will be shown later, a possible result of the sequence /-u+V/, when juncture is lost, is /-uwV/; another result, of course, is loss of the /u/. I should interpret the form /-uwV/ as one in which /-u+/ also involved a lengthening of the preceding phoneme, which, if lengthened still more, gives rise to a new segmental phoneme. The type /uwV/ represents a slower type of articulation, in which juncture is so to speak increased; contraction of vowels with loss of juncture represents more rapid speech. I should define juncture, then, as a lengthening of the preceding phoneme by one half-unit, where a full unit is equal to the average length of a sound as member of a phoneme.⁶

There is only one set of contemporary statements bearing on juncture, unfortunately not very clear. This is the statement of Velius Longus and others that *hoc erat* was pronounced with two c's.⁷ The usual interpretation of this is that *hoc* contained a final geminate before vowels, but I doubt if the statement will support this. The grammarians mention only phrases containing *hoc* and forms of *esse* and, indeed, the best supporting spelling is the inscriptional *occest* for *hoc est*. Both the nature of the phrases mentioned and this spelling make me think that *hoc* could preserve its etymological geminate before vowel only if a juncture was lost; and I would further conclude that this is in line with

⁵ Hoenigswald 273.

⁶ Such a definition of juncture as a half-unit lengthening is in accord with measurements secured by Martin Joos for English, using the sound spectrograph. (Private communication).

⁷ Ferdinand Sommer, *Handbuch der lateinischen Laut- und Formenlehre* 276 (Heidelberg, 1914).

the general simplification of geminates in final position; that is, geminate followed by juncture was not a permitted sequence.

There is, however, another possible interpretation of Velius's statement. It is to be noted that *hoc erat* is generally treated differently in spelling from *hoccēdō* and *occidō*. One can therefore assume that Velius was describing juncture, rather than two genuine consonant phonemes in this particular phrase. If this is true, his statement is not without parallel. The excellent medieval English phonetician Orrm writes a syllable-final consonant after a short vowel (e.g. *upp*) as if it were two full phonemes, though English in his day still contained medial geminates. In short, if the phrase was actually /hok+erat/, with half-unit lengthening of /k/, I do not believe that Velius would have been likely to describe it otherwise than as he did.

We can now define several types of syllable division in relation to necessary and permitted juncture. (1) /VCV/, division before the consonant, juncture to be inferred only from a word boundary: *ego* /ego/, *ipse dixit* /ipse+diLksit/. (2) /VLCV/, division again before the consonant, juncture again to be inferred only from a word boundary: *dēcēdō* /deLkelɔL/, *amō tē* /amOL+teL/. (3) /VC+V/, division after the consonant, with juncture necessary to establish the type, its presence being revealed by a word boundary and by the development in daughter languages with different treatments of closed and open syllables:⁸ possibly *hoc adest* /hok+adest/. (4) /VLC+V/, division again after the consonant, with juncture again necessary to establish the type but not to be inferred from scansion: *nīl agere* /niLl+agere/.

In a sequence of two intervocalic consonants, the normal syllable division is between the consonants. This statement, made above, has yet to be examined; double consonants, as in *Atticus* or *dat tibi*, are a good place to begin. Whenever there was a word boundary, it fell between such consonants, never before or after them;⁹ and scansion shows that the syllable division was in the same place. As usual, I should assume that there was a juncture in *dat tibi*, though the only evidence for it is morphological. Double consonants are not the only ones in which the syllable division necessarily fell within the cluster. Latin consonants can be divided into three groups according to their sequence in initial clusters: group 1, spirants; group 2, stops; group 3, liquids and nasals. In final and medial clusters, order varied; in final position the order could be forward, as in *post* (1-2), or more commonly reversed, as in *arx* (3-2-1), and medially the order could be mixed, as in *admixtiō* (/kst/ 2-1-2). Whenever two consonants belong to the same group, as in *abdō* (2-2) or *amnis* (3-3), the syllable division again fell between them. There is also evidence on juncture in this situation. Latin did not develop, as some Indo-European languages did, a complex set of assimilations across word boundaries; but within words assimilations were frequent. Assimilations of consonants of the same group are perhaps not the most common, but certainly occur, as in **quid-pe* to *quippe*, or in Vulgar Latin *amid-*

⁸ Hoenigswald 276 fn. summarizes metrical practice as follows: /V CV/, /VCV/, /VC V/. He writes a space for word boundary, and /C/ means any stop. In all three of these situations the first syllable is short. Such scansion can be understood only if we assume that poetical convention suppressed the juncture in /VC V/.

⁹ See the discussion of *hoc erat* above.

dula for *amigdula*. Commoner are assimilations like *ad ferō* to *afferō*. I should interpret all such assimilations in the light of a loss of juncture; that is, the sequence /VC+CV/ operated to protect the first consonant by lengthening it, but with loss of juncture, regressive assimilation became possible. The inference from forms with and without assimilation is that juncture was possible between consonants, and was not the same thing as syllable division, which remained unaffected even after assimilation had taken place.

The syllabic types so far definable when there are two consonants are the following: (1) /VC-CV/, divided into one subtype with juncture and one without, these being distinguished by word boundaries and assimilatory developments: (a) /VC+CV/ *quid ferō* /kwid+ferōL/; (b) /VC-CV/ *adferō* > *afferō* /adferōL/ > /afferōL/; the presence of this type, but not the presence of juncture, is shown in scansion when the preceding syllable is treated as long, as in *estis*, scanned *es-tis*.¹⁰ (2) /V+CCV/ with division before the consonants and the preceding syllable therefore short, with or without word boundary: *potestās* /pote+stās/ as in the form recommended by grammarians, though this does not seem to have been reflected in common practice; *ipse spectat* /ipse+spektat/. (3) /VCC+V/, always with word boundary: *est id* /est+id/.

A group in which there was more variation in syllable division,¹¹ and which has therefore received more scholarly attention, is that composed of stop plus liquid. We can accept the fact that literary Latin commonly divided before the group, though departing from this practice on occasion; but the variation is greater than the evidence from the literary language shows us. Thus, French *couleuvre* from *colubram* reflects division between the consonants, since the position of the stress shows a long syllable, yet the development of the vowel in French is that of an open syllable. The explanation is, I believe, that a new division was introduced by the rise of a juncture before the cluster. A different type is shown in Italian *labbra* from *labra*, where the consonant strengthening must go back to */lab+ra/; Italian *pietra*, with open vowel, points to */pe+tra/. The whole set of variations, in Classical Latin, in Vulgar Latin, and in Romance, can be fitted into the pattern so far established. Without juncture, syllable division was between the consonants; but a juncture could occur before or after the first consonant. Variation in treatment reflects variation in the position of the juncture, not departure from the normal type of syllable division.

A further group needing special treatment, particularly since it bears on a phonemic problem discussed below, is that consisting of consonant plus semi-vowel. Prominent among these groups is /kw/, not only because of its special spelling, but because it almost never 'makes position', the syllable division being *a-qua*, *e-quus*. A phonetic reason for such a division can be found in the sugges-

¹⁰ Sommer 282.

¹¹ There is no proof that normal syllable division fell between consonants unless juncture upset the situation, which is the assumption made here. The orthodox interpretation is one of different divisions in the various clusters. If, however, the situation is such as to permit of a general rule, with variations referred to juncture, such a view seems both more consistent and in the long run simpler. It should be emphasized, therefore, that common syllable division and normal syllable division are not the same things.

tion by Sturtevant, *Lg.* 15.221-3 (1939), that velar closure and lip-rounding occurred simultaneously, so that the group was something less than two full consonants in time. There is a possible phonemic reason which is even more cogent. I believe that it can be shown that in early Latin [w] and [y] were allophones of /u/ and /i/. In accord with this belief, the usual division before *qu* is just what we should expect of a single consonant between vowels. Again, of course, there could be a juncture after the /k/, which would upset this syllable division; such a form in Latin was *lācue* /laLk+we/, and Italian *acqua* is from a late */ak+wa/. On the division of /Cy/ there is very little evidence from Latin itself, but at least there is a form *o-riundi* in Lucretius 2.991, in place of the expected *or-iundi*. In Spanish and Portuguese, however, the group /ry/ normally goes with the following syllable, and requires a juncture to divide after the /r/.¹² I conclude, somewhat tentatively, that normal syllable division was before any two-consonant cluster with /w/ or /y/ as the second member; all contrary instances had juncture after the first consonant. The sequence /C+S/ (where S is a semivowel) was rather common: Italian *mezzo*, *prezzo*, etc. must go back to */med+yus/, */pret+yum/ from Classical *medius*, *pretium*.¹³

The sequence /VLCCV/ is a further exception to the rule that normal syllable division is between consonants, but the reasons for the exception are different. We have seen that in the sequence /V+CCV/, the prolongation of /V/ by a half unit throws both consonants into the second syllable. Prolongation by a full unit acts in the same way. (The sequence /VL+CCV/ is possible, but would not affect the syllable division.) Division between or after /CC/ was accomplished only by a juncture. It can be said, then, that /L/ acts as a syllable closer unless it is followed by one or more consonants AND by juncture. The syllable types involving /L/ are then: (1a) /VL-CCV/, without juncture: *flābra* /flaL-bra/; (1b) /VL+CCV/, inferrible only from word boundary: *dē plēbe* /deL+pleLbe/; (2) /VLC+CV/, inferrible only from word boundary: *nīl dīcō* /niL+diLkōL/; (3) /VLCC+V/, inferrible only from word boundary: *plēbs est* /pleLps+est/.

Metrical evidence fails when there are three or more consonants, but word boundaries and clustering habits give us some information. Three consonants occurred initially and finally, as in *strabō* 'squinter' and *arx* 'fortress', and such normal 1-2-3 or 3-2-1 groups could occur without a juncture internally. In a sequence like that in *affluēns* there was syllable division after the first consonant, but the assimilation shows that there was no juncture. The group in *abstō* (if the spelling is to be believed) had the same syllable division, but (since no assimilation has occurred) a juncture after the /b/. A 3-2-2 cluster, as in *adiunctus* /-nkt-/, would also have normal syllable division between the /k/ and /t/, with no need for juncture.

Much the same conditions apply to the rarer four-consonant clusters. These occur across word boundaries, as in *post plangōrem* 'after the noise' (where a juncture is to be inferred), but also within a word, as in *adscrībō* 'write in addi-

¹² Sommer 284 denies this kind of division in Romance. My statement I owe to a private communication from Robert P. Stockwell.

¹³ Sommer 133.

tion' where there was syllable division after the first consonant, and (since there has been no assimilation) juncture as well. In forms like *ascribō*, on the other hand, from */asskriLBOL/, one can assume syllable division after the first consonant, but without juncture, since the /d/ must have been assimilated before disappearing.

Juncture in sequences of vowels has received more attention than in sequences of consonants; Hoenigswald, *Antevocalic u-diphthongs in Latin*, *Lg.* 25.392-4 (1949), gives an excellent list of the distinctive sequences. But Hoenigswald, though he recognizes the existence of juncture ('Word boundary is a phoneme', 392 fn. 1), makes no use of it as a means of distinguishing the various sequences. Instead, he makes the necessary distinctions by setting up three series of two phonemes each: pure vowels /i u/, semivowels /i̯ u̯/, and consonants /y w/. If the juncture which Hoenigswald recognizes is used, these series can be reduced. As I mentioned earlier, I believe that there are two stages of Latin phonemics, one in which there were only the two phonemes /i/ and /u/, each with consonantal allophones, and one in which there were four phonemes—/i u/ and /y w/. I shall begin with the former stage.

One type of sequence is represented by *aes* and *haud*. Despite the somewhat unexpected spelling of the former, it is generally assumed that the vocalic nuclei were phonetically [ay] and [aw]. The [y] and [w] can be set up as allophones of /i/ and /u/; the rule for their occurrence is that a vowel extreme (/i/ or /u/) after a non-extreme vowel assumes its consonantal shape.

Another type includes *iecur* 'liver', *uoluit* 'it turns', *uulpes* 'fox', and *adiiciō* 'I add', in which the first vowel of the sequence denotes phonetic [y] or [w]. The rule here is that a vowel extreme before any vowel assumes its consonantal shape. A necessary further rule, to be discussed later, is that when two vowel extremes occur together with no third vowel following, the first assumes its consonantal shape.

Sequences of more than two vowels always contain one or more vowel extremes. In a three-vowel sequence, if the second is an extreme it assumes its consonantal shape, with syllable division before it: *Auernus* /a-uernus/; the rule applies even if the first and third are also extremes: *biuugis* 'yoked two together' /bi-iugis/. If the second is a non-extreme vowel, first and third assume their consonantal shape without syllable division: *uae* /uai/. In a four-vowel sequence, the treatment depends primarily on the first vowel. If it is non-extreme, consonantal allophones appear for the second and third, with division between them: *aeuī* /ai-uī/ and (in spite of the spelling) *maius* /mai-ius/. If the first is an extreme, consonantal allophones appear for the first and third, with division before the third: *iuuō* /iu-uō/.

One other type of vowel sequence involves no necessary juncture: a long vowel followed by another vowel. This type is always dissyllabic, the /L/ closing the first syllable as we should expect: *āeris* /aL-eris/ 'of the air', and *dēerrō* /dēL-erō/ 'I lose my way'.

Other apparent vowel sequences contain a juncture, and contrast with those which do not. A good minimal contrast is that between *aes* 'brass', and *ais* 'thou sayest'. Phonetically the two are [áys] and [á'ís], the first being one syllable.

ble and containing only one true vowel, the second being two syllables separated by a half-unit of lengthening and containing two vowels. Phonemically, the two are /ais/ and /a+is/. Other examples of sequences with juncture are *deinde* /de+inde/ 'thereupon', *coit* /ko+it/ 'it comes together', and *ruit* /ru+it/ 'it falls'; the rule is that two true vowels are always separated by a consonant, by length, or by a juncture. A second type of contrast is revealed by syllable division. Hoenigswald contrasts *biugis* /bi-iugis/ with the proper name *Gnaeus* /ɣnai-us/; the long first syllable in the second form indicates the presence of juncture: /ɣnai+us/. The juncture is necessary to accomplish this type of division; but if there are two consonantal allophones, juncture does not affect the syllable division, cf. *maius* /mai-ius/. The Italian form with consonant strengthening (*maggiore*) goes back to a variant */mai+ius/, with permissive juncture.

The rules here given would establish complementary distribution for all occurrences of [i u] and [y w], except for one difficulty: the pair *cui* and *huic*. The rule given above, that of two vowel extremes only the first assumes its consonantal shape, is valid only as long as there are no contrasts of the type [yi]-[iy], [wu]-[uw] or [yu]-[iw], [wi]-[uy]; if it is possible for either of two vowel extremes to be consonantal, then obviously consonantal shape is not predictable. When the contrast [wi]-[uy] came into existence in Imperial Latin, /i/ and /y/, /u/ and /w/ at once became separate phonemes; the matter has been fully discussed by Sturtevant.¹⁴ The new diphthong appeared in *cui*, *cuius*, and *huic*; thereafter, scansion shows it as a variant in a number of other forms which ordinarily had /u+i/. Of the three crucial forms, it might be possible to explain away *cui* and *huic*, where we know that two-syllable variants occurred, and that archaic forms with /oy/ persisted; but no such explanation will apply to *cuius*, where we know beyond question that the pronunciation was [kuy-yus], with one [y] in the first syllable. Further it is unlikely that the diphthong here was /oy/, since there would have been no reason for not so spelling it. In Republican times, all three forms had had /oy/, which is in accord with the rules of distribution given above; but in all three the old sequence was replaced by [uy]. We must then assume either that /y/ and /w/ had always been separate phonemes, or that a change in phonemic structure occurred between the Republic and the Empire. Several considerations make the latter view the more likely. First, until the development of *cui*, *cuius*, and *huic* there was no instance of contrast which would have made separate phonemes necessary. Again, Latin never developed any systematic way of spelling the new phonemes. And finally, the considerable hesitation in the Romance languages over the treatment of [y] and [w] is at least partially explained if we assume that the parent language showed similar hesitation, likely to have worked itself out in dialect variation. For these reasons I consider it necessary to posit a phonemic change, one which is the more interesting in that it was accomplished without borrowing and without the creation of any new phonetic entities, merely by the appearance of a new sequence. The new situation has /y/ and /w/ in all sequences which pre

¹⁴ The pronunciation of *cui* and *huic*, *Transactions of the American Philological Association* 43.57-66, (1912).

viously had consonantal allophones of /i/ and /u/: *aes* /ays/, *haud* /(h)awd/, *iecur* /yekur/, *uoluit* /wolwit/, *uulpes* /wulpes/, *adiiciō* /adyiki+ol/, *biugis* /biyugis/, *aeuī* /aywīl/, *maius* /mayyus/, *Gnaeus* /ɲay+us/. Forms with juncture, such as *ais* /a+is/, remain unchanged or lose their juncture, so that /ais/ is now in contrast with /ays/. (This is roughly the state of affairs in Spanish, where *poeta* occurs as /po+eta/ or /poeta/.)

Some questions remain. What happened in the early period when a juncture was lost between vowels? One result was loss of one of the vowels, as shown by the practice of elision in verse. But in other situations the result was different. Sommer notes (164) that when a vowel followed /i/ or /u/ in 'continuous articulation', a semivowel [y] or [w] was generated as a transition sound; as proof of this development, which does not appear in the writing, he cites spellings in which etymological [y] or [w] is dropped in a similar situation, as *FLVIO* for *fluuiō*. Forms with these transition sounds reflect the loss of a juncture; a spelling like *patrius* is ambiguous, standing for either /patri+us/ or /patriyus/. Sommer further suggests (165) that [w] could be substituted for juncture after /o/ as well as /u/, as in *GLOVE* for *Chloē*.

Another unsettled question concerns sequences of like vowels with juncture and with length. For the vowel /u/, the following contrasting sequences are possible: /ul/ *pūpa*, /u+u/ *cornuum*, /uu/ > /wu/ *uulpes*, /uw/ *fluuiō*, /ulw/ *ūua*. These contrasts establish at least that /ul/ cannot have been phonetic [uw]; for unless syllable division is to be set up as a phoneme, /ul/ contrasts with [uw]. Between consonants /ul/ may well have been simply [uu], but a similar solution seems unlikely for such forms as *dēest* 'is absent'. If all such forms had juncture, what allophone of juncture was it? And if they did not require juncture, was the sequence simply [eee]? If so, we have a third degree of length, which seems unlikely, and is without parallel in related languages. It seems to me plausible to suppose that /L/ between like vowels was a glide of full phoneme length, but not high enough to reach /y/ or /w/, or (after /a/) to reach /e/ or /o/; thus, /eLe/ may have been [eēe] or [eje].

Throughout this paper I have assumed that juncture and syllable division in Latin were different though related phenomena, and have relied on the customary writing of word boundaries to establish the existence of juncture. In phrases like *dat tibi* and *amō tē*, both the number and the position of the stresses are different from what they would be in similar sequences without intervening word boundaries; yet the syllable divisions are normal, and would be the same even if the word boundaries disappeared. Are we to believe, then, that because there is no evidence for the PRESENCE of juncture in such phrases, there is evidence for the ABSENCE of juncture?

We might assume that juncture was lost in sandhi after a single consonant when the next phoneme was also a consonant, and after long vowels generally; and we could interpret the writing, with its consistent use of spaces, as a typical morphophonemic spelling. Stress, in that case, would have to be viewed as far freer than it is usually said to be. But since stress remains predictable, or nearly so, in terms of word boundary, we would be forced to a further conclusion: that the juncture pattern, at an earlier time so closely in accord with word bound-

aries that stress could be fixed in terms of it, was later changed so as to lose its relation to stress.

An alternative is to assume that there were junctures which did not affect syllable division, and so are undiscoverable by this kind of evidence. The stress pattern of the cited phrases would then be interpreted not merely in terms of word boundaries, which were presumably morphological rather than phonemic entities, but in terms of junctures. This alternative is simpler, since it does not require the postulation of a major change in juncture habits. For that reason it has been adopted in this paper.

If the description of juncture here outlined can be accepted as helping to account for the distinctions established by Latin spelling, prosody, grammatical statements, and historical development, then to the fourteen consonants, five vowels, and two semivowels, as well as the several pitch and stress phonemes, must be added two phonemes of time, which both involve a lengthening of the preceding phoneme. These are /L/ and /+/, entities which not only have phonetic characteristics in common, but also pattern alike. Both act as syllable closers, both go with the preceding syllable. It has been said that /L/ is a consonant,¹⁵ but this seems unacceptable. First, /L/ occurs only after vowels, never after consonants; long consonants in Latin are better analyzed as geminates, since they normally belong to different syllables. Second, /L/ never goes with a following syllable, as genuine consonants always do in normal transition. Third, /L/ cannot be doubled like the genuine consonants. For these reasons, the proper place of /L/ is with /+/, together, these two constitute a separate class of Latin phonemes.

¹⁵ Robert A. Hall, Jr., Classical Latin noun inflection, *Classical philology* 41.84 (1946).

NOTES ON GOTHIC FORMS

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1. THE VERBAL REDUPLICATING PREFIX. If the verbal stem in Gothic contained an initial cluster of two consonants, only the initial consonant was transferred to the prefix, except for the indivisible clusters *sk* and *st* (no examples of *sp* are recorded). The few cases of these two types of transference preserved in Wulfila's Gothic are: type 1 *fraisan* 'to attempt' : *fatfrais*, *gretan* 'to weep' : *gatgrot*, *flokan* 'to lament' : *fatflok*, *slepan* 'to sleep' : *satslep*; type 2 *skaidan* 'to divide' : *skatskaiþ*, *ga-staldan* 'to acquire' : *ga-statstald*. The digraph *hw* must have also represented two indivisible phonemes *hw* (and will here be so written for convenience), since *hw*, not *h* alone, was transferred to the prefix: *hwopan* 'to boast' : *hwathwop*. The indivisibility of the clusters *sk* and *st* may have been due to the fact that the IE stops **k* and **t* after *s* were not shifted, and hence could not be divorced from the preceding *s*. But this was not the case with the initial clusters *fr*, *gr*, *fl*, *sl*. To explain the loss of the second phonemes in the prefix we are justified in assuming that in pre-Gothic (i.e. in Proto-Germanic) these clusters were transferred to the prefix, but that the second phonemes *r*, *l* were later lost through dissimilation: **fraitfrais* > *fatfrais*, etc. This loss brought the form of the prefix into the pattern of the more frequent type of reduplication with a single initial consonant; thus, *fatfrais* like *fatfalþ* (from *falpan* 'to fold'), *satslep* like *saiso* (from *saian* 'to sow'), etc. One may object to this explanation on the ground that analogy alone could have produced the same result, since the great majority of reduplicating verbs in Gothic had a single initial consonant. Perhaps the safest assumption is that the leveling was brought about by a combination of phonetic change and analogy.

2. THE DECLENSION OF *reiks* 'KING, RULER'. The substantive *reiks* (masc.) represents a survival of the IE consonantal declension (cf. Lat. *rēx*) with the intrusion of the *a*-declension. The consonantal case forms are preserved in *reik* (dat. sg.) and *reik-s* (nom.-acc. pl.); the intrusion of the *a*-declension is represented by the forms *reik-is* (gen. sg.), *reik-e* (gen. pl.), and *reik-am* (dat. pl.). The forms *reik-s* (nom. sg.) and *reik* (acc. sg.) could belong to either declension. My purpose here is to point out in the inflection of *reiks* the possible factors which contributed to the preservation of the original case forms of the masculine consonantal stems, which elsewhere in Gothic were regularly supplanted by those of the *u*-inflection. Thus *fot-us* (IE **pōd-s*, cf. Gk. *poús* : *pod-ós*) and *tunþ-us* (IE **dnt-*, cf. Lat. *dēns* : *dentis*) show a complete shift from the IE consonantal inflection to the *u*-inflection.

The word *reiks* was borrowed from Celtic *rīx* (*rīg-s*) 'king' during the prehistoric era when the shift of IE **g* (Celtic *g*, cf. OIr. *rīg*, gen. sg.) > Gmc. *k* was not yet completed. The word was incorporated into the Gothic vocabulary at such an early period that no trace remained of its foreign origin except the Celtic vowel *i*, Gothic *ei*. To explain the inflectional discrepancy between *reiks* and the normal type of *fot-us* we may assume that *reiks* fitted into some al-

ready established declensional pattern—one which could not affect the type that yielded to the analogy of the *u*-stems. One inflection may affect another through semantic factors common to both; cf. *nahts* (fem. cons. stem) 'night' : *dags* 'day' (masc. *a*-stem), but together *dag-am jah naht-am*, the ending in *dag-am* displacing the earlier dative plural ending of *nahts* (probably *-im*, as in the fem. cons. stem *baürg-im* 'cities'). The semantic association between *nahts* and *dags* is obvious, as is that between *fotus* and *tunþus*. But note that *fot-us* and *tunþ-us* denote inanimate objects, whereas *reiks* denotes a person. This semantic difference may be a point of departure for explaining the inflectional cleavage between the two types: *reiks* did not yield to the analogy of the *u*-stems because it entered into the pattern of the so-called *nd*-stems, the substantivized present participles. The *nd*-stems all denote personal agents, and are always inflected as consonantal stems, with the intrusion of the endings of the *a*-declension in the same case forms as in *reiks*. (The single exception is *daupjands* 'baptist', which has preserved the weak endings of the participle in the gen. and dat. sg.)

The immediate point of contact between *reiks* and the *nd*-stems was undoubtedly furnished by those of the latter group with meanings similar to 'lord, ruler, master'. Examples are *garda-waldands* = *oiko-despótēs* 'master of the household' (*waldan* 'to wield power'), *all-waldands* = *panto-krátōr* 'almighty ruler', and *frauþinond* (voc.) = *déspota* 'lord, master' (*frauþinon* 'to rule over'), gloss for *frauþa* 'id.' in Luke 2.29. Furthermore, the declensional pattern of the *nd*-stems belonged to an established category for personal agents, whereas *reiks* was an isolated substantive of the consonantal declension. For this reason it was fitted into the pattern of the *nd*-stems; otherwise it would have yielded to the analogy of the *u*-stems.

3. THE INFINITIVE *ga-stopanan*. This verb occurs only once (Rom. 14.4) and only in the infinitive form *ga-stopanan* = *stēsai* 'to make stand, aufrecht erhalten'. Two problems are presented by this form: the anomalous suffix *-anan*, and the irregular stem form *stop-*, with the high-grade vowel *ō* of the corresponding intransitive verb (*standan*), but without the *j*-suffix to denote the causative.

(a) The first syllable *-an-* of the suffix *-an-an* in *ga-stop-an-an* occurs likewise in the past participle form *ga-ain-an-aidai* (Thess. 2.17, B) = *aporphanisthéntes* 'separated'; but it has no historical support, since it does not occur elsewhere in Gothic and nowhere in the other Gmc. languages. Most editors of the Gothic Bible have discarded it as an orthographical error. Jellinek suggests¹ that the syllable *-an-* may be the relic of an original verb suffix *-anan*; but for this, as far as I can discover, there is no evidence. I shall therefore assume the emended forms *ga-stopān* and *ga-ainaidai* to be correct.

(b) The form *ga-stopān* is anomalous insofar as most causative verbs derived from strong intransitives are formed as weak *jan*-verbs with the high-grade vowel (cf. *reisan rais* 'to rise' : *rais-jan* 'to raise'). We should expect a form **ga-stop-jan* or **ga-stod-jan* (with *d* from *þ* by Verner's law); cf. ON *stþō-ja* 'to make stand, zum Stehen bringen'. But a form *-stodjan* already existed in

¹ *Geschichte der gotischen Sprache* §233 footnote: 'Liegt hier der Rest einer Ableitungsart auf *-anan* vor?'

the sense of 'begin', and so could not be used in the causative sense of 'make stand'. A new form was created for the causative by resorting to a secondary formation from the adjective **ga-stops*, preserved in the form *un-gastopai* 'not having a permanent abode' = *astatoûmen* (Cor. 4.11). The verb *ga-stopan* then entered into the category of the weak *ē*-verbs derived from adjectives, which could acquire a causative force; cf. *fastan* 'to fasten, make fast' (adj. Goth. **fasts*, ON *fastr*, OS *fast*, etc.), *weiðan* 'to consecrate, make holy' (adj. *weihs*), *ga-ainan* 'to make each one separate' (adj. *ains*, cf. Germ. *vereinzeln*). In the past participle *ga-ain-an-aidai* (see above) the syllable *-an-* was most probably due to a scribal error, the *-an-* of the present participle *ga-ain-an-ds** being carried over into the past participle.²

4. THE SUFFIX IN *mannisk-odus** 'HUMANITY'. This word occurs only once, and in the Skeireins (6.12): *insaht manniskodaus*, which evidently translates the Greek *tò tēs anthrōpôtēs skhēma* (Ammianus 1432). The abstract *mannisk-odus* is anomalous insofar as the suffix is here attached to an adjectival stem (*mannisk-s* 'human'), whereas it is elsewhere regularly attached (also as *-opus*, without dissimilation) to verbal *ōn*-stems; cf. *auhj-odus* 'noise' (*auhj-on*), *gaun-odus* 'grief' (*gaun-on*), *wrat-odus* 'journey' (*wrat-on*). Aside from *mannisk-odus* only one other abstract is recorded with this suffix, viz. *gabaúrj-odus* 'pleasure'; but since neither the corresponding verb nor the corresponding adjective is recorded, the category of this word is doubtful. The purpose of the following discussion is to determine why the author of the Skeireins used the suffix *-odus* in *mannisk-odus* when he had already at his disposal the regular adjectival suffixes **-īn-* and **-dūþ-i-* (cf. *undiwan-ei* 'immortality', *ajuk-dūþs* 'eternity').

Jellinek³ has pointed out that the pattern for the formation *mannisk-odus* could not have been furnished by the Greek abstract *anthrōpôtēs*, which it translates: the Greek form is not based upon the adjective *anthrōp-in-os* 'human' with the suffix *-in-* parallel to the *-isk-* of Gothic *mann-isk-s*; *mannisk-odus* would be a literal translation of a Greek **anthrōp-in-ôtēs*, which does not exist. He therefore suggests that the Latin abstract *hūmānitas* (adj. *hūmānus*) may have served as the pattern. But against this hypothesis is the fact that the Latin suffix *-itā-* corresponds to the Gmc. abstract suffix **-iþō-* (cf. Goth. *weit-wod-iþa* 'witness', *garaiht-iþa* 'righteousness'). If *hūmānitas* had been the model, we should expect **mannisk-iþa* (or *-ida*), especially since *-iþa* was much more frequent than *-odus*, of which only five examples are recorded. Of these, three are derived from *ōn*-verbs (see above); it has generally been assumed that the verbal pattern (with the vowel *ō* abstracted from the verbal stem) was transferred to adjectival abstracts in the two remaining examples, *mannisk-odus* and *gabaúrj-odus* 'pleasure'. Objection to this hypothesis has been raised by Fritz Mezger⁴ in the case of *gabaúrj-odus*. For this one must posit a verb **ga-baúrj-on*

² Cf. the confusion between the present and past participle of strong verbs, due to the congruence of the suffix *-ans* (< *-ands*) of the present participle with *-ans* of the past participle; cf. Eph. 2.16 B *af-slahans* 'having slain' (*apoktetnas*) = A *af-slahands* 'slaying'.

³ Jellinek 198, §253: 'Auffällig ist *manniskodus** als Adjektivabstraktum (§215); ausserdem wäre der Ausdruck wörtlich übersetzt nicht *anthrōpôtēs*, sondern ein **anthrōpinôtēs*, was nicht existiert. Sollte *humanitas* als Vorbild gedient haben?'

⁴ Cf. *Lg.* 21.97-8 (1945) and the literature there cited.

'sich freuen' or an adjectival stem inferred from the existing adverbial form *gabaúrj-aba* 'with pleasure, gladly'. Since, as Mezger points out, verbs cognate with **gabaúrjon* in the other Gmc. languages have senses incompatible with that of **gabaúrjon*, it seems more likely that the suffix in *gabaúrj-opus* was based directly upon the adverbial stem. This assumption would justify the inference that in *mannisk-odus* the suffix was analogically extended from the verbal to the adjectival pattern, except that the question remains: why should the author of the Skeireins have resorted here to *-odus*, a suffix of verbal origin when adjectival suffixes were already at his disposal?

The apparent irregularity can be explained if we assume that the suffix in *mannisk-odus* represents not an extension of the Gothic verbal type, but a direct inheritance from the IE suffix *-*ātu-*, which had already been transferred from the verbal to the adjectival type; cf. Lat. *ōrn-ā-re* 'to adorn': *ōrn-ā-tus* 'adornment'. The Latin and the Gothic conditions are parallel, both inherited from IE,⁵ except that the suffix *-*ātu-* in Latin *sen-ātus* had acquired a collective force ('a group of old men'), whereas the suffix in *mannisk-odus* had acquired an abstract force from the verbal type. If the suffix in *mannisk-odus* represents a direct inheritance from the IE adjectival type *-*ātu-*, there is no reason why the author of the Skeireins should here have used any other suffix: he simply retained the original.

5. THE LOSS OF *-j-* IN *-ij-* BEFORE A VOWEL. This loss occurs in the verb *fijan* 'to hate' and the derivative *fiapwos* (pl.) 'hostility'; in the verb *frijon* 'to love' and the derivative *frijapwa* 'love'; and in *sijum*, *sijais*, etc., forms of the verb *wisan* 'to be'. So far as I know, the only attempt to explain the loss was made by Sievers in his Schallanalyse,⁶ which is based upon theories of intonation that defy objective analysis.

The development of the cluster *-ij-* from the phoneme [i:] before a vowel can be satisfactorily explained if we assume with Herbert Penzl⁷ that it represents a cluster of two short vowels [ii]. Before another vowel, the second *i* of this cluster opened the syllable and was thus initial; accordingly it was shifted to the half-consonant *j*: **fi-an* = **fian* > **fi-ian* > *fi-jan* (cf. *kuni* : *kun-jis* 'race'). After a short stem syllable, an initial *j* before another vowel could not have been lost through any phonetic process; its loss must have been due to analogy. Note that the loss occurs only in verbal forms and their nominal derivatives.

The verb *fijan* belongs to the *ē*-class of weak verbs (cf. 3rd sg. *fiaip*, John 12.25); but the secondary *j* after the *i* of the stem syllable represents the same phoneme as the primary *j*-suffix in *jan*-verbs (cf. *sto-jan* 'to judge'), not only in the infinitive but also in the present participle *fijands* (*sto-jands*) and in most forms⁸ of the present tense, such as 1st sg. *fija* (*sto-ja*). Since the *ē*-verbs never

⁵ For the IE status in Sanskrit see Kluge, *Nominale Stammbildungslehre der altgermanischen Dialekte* §134: '*manniskōdus* "menschlichkeit" ist adjektivabstraktum (gebildet wie skr. *jivātu* f. "leben" und avest. *gyātu* m. "leben")'.

⁶ See Streitberg, *Gotisches Elementarbuch*⁵⁻⁶ §30.

⁷ Orthography and phonemes in Wulfilā's Gothic, *JEGP* 49.225 (1950).

⁸ Not all forms are recorded, but they can be reconstructed on the pattern of *ē*- and *jan*-verbs.

had a *j*-suffix, the partial convergence between the pattern of the *ē*-verb *fijan* and that of the *jan*-verbs was removed by omitting the *-j* in *fijan*.

The form *frijon*, on the other hand, did not differ from the regular pattern of the *jon*-verbs (cf. *hausjon* 'to hear'), except for the combination *-ij-*. Since *frijon* resembled *fijan* in possessing this combination, the loss of *-j* in *frijon* was undoubtedly due to the loss in *fijan*. The form *frion* then brought the verb into the pattern of *ōn*-verbs without *j*-suffix; cf. *salb-on* 'to anoint'.

The two verbs *fijan* and *frijon* thus established a pattern for the loss of *-j* in the cluster *-ij-* before a vowel, which was later extended to *sijum* > *sium*, *sijais* > *siais*, etc.

6. *Hatan* : *hatjan* 'TO HATE'. The loss of *-j* in the type *fijan* suggests a possible explanation for the presence of a *j*-suffix in *hatjan* beside *hatan*. Kieckers⁹ assumes that this suffix is of IE origin. In view of the established pattern of *fijan* > *fian*, I believe that a secondary origin, specifically Gothic, is far more probable.

The verb *hatan* occurs only in the present participle (*pize hatandane*, Luke 1.71; *pans hatandans*, Luke 6.27), and therefore furnishes no clue to whether it was a strong verb of the 6th ablaut series or a weak *ē*-verb; on the evidence of OHG *hazzēn* 'to hate', we may assume that it belonged to the *ē*-class, as did *fijan* 'to hate'. The form with *j*-suffix likewise occurs only in the present participle (*paim hatjandam*, Matt. 5.44, and Luke 6.27 as a marginal gloss for *fijandam*). In all these passages the present participle *hatjands** translates the present participle of the Greek verb *miseîn* 'to hate', whereas the nominal *nd*-stem *fijands* translates the Greek substantive *ekhthrós* 'enemy'. Cf. *agapâte tous ekhthrous humôn, kalôs poiête tois misoûsin humâs* = Matt. 5.44 *frijop fijands izwarans ... waila taujaiþ paim hatjandam izwis*; Luke 6.27 *frijod pans hatandans izwis; waila taujaid paim fijandam izwis*, with a marginal gloss *hatjandam* because the substantive *fijandam* cannot be used with verbal government. Similarly, Luke 1.71 *ex ekhthrôn hēmôn* = *us fijandam unsaraim*; *pántōn tōn misoúntōn hēmâs* = *allaize pize hatandane unsis*.

Since *fijan* and *hatan* are synonyms, both translating Greek *miseîn*, the semantic association could have led to a formal association as regards the phoneme *-j-*; i.e. the pair *fijan* : *fian* could have led to the reversed pair *hatan* : *hatjan*. As *fian* and *hatan* were on a level with each other, the same relation was preserved in *fijan* and *hatjan*: the first pair without *-j-*, the second pair with *-j-*. This secondary *-j-* in Gothic *hatjan* must be distinguished from the original Proto-Germanic *-j-* in **hatjan*, which is reflected in the causative verbs OE *hettan*, OS *hettian**,¹⁰ OHG *hezzen* > NHG *hetzen*. The causative verb is not recorded in Gothic.

⁹ *Handbuch der vergleichenden gotischen Grammatik* 244: 'Got. *hatjan* kann auf **qəd-ið* zurückgehen ... Dass es erst aus *hatan** umgebildet ist, kann man nicht beweisen.'

¹⁰ OS *hettian** may be postulated from the recorded *nd*-substantive *hettiand*.

MISCELLANEA

εἵνεκα AND ἔνεκα IN HOMER

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My treatment of the problems connected with the use of these words in Homer is based on Bechtel 114-6, and on Wackernagel (a) 134-8.¹

It is at present generally agreed that a phrase ἐν φέκα lies at the start. The existence of φέκα is attested by Hesychius' gloss οὐφεκα· οὐκ ἀρεστῶς, which, as Schulze saw (394 n. 3), must be corrected to οὐ φέκα. Φέκα is an adverb agreeing in ending with σάφα etc. (cf. Risch 307), etymologically equivalent to Skt. *vasi-*, and akin to ἐκῶν.

Bechtel explains the structure of the phrase, and draws attention to the fact that it must have been built before ἐν + acc. was supplanted by ἐνς (> εἰς, ἐς) + acc. Of the meaning Wackernagel says (b 2.157): 'Was das ἐν darin will, ist bis auf den heutigen Tag nicht erklärt.' In other words ἐν φέκα and φέκα are for us indistinguishable synonyms. We may expect to find them in competition with each other. We may also expect to find alongside of φέκα an alternant *ἔφεκα, because of other examples of such prosthesis; cf. Chantraine 1.181-3.

The phrase ἐν φέκα will become in Aeolic ἔνεκα, in Ionic εἵνεκα, in Attic ἔνεκα (whose breathing was transferred to the Ionic forms in the course of the tradition). As the first two forms are metrically equivalent, εἵνεκα in our poems supplanted ἔνεκα and came into competition with ἔνεκα. The problem is to account for the presence of both forms.

Some may believe that the problem can be lightly dismissed. Given ἔνεκα, metrical lengthening will account for εἵνεκα. But εἵνεκ' stops that explanation in its tracks, for it is a type never produced by metrical lengthening. Besides, εἵνεκα was spoken extensively, if not exclusively, by Ionians. It would be a counsel of desperation to call ἔνεκα an Atticism of the poets. As Wackernagel indicates, there is so much of it that this explanation becomes implausible. Solmsen presented a theory that while -υφ- caused compensatory lengthening of a preceding vowel, -ν φ- did not. But, says Wackernagel (a), 'diese Theorie ist von Danielson *IF* 25.264 ff. definitiv widerlegt.'

Nothing seemed left but to revert to a suggestion of Schulze (115, 494 ff.): ἔνεκα is a late form substituted for one beginning with a short vowel, which is now lost entirely. Wackernagel believes that at present we cannot go beyond this possibility, 'zumal für ἔνεκα selbst noch immer keine wirklich überzeugende

¹ The following works are cited by the author's name: Fr. Bechtel, *Lexilogus zu Homer* (Halle, 1914); Paul Cauer, *Grundfragen der Homerkritik*² (Leipzig, 1924); P. Chantraine, *Grammaire homérique* (Paris, 1942, 1953); E. Risch, *Wortbildung der homerischen Sprache* (Berlin-Leipzig, 1937); W. Schulze, *Quaestiones epicae* (Gütersloh, 1892); J. Wackernagel (a), *Sprachliche Untersuchungen zu Homer* (Göttingen, 1916); J. Wackernagel (b), *Vorlesungen über Syntax* (Basel, 1926, 1928).

² At the time I argued to the same effect; see *AJP* 33.401 ff. (1912). In spite of Bechtel's acceptance of Solmsen's theory, my opinion has remained unchanged; cf. *Lg.* 27.74 fn. 10 (1951). Compare Cauer 151 n. 13, and for other references see Schwyzler, *Gr. Gr.* 1.229.

Erklärung gefunden ist.' He goes on to point out that regarding *τοῦνεκα*, *οὔνεκα* as remodellings of *τοῦ* (f) *έκα*, *οὔ* (f) *έκα* would rid us of an 'auffällige Krasis'.

I hope that the distribution of the forms in the *Iliad*³ will take us further. *Εἵνεκα* will be the normal continuant of *ἐνφέκα*. It appears in the oldest parts of the *Iliad*: A 174 *λίσσομαι εἵνεκ' ἐμείο μένειν*, A 298 *μαχήσομαι εἵνεκα κούρης*, A 336 *Βρισηΐδος εἵνεκα κούρης*, Z 328 *σέο δ' εἵνεκ' αὐτή*, Z 356 *εἵνεκ' ἐμείο κυνός*, Z 525 *οἱ ἔχουσι πολὺν πόνον εἵνεκα σείο*, Ξ 89 *ἧς εἵνεκ' οἷζύομεν*, Σ 171 *οὐ εἵνεκα φύλοπις αἰνή*, and continues throughout the poem: A 214, B 138, 177, 377, Γ 87, 100, 128, 290, E 651, H 374, 388, I 560, 637, K 27, Ξ 309, Π 539, Τ 58, 325, Υ 235, Χ 236, Ψ 608, Ω 106, 501, Β [161], Σ [498].

When *φέκα* is used outside of the phrase *ἐν féκα*, the sound represented by *f* disappears; for instance *οὐ féκα* > *οὐ* (f) *έκα* > **ούκ έκα*. A phrase with *ἐν* may be built again: *ἐν-εκα*. Compare the way in which *εἰς ὦπα* is formed, though *ἐν ὦπα* has not been forgotten.

Then *ἐνεκα* should not be found in passages composed before the change *f* > 0. **Ενεκα* is generally found in late environments: Γ 57, 100, 206, E 640, Θ 428, I 327, 339, P 92, Σ 457, Τ 21, 298, Φ 380, 463, Α [110], Ω [28]. Only four passages need to be looked upon as modernized: A 94 *ἀλλ' ἐνεκ' < ἀλλά féκ'*, A 152 *Τρώων ἐνεκ' < Τρώων έφεκ'*, Z 356 *'Αλεξάνδρου ἐνεκ' < 'Αλεξάνδροιο féκ'*, Π 18 *ὑπερβασίης ἐνεκα σφῆς < ὑπερβασίας έφεκα σφῆς*.

Τοῦνεκα is found in fourteen passages, of which A 96, 291, Z 334 must once have had *τοῦ féκα*.

Οὔνεκα is found in 34 passages, of which A 11, 111, Z 78, Ξ 366 must once have had *οὔ féκα*.

The above is presented not as proof of what happened, but as a sketch of one combination of circumstances that may have led to our text of the *Iliad*. I hope it may be so regarded until one with greater probability is discovered.

GREEK ETYMOLOGIES

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1. UWAIŠ ZAN. These observations on Cypriote *uwaiš zan*¹ are intended chiefly as comments on Eric P. Hamp's treatment in *CP* 48.240-3 (1953), but take into account also the previous bibliography² and attempt to provide some

¹ There is no need to consider the *Odyssey*, since all of it was composed after the loss of *f*. In his *Homerische Kunstsprache* 204, Meister makes a surprising statement: 'In der *Odyssee* kommt nur *ἐνεκ' εἵνεκ' ἐνεκεν*, nicht *εἵνεκα* vor.' But *εἵνεκα* is frequent, and *ἐνεκα* is not cited by Gehring except under *οὔνεκα* and *τοῦνεκα*. Any significance that the facts may have will be in another connection.

² [All Greek forms in this article have been transliterated by the Editor according to the principles set forth by André Martinet, A project of transliteration of classical Greek, *Word* 9.152-61 (1953). Most forms will be obvious to those familiar with Greek; note especially that *w* represents digamma. The letters *ε̄* and *ο̄* (or *ε̂* and *ο̂* with circumflex) everywhere represent eta and omega, except that in Laconian *āniokhtōn* the *ο̄* stands for a long omicron.]

³ Cf. Deecke, *BB* 8.149-50 (1884), with references to Ahrens; J. and T. Baunack, *Studien* 1.16-8 (Leipzig, 1886); R. Meister, *Die griechischen Dialekte* 2.221, 227, 254, 284-5 (Göttingen, 1889); O. Hoffmann, *Die griechischen Dialekte* 1.71-2 (Göttingen, 1891); F. Bechtel, *Die*

new suggestions. Hamp's analysis, *uwais zan* < **u ais zan* < **u aiwis zan* < **u aiwins z(w)āōn* < IE *(H)*ud Haiwins Hywāsōm* 'per saecula saeculorum', conceives the expression as a petrified formulaic phrase. The interpretation of *u* as the Cypriote preposition corresponding to *epí* and cognate with Skt. *ud* 'up, forth'³ follows well-established tradition. Hamp takes **aiwins* to be an *i*-stem accusative plural, and thus the first part comes to parallel closely Goth. *in aiwins* and Gk. *eis aiōnas*. This contradicts the argumentum e silentio in Schwyzler-Debrunner's *Gr. Gramm.* 2.432 (Munich, 1950), where, on the basis of Cyp. *u tukhā* 'epí tukhēi', it is claimed that *u*, like *sún*, governs only the instrumental dative. A remarkable correspondence to such use of **ud* as a preposition with the accusative has now appeared in the new Venetic transcriptions from Lagole; see M. Lejeune, *REA* 54.74-6 (1952). We find *u. dono.m.* 'for (?) a gift' and *u. teu.ta[m]* 'for (?) the people' (Celtic form *teuta*), which presumably point to **ūd* with loss of final -*d* after a long vowel. This parallels the use of *epí* in expressions like *epí toúto* 'for this purpose', *epí ti?* 'to what end?'.

The crux of the matter has always been *zan*, and here problems still remain. Naturally, since the phrase forms a unit, any given approach to *zan* will affect the validity of the whole explanation. Hamp's discussion is devoted largely to early noun structure; he makes a good case for a collective **HywāH* as basis for a feminine genitive plural **z(w)āōn* > *zan*. Yet it is hard to realize why two different noun formations would be used in the same repetitional phrase. To clinch his connection, Hamp tacitly subscribes to the convincing argument of Hammerich and Lehmann that a word-initial cluster of voiced laryngeal plus the nonsyllabic allophone of the resonant /y/ yields *z-* in Greek. This involves an assumption that the *a*-colored laryngeal of **Haiw-* and **Hywā-* is voiced; if substantiated beyond doubt, it would be an important point in the laryngeal theory. The troublesome feature concerns the clustering patterns of laryngeals and resonants which are posited in Hamp's approach. Doubtless some elements of Edgerton's 'allophonic interchange' of IE resonants represent an ideal schematization; anyone who observes the inconsistent workings of Sievers's law in Vedic or other IE dialects should be disinclined to postulate a perfect mechanism for the Indo-European period. Yet a cluster like **Hyw-* would seem to contradict the basic rule that a resonant between two nonsyllabics is syllabic. This is a difficulty which Hamp does not mention; nevertheless, his viewpoint may be accepted provisionally as a working hypothesis in the hope that he will clarify and justify it further.

The root with which we are dealing is IE **Héy-w*⁴ (with tentative voiced *a*-
griechischen Dialekte 1.440-1 (Berlin, 1921); C. D. Buck, *Greek dialects*² 97 (Boston, 1928); F. de Saussure, *Recueil des publications scientifiques* 457 fn. 2 (Genève, 1922); E. Schwyzler, *Gr. Gramm.* 1.631 fn. 2 (München, 1939).

³ Cf. Walde-Pokorny 1.189-91. See also P. Kretschmer, *KZ* 31.415-6 (1892); F. Solmsen, *KZ* 34.450-1 (1897).

⁴ My reconstructions are phonemic, but attempt to give some allophonic interpretation of resonants on Edgerton's principles, however schematized; thus /y/ may appear as *y*, *i*, or *iy*. The laryngeals are treated consistently as consonants. In reconstructions with laryngeals the macron denotes a long-grade or contracted vowel; otherwise a long vowel of any provenience. Next to an *a*-coloring laryngeal the basic vowel *e* is written rather than an allophone *a* (as yet nonphonemic).

colored laryngeal), seen in Ved. *āyuh* 'vital force', *āyú-* 'lively', Gath. *āyu-* 'span of life', Hom. *aión* 'vital force, life'; already de Saussure (*Recueil* 457) surmised that the IE words for 'young' (Skt. *yuvan-*, Av. *yava-*, Lat. *iuuenis*) belong to the same root (i.e. **Hy-éw-*); Benveniste (*BSL* 38.103-12 [1937]) has since demonstrated how out of these notions evolved the IE idea of eternity (Av. *yavāi*, Gk. *aiet* 'ever'; Av. *yavāštāt*, Gk. *aión*, Lat. *aeuus*, Goth. *aiws* 'eternity'). When de Saussure proposed comparing *uwais zan* with Av. *yavāz-fi-* (Yt. 4.4, 19.11, 39.3) 'ever-living', he was thus still operating with the same root. This is the only serious rival interpretation to the main line of thought from Ahrens to Hamp. Formerly the two approaches to *uwais zan* could be said to exhibit a certain amount of genetic affinity, but by accepting Hamp's **z(w)āōn* < **Hywāsōm*, de Saussure's etymology is rendered phonemically impossible: an *uwais zan* should go back to **huwai-zēn* (if *-s* is an extension, and *zan* is secondary for *zēn*; cf. Schwyzler, *Gr. Gramm.* 1.631) < **Hyuwāy-g^wyē-* < **HyuweHey-g^wyeH-*; but if *H-* was voiced, we should have instead **zuwai*.⁵

Hamp's **zāōn* would seem to be supported by the generally accepted comparison of Avestan *avi-yāo-* 'adult' with Hom. *ai-zēós* 'vigorous, sturdy'; yet the Avestan form is a ghost word unknown to Bartholomae's *Altiranisches Wörterbuch*, but innocently perpetuated through Boisacq's and Hofmann's etymological dictionaries. The comparison itself is attractive, and it is surprising that H. Frisk (in his new *Griechisches etymologisches Wörterbuch*) implicitly classes it with other unmentioned and inconsequential interpretations, but at the same time fails to unmask the irreality of the connection itself. In these circumstances *ai-zēós*, and likewise *minúzēon* : *oligóbion* (Hes.), may rather contain **-g^wyeHw-*. Yet the other possibility remains; in a reconstructed **-Hyēw-* the long grade raises the question whether Van Langenhove (*Linguistische Studiën* 3.87-100 [Antwerpen, 1946]) was not on the right track after all in looking for a laryngeal extension in **Héy-w-*; this would yield a nominal formation **Héy-H-w-* with two weak-grade increments. **Hy-éw-* could then be analyzed as **Hi-H-éw-*, and **-Hyēw-* as **Hy-éH-w-*. Yet for lack of adequate evidence I prefer to regard **-Hyēw-* as the long grade.

Let us also consider for a moment the island name *Zákunthos*. The odds are heavy that this is non-Greek, like *Ithákē* and *Kórkūra*, and the suffix *-unthos* adds further weight to such a belief. But since *-inthos* and *-unthos* were stereotyped suffixes for place names, there can be no serious objection to looking for Greek elements in the first part. Hence *Zak-* may perhaps be analyzed as **zwak-* < **Hywŋk-*, showing a formation like that of Lat. *iuuencus*, Skt. *yuvāś-* (**Hyuwŋkó-*), W *ieuanc*.⁶ If we remember that Homer's *hulēssa Zákunthos* resembled another island described as *aigibotos d'agathē kai bouibotos, esti mēn hulē | pantolē, en d'ardmoi epētanoi paréasi* (Od. 13.246-7), we may perhaps think of island names like *Eúboia* and connect *Zak-* with Lat. *iuuencus* much as Osc. *Vítel(l)íú* 'Italy' (originally 'pasture land') belongs with Lat. *uitulus*.

2. HĒNĪĀ. The affinity of Gk. *hēnīā* 'bridle' with Skt. *nasyā* 'id.' and the IE

⁵ Likewise, if Döhring's old connection of Gk. *hyákinthos* with Lat. *iuuencus* needed any further refutation (see Boisacq s.v.), this argument would be helpful.

⁶ For the exceptional lack of metric 'position' before the initial *Z-* of names like

word for 'nose' is asserted by Boisacq, but the proposed reconstructions are imperfect both phonemically and in relation to root theory. Walde-Hofmann (s.v. *ānsa*), on the other hand, reject this connection and compare *hēntā* with Lat. *ānsa*, Lith. *qsd*, OPruss. *ansis* 'handle', Mlr. *ēsi* 'reins'. 'Handle' would seem a more probable original meaning than 'bridle, rein', but the specialized concordance of Gk. *hēntā* and Mlr. *ēsi* may also warrant the inverse assumption. If so, the two etymologies are no longer mutually exclusive, but may be united under a common root structure. The main cognates would be Ved. *nās*- 'nose', du. *nāsā* 'nostrils'; Av. *nāh*-, *nāṇhā*; OPers. *nāham* 'narem'; Lat. *nāris* 'nostril', *nāsus* 'nose'; Oícel. *nqs* 'nostril', OE *nasu*, OHG *nasa*; Lith. *nasraĩ* 'nostril', *nósis* 'nose'. An initial laryngeal is indicated by Vedic lengthenings in compounds like *urūṇasāú* 'broad-nosed' (RV 10.14.12). We have probably a root with two weak-grade increments, pointing to a nominal formation, viz. **Hén-H-s-* with two *a*-colored laryngeals. With the various full-grade possibilities of the extensions this gives **Hn-éh-s-* > **nās*- and **Hṇ-H-és-* > **nas*-, which account for most of the cognates cited above. Like *ānsa* and the like, *hēntā* is probably from **Hén-H-s-*, i.e. **Hen-H-s-iyēH* > **anstā* > Dor. *āntā* (> *hēntā*), just as **éphansa* > **éphāna* (> *éphēna*). The rough breathing of Attic and Homeric Greek can hardly have an IE source. However attractive it may be to seek its origin in the laryngeal or the *s*, the regular root structure presented above seems to rule this out. A voiceless laryngeal yields preaspiration in Greek only initially before a nonsyllabic resonant (as in *hestiā*), and the *s* is too far removed from the initial to permit transfer of aspiration. Besides, the Laconian of the Stele of Damonon, where *h* is carefully preserved and written, has *ānio-khōn* 'hēniokhēōn', which shows Doric *āntā* to be a regular historical form.

3. *ÁLALKON*. The Epic augmentless thematic reduplicated root aorist *álalkon* looks irregular (as if from **álkō*) when confronted with the verbs *alékō*, *aléxō*; at first sight it seems to exhibit a type of root structure more reminiscent of *alkē*, *alkí*, *álkar*, *alkáthō*, *alkaios*, *álkimos*. These forms can be analyzed in terms of an IE extended root with an *a*-colored laryngeal: **Hél-k-* (*alkē*), **Hl-ék-* (*alékō*), **Hl-ék-* (Skt. *rākṣati*). With this in mind we may reconstruct, on the proper basis of **Hl-ék-*, the normal structure of the aorist in question: reduplication with *e*-vocalism, weak grade of the root, and thematic endings, i.e. **Hle-Hl-k-om*; with *a*-coloring of the vowels, loss of the laryngeals, and ensuing allophonic readjustments of the resonants, this yields precisely the form we have, *álalkon*. The initial *a*- here is thus also a 'prothetic vowel'.

A trace of the laryngeal is clearly present in the long reduplication of the Vedic perfect middle participle *rārakṣānā-* (RV 4.3.14). We can also establish a structural parallel Gk. *álalk-* : Skt. *rārakṣ-* < **Hle-Hl-k-* : **Hle-Hlōks-*, and Gk. *énegka* : *enénokha* < **He-Hne-Hṇk-* : **Hne-Hnōk-*, which brings *álalkon* into line with the general pattern of such formations.⁷

Zákunthos and *Zéleia*, see P. Chantraine, *Gramm. hom.* 1.110 (Paris, 1942). If we posit **zw-*, the reason for similar treatment is still equally compelling (avoidance of a sequence long-short-long). Since the occurrences are all late (Odyssey and Catalog of Ships), a digamma may also have been largely disregarded in this unusual combination.

⁷ [Cf. Werner Winter, On the origin of the samprasaraṇa reduplication in Sanskrit, *Lg.* 26.365-70 (1950).]

MORPHOLOGICALLY DETERMINED ALLOMORPHS IN SPOKEN TIBETAN

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Among the several marks which serve to distinguish the noun as a form class in spoken Tibetan,¹ its exhibition of morphologically determined allomorphs is of comparative as well as descriptive interest. These variants are found for a relatively small proportion of the total noun-morph inventory; and probably because the traditional Tibetan script gives little or no clue to their existence, they have been all but ignored in the previous treatments.² Nevertheless, they are found in some of the most common words of the language, and one cannot collect texts for long without encountering them.

Thus, beside CT³ *tá* 'horse', we find *táb-* in *tábḍug* 'horse race'; *khá* 'mouth' ~

¹ My field work in spoken Tibetan, in the course of which I collected most of the data here presented, was carried out in Darjeeling, West Bengal, from August 1953 to March 1954. It was made possible by a grant from the Board on Overseas Training and Research of the Ford Foundation.

² See for example Sir C. A. Bell, *Grammar of colloquial Tibetan*³ 15 (Calcutta, 1939): 'There are also other irregular sounds, but it seems unnecessary to enumerate them all here. They will be picked up more easily in the course of conversation.' In P. M. Miller's treatment they are to some extent recognized (see note 3); but his 'prenasalization' analysis, for which there is no formal basis, artificially segments the materials, and prevents him from facing the problem.

³ Abbreviations of languages cited: CT = Central Tibetan, WT = Written Tibetan, Lh. = Lhasa dialect, Ld. = Ladakh dialect; Ar. Ch. = Archaic Chinese, An. Ch. = Ancient Chinese; WB = Written Burmese, Bu. = spoken Burmese, Rangoon dialect. All citations in this paragraph are CT; below, citations are identified. The CT, Lh., and Ld. materials are from my field notes (see fn. 1); the transcriptions are phonemic.

Symbols employed have their usual values, but the following may be noted. Acute = high level tone, grave = low or in certain environments low-mid-low tone; *t*, *th*, *ḍ* = palatal stops. Final stops are unexploded; in CT and Lh. -*b* and -*g* are [p] and [k] with high level, [b] and [g] with low; in Ld., -*b* -*d* -*g* are unexploded [p t k]; all these stops have homorganic voiceless spirantal releases before pause. *ō* is mid-front rounded, *ū* is high-front rounded.

My main informant for Lh. was a former Lhasa lay-official now residing in Darjeeling and Kalimpong; his speech agrees well with the dialect recorded by Chao (Y. R. Chao and Yu Dawchuan, *Love songs of the sixth Dalai lama* = *Academia sinica monographs*, Series A, No. 5; Peiping, 1930). My informant for Ld. was an educated layman from Leh. My main informant for CT was a lama residing in Darjeeling. His dialect closely resembles that described by P. M. Miller, The phonemes of Tibetan (U-Tsang dialect) with a practical romanized orthography for Tibetan-speaking readers, *Journal of the Asiatic Society: Letters*, 17:3.191-216 (1951), at least as far as the phonetic data are concerned; but Miller's analysis, of which I find important parts unacceptable, leaves him with a phonemic transcription differing considerably from that which I employ here. (Georges de Roerich, Modern Tibetan phonetics, with special reference to the dialect of Central Tibet, *JASB* NS 27.285-312 [1931], is more an attempt to explain the writing system than to describe a dialect; as far as it goes, it deals with a dialect not unlike our CT.) WT is a literal transcription of the conventional Tibetan orthography, but distinguishing subscript *i*, here written *j*, from prefix plus *i* or initial *i*, here written *y*, on the basis of what was evidently a phonemic distinction in the dialect reflected in the WT orthography. Ar. Ch. and An. Ch. are reconstructed by B. Karlgren, *Études sur la phonologie chinoise* = *Archives d'études orientales*, Vols. 12, 13, 19, 24 (1915-26), and conveniently available in his *Analytic dictionary of Chinese and Sino-Japanese* (Paris, 1923); for Ar. Ch., see his *Grammata serica* = *BMFEA* 12.1-471 (1940). WB is a literal transcription of the orthography; Bu. follows W. Cornyn, *Outline of Burmese grammar* (Language dissertation 38; Baltimore, 1944), with a few changes in symbols but not in analysis.

khám- in *khámdzü*⁴ 'case for litigation' ~ *kháb-* in *kháblòg* '[lying] face downwards', *khábsè* 'food' ~ *kha-* in *khálèn* 'a mock fight'; *ká* 'word, order' ~ *káb-* in *kábdòn* 'a rebuke' ~ *kán-* in *kándür* 'section of the canon', *kándi* 'a request' ~ *ká-* in *kádü* 'a chastisement'; *kü* 'body, respectful prefix' ~ *kúr-* in *kúrdáb* 'falling down' ~ *kúm-* in *kúmdün* '[your] presence' ~ *kü-* in *kúqin* 'kindness'; *tshág* 'hand [resp.]' ~ *tshán-* in *tshándzö* 'treasurer', *tshándi* 'small knife'; *lá* 'god' ~ *lé-* in *lésá* 'Lhasa'; *ló* 'south' ~ *lób-* in *lóbqäg* 'Southern Rock [PN]'; *sér* 'gold' ~ *sé-* in *sélay* 'a gold coin'; *là* 'mountain pass' ~ *láb-* in *lábtshtigäy* name of a mountain, perhaps Everest.⁵

These allomorphs are of considerable interest for comparative grammar involving Tibetan, where discrepancies in morph-final consonants have long caused investigators serious difficulties.⁶ When Karlgren reconstructed Ar. Ch. 'two' as **nijər*, the final **-r* here was partly in recognition of the final *-s* in WT *gnjis*,⁷ CT Lh. *ñi* 'two'; and these do indeed reinforce one another, so to speak, well enough. But this reconstruction by Karlgren is only part of a whole system, which ends in reconstructing final consonants in Archaic Chinese in what often seem, from the viewpoint of Tibetan, to be strange and difficult places. Thus, the same reconstruction which gives us the convenient **-r* in Ar. Ch. **nijər* 'two', gives us also the remarkably inconvenient **-d* and **-g*⁸ in Ar. Ch. **sijəd* 'four' and **kijüg* 'nine'; cf. WT *bzji*, CT Lh. *ši* 'four', and WT *dgu*, CT *gü*, Lh. *khü* 'nine'.

It is here that the morphologically determined allomorphs of spoken Tibetan, once described, can help in the resolution of apparent difficulties. With Ar. Ch. **sijəd* 'four', we should compare not only WT *bzji* and its kin, but also the original allomorph revealed by such modern reflexes as Ld. *žer-* in *žergu* '49', and CT *šer-* in *šergü* '49',⁹ while with Ar. Ch. **kijüg* 'nine', instead of simply WT *dgu* and its kin, we should compare also the allomorph Ld. *kog-* in *kogšig* '91', *kognyis* '92', and *kogsum* '93'.¹⁰

Another illustration from the number system concerns a form in which the Chinese final consonant is far more securely established than the **-d* and **-g* with which we have been dealing, An. Ch. **ziəp* < Ar. Ch. **diəp* 'ten', where

⁴ *-dzü* < *tshü-* 'beak'; cf. *tshütó* 'lip'. The morphophonemic change here, though sporadic in its operation in CT, is otherwise identical with that described for Bu. by Mary R. Haas, The use of numeral classifiers in Burmese, *Semitic and oriental studies presented to William Popper* 11.191-200 (University of California publications in Semitic philology, 1951).

⁵ A small subclass of verbs shares this formal mark with these nouns.

⁶ For bibliography of these problems see S. N. Wolfenden, *Outlines of Tibeto-Burman linguistic morphology* 203-14 (London, 1929); R. A. D. Forrest, *The Chinese language* (London, 1948).

⁷ For this transcription, and those below, see my remarks in *Wennti* 5.14-6 (1953).

⁸ I.e. final /d/ and /g/, perhaps better interpreted phonetically as spirants; cf. my discussion, A note to Karlgren's *Phonologie*, *The Sino annual: Papers of the University of California oriental languages honor society*, Vol. 3 (Berkeley, 1952).

⁹ The complete statement for this allomorph in the forties of the Ld. number system is as follows (numbers give the expression where the particular allomorph is found): *ža-* 45, 46 ~ *žib-* 40 ~ *žag-* 41, 42, 43 ~ *žab-* 44, 48 ~ *žeb-* 47 ~ *žer-* 49. In CT we find: *šə-* 41, 42, 43, 44, 45, 46 ~ *šib-* 40 ~ *šəb-* 47, 48 ~ *šər-* 49. Lh. offers no materials here.

¹⁰ The complete statement for this allomorph in the nineties of the Ld. number system is as follows: *ko-* 95, 96 ~ *kub-* 90 ~ *kob-* 94, 97, 98 ~ *kog-* 91, 92, 93 ~ *kor-* 99. In CT the morph appears in different shapes: *gó-* 91-99 ~ *güb-* 90, not useful here.

the $*-p^{11}$ is attested by living dialects on the one hand and by ancient loans into several contiguous languages on the other. Here we may compare with Ar. Ch. $*d̥jəp$ not only WT *btju*, CT Lh. *tšú*, but also the allomorphs Lh. *tšúb* and *tšób*- in '14', '17', and '18', CT *tšób*- in '18', and Ld. *tšub*- in '14', '17', and '18'.¹² Here the vocalization of the Tibetan forms seems to be a secondary development; cf. WB *sām* > Bu. *θqú* 'three', as against WB [ta] *chay* > Bu [ta] *s'é* 'ten'. Perhaps it has resulted from assimilation before the labial final; the parallel *a/u* in Ar. Ch. $*səm$: WT *gsum* 'three' :: Ar. Ch. $*d̥jəp$: Proto-Tibetan $*djub$ 'ten', is rather striking. This lost final $*-b$ in Proto-Tibetan would also explain the vocalization in *-u* of WT *btju* 'ten', the ablaut vowel persisting after the loss of the final from most forms.

Comparison of this type is equally effective with other members of the form class, of which the numbers are only a subdivision. For example, with An. Ch. $*k'jwen$ 'dog' we may compare the allomorph of CT, Lh. *thi* 'dog' in *thibitšay* 'jackal' (WT *khji.spjan*!). WB *khwə* > Bu. *khwəi* 'dog', together with complete loss of the final, shows a trace of the same $-y-$ preserved in the Chinese form, where there has been subsequent dissimilation between it and the labial final.

Two points are worth emphasizing: first, that in this aspect of Tibetan comparative grammar it is to the modern dialects that we must look for our materials,¹³ not to the forms preserved in the orthography of WT; second, that we must gather these forms from a variety of dialects, and must exploit the total resources of primary comparison of these dialects with each other.

¹¹ Again, perhaps better [-b]; cf. fn. 8.

¹² The complete statement for 'ten' in the teens of the number system for CT, Lh., and Ld. is as follows: CT *tšug*- 11, 13, 16 ~ *tšúg*- 12 ~ *tšúb*- 14, 17 ~ *tšúr*- 19 ~ *tšób*- 18 ~ *tšō*- 15; Lh. *tšug*- 11, 12 ~ *tšóg*- 13 ~ *tšúb*- 14, 17 ~ *tšób*- 18 ~ *tšú*- 16, 19 ~ *tšō*- 15; Ld. *tšug*- 11, 12, 13 ~ *tšub*- 14, 17, 18 ~ *tšur*- 19 ~ *tšu*- 16 ~ *tšo*- 15.

¹³ This is not to say, of course, that such morpheme variants are not of old standing in the Tibetan dialects. The 9th-century inscriptions clearly show them in operation even at that date; cf. Paul Pelliot, *Quelques transcriptions chinoises de noms tibétains*, TP 16.1-26 (1915), and see P. Demiéville in Pelliot's posthumous work, *Les débuts de l'imprimerie en Chine* 136-7 (Paris, 1953).

REVIEWS

Die Gliederung des indogermanischen Sprachgebiets. By WALTER PORZIG. (Indogermanische Bibliothek, 3. Reihe: Untersuchungen.) Pp. 251. Heidelberg: Carl Winter, Universitätsverlag, 1954.

Reviewed by W. P. LEHMANN, *University of Texas*

If we set out to reconstruct Proto-Indo-European or even early stages of the Indo-European dialects, we need to have a grasp of their earliest ascertainable geographical relationships as well as of the chronological relationships of our data. Although considerable material on Indo-European dialect geography has been published recently, for the standard comparative works the criteria primarily utilized have been chronological. Since, at the time that these works were prepared, Vedic Sanskrit and Homeric Greek comprised the oldest dialect materials, Proto-Indo-European was reconstructed chiefly on the basis of these two dialects. Little attempt was made to determine whether the geographical locations of pre-Greek and pre-Indo-Iranian might have been peripheral rather than central among the Indo-European dialects, and whether accordingly other dialects might represent a less modified form of Indo-European.

The relatively late attention to the geographical relationship of the early Indo-European dialects has various reasons: (1) dialect geography is one of the more recently developed techniques of linguistic study; (2) the chronological relationships of our Indo-European materials are indisputable historical facts, while the geographical relationships have largely to be inferred from internal linguistic evidence; (3) the neat Proto-Indo-European reconstructed on the basis of Sanskrit and Greek, seemed to have been disrupted, in other dialects, by a millennium or two of further development, until the discovery of our oldest Indo-European materials in Hittite, which in some respects resembles Greek, Sanskrit, and the putative Proto-Indo-European less than it does some other dialects. Future Indo-European comparative work will accordingly pay greater attention to the geographical relationships of the dialects. Porzig's book, whose aim is to examine the results of previous work on the distribution of the Indo-European dialects and to contribute further data, is therefore very welcome at this point.

In carrying out his aim, Porzig uses stringent methods to establish interrelationships; he discusses the methods in detail after surveying at length the work of his predecessors. Since in many areas we must operate with little evidence, Porzig's conclusions and those of his successors depend largely on the validity of his methods; hence these are probably the most important element of his book to survey here.

Arguments which make use of negative evidence are not allowed. If in one area we find a form x meaning 'a-c', in another a form y meaning 'b-c', Porzig demands that we investigate whether in other areas x means 'b-c' and y means 'a-c'; further, that we find the linguistic form conveying 'b-c' in the area in which x means 'a-c', and the converse for the y area. Without such data he denies the validity of the evidence. But even if these data are available, he may use the

evidence with caution, for both x and y may be late innovations, neither of them the Proto-Indo-European form. Proceeding with such care, Porzig eliminates many of the comparisons used in earlier work. Moreover, he does not aim at drawing a map of the original location of the Indo-European dialects, or at ascertaining that such dialects as Indo-Iranian and Slavic were spoken in contiguous areas, or even at stating that pre-Indo-Iranian and pre-Slavic were spoken in contiguous areas. The only conclusion that Porzig thinks our materials will allow is that the similar Indo-European materials, e.g. those found in Indo-Iranian and Slavic, can be ascribed to contiguous areas of the Indo-European linguistic territory. Hence the title of his book. And such conclusions he permits only when two dialects are the sole repositories of linguistic innovations. With his cautious aims he does not even sketch a stylized map giving the 'original' locations of the Indo-European dialects, like those which we find in our handbooks. Although, because of his severe methods, he is not left with an abundance of evidence, when he discusses the relationship of Germanic and Italic he finds 32 different criteria for setting up isoglosses, a number which in view of the availability of evidence may be not unfavorably compared with the 114 used by Hans Kurath in *A word geography of the eastern United States* in establishing the relationship of American dialects.

Like Kurath and other linguists today, Porzig makes considerable use of lexical data. Most earlier Indo-Europeanists did not; instead, following Schleicher, they based their statements on phonological and morphological criteria. But as Porzig points out, Schleicher's aims differed from his. To Schleicher, lexical evidence was of limited value because he wished to establish a family tree of the Indo-European dialects, and lexical items are more readily borrowed than phonological or morphological items. Since Porzig attempts only to discover contiguous dialects, borrowings are as useful as are phonological changes which at a specific time may differentiate one dialect area from all others; accordingly lexical evidence is of prime importance, even if it is not derived from that central section of the vocabulary which is now being used by the glottochronologists.

Between Schleicher's day and ours, there have been many studies of the relationship of dialects to one another, and of linguistic change. These studies have largely been a part of the discussion concerning the family tree theory and the wave theory. Both of these theories have now been greatly altered from their original forms, and there is only historical reason for investigating what they meant to their authors, our predecessors; probably most historical linguists today would be surprised to learn that for the author of the wave theory the waves were languages, not specific linguistic items. Moreover, it would be idle to discuss at length the overliteral application of these symbols; they probably have done no more harm than others introduced into linguistic theory and still in use. Each theory, in adequate form, applies to a specific type of linguistic expansion. The type of expansion which the family tree theory was designed to account for is carried out by migration; the type treated in the wave theory is diffusion from a central source. Linguistics has now developed far enough so that we need no longer label processes of language development with colorful epithets, whose chief purpose seems to be to instruct the intellectual featherweights and to pro-

vide material for polemics. Since historical linguistics is now being refashioned on the basis of increased historical work and of the improved descriptive techniques of the past decades, we can aid our successors by abandoning the fanciful labels in favor of sober descriptive terms. For the two processes of linguistic extension, I suggest EXPANSION BY MIGRATION and EXPANSION BY DIFFUSION. Although this is not the place to discuss either process, they are central to Porzig's book and to any analysis of the spread of the Indo-European dialects.

Expansion by migration, though dealt with earlier, has been less thoroughly explored than expansion by diffusion. For further linguistic work we will need to ascertain the varying effects produced by types of migration as different as (1) the spread of German to Pacific Islands like Samoa by a few administrative superiors, (2) the spread of Pennsylvania Dutch to America by a small group of settlers who after some time received no additions from their area of origin, and (3) the spread of English to America by a relatively large group of settlers who maintained contact with their area of origin by the constant arrival of new settlers and the return visits of old ones. These three types vary according to their relation to the indigenous groups in the new areas. Moreover, they vary only in degree, not in kind; probably no single spread is pure in type; rather, it will fit somewhere between the three types listed. Further settlers of type 1, and even of types 2 and 3, may not be able to establish their language permanently, as we note from the fate of German in Samoa; in the early history of the Indo-European dialects, the gradual disuse of dialects like Oscan, Gaulish, Thracian, and probably Illyrian is of great importance, and makes difficulties for us when we attempt to fix the relative position of the Indo-European dialects. To type 1 belongs the expansion of the Indo-Iranian dialects among the Mitanni, and probably that of Hittite and Tocharian (cf. Porzig 63). To type 2 belong the Greek colonies, such as those on the Black Sea, and the Roman settlement in Rumania. To type 3 belong the Anglo-Saxon settlement of England and the Norwegian settlement of Iceland. But in general, very little is known about the expansion of most Indo-European dialects—whether Sanskrit was brought to India by large numbers of settlers, what the history of Germanic was between Indo-European times and our earliest records, and so on. We need studies of the histories of type settlements in recent times; for we agree thoroughly with Porzig's statement (27), that the study of living languages has been too widely neglected in attempts to deal with problems of Indo-European dialects. Such studies, after comparison with known types of expansion in ancient times, may give us patterns for determining the types of migration among specific early Indo-European settlements. Possibly after such studies we will not consider merely symbolic the early statements on the spread of Indo-European groups, like Jordanes' that the Goths migrated from Scandinavia to the mainland in only three ships.

The principles of expansion by diffusion have been more exactly formulated, especially by dialect geographers and neolinguists. We need not list the contributions of dialect geography, but may note that Porzig reviews and discusses Bartoli's four principles, which were proposed to deal with geographical situations for which we have no socio-historical data other than linguistic: (1) of two transmitted linguistic items, if one has become greatly restricted in usage and the other

is widespread, the former is generally the older; (2) of two linguistic items, if one is found primarily in peripheral areas and the other in central areas, the former is generally the older; (3) of two items, if one is found among the larger proportion of speakers, it is generally the older; (4) of two items, if one is found in areas of little access to outside contacts, it is generally the older. Porzig, like Bàrtoli, finds the last two principles restricted in usefulness, especially when we have no exact information on the social conditions under which the dialects were spoken. But it would be erroneous therefore to discard them, for we have come to realize that in linguistics as in the other social sciences we cannot always set up optimally valid sets of principles; rather, we rank the usefulness of our principles by the frequency with which they are applicable, and if possible apply those of highest rank.

By the use of these formulations of linguistic expansion, the historical study of language groups will become increasingly complex. But this is in line with Porzig's expectations, and one of the reasons why he wrote his book at this time, summarizing previous discussions of the problem. With the increase of complexity there may be greater hope of correlating linguistic and archeological evidence; but since archeological data are also increasingly complex, specialists from various fields will have to collaborate in prehistorical study. Though some linguists have attempted to treat archeological findings also, Porzig limits himself to linguistic data.

For setting up his primary subgroups of the Indo-European area, Porzig uses seven phonological and four morphological items. The phonological items are the development (1) of the vocalic allophones of /r l m n/, (2) of voiced aspirate stops, (3) of palatovelars as stops or fricatives, (4) of dental stop plus /t/, (5) of the group /sr/, (6) of final /m/, and (7) of consonants in the Germanic and Armenian shifts. Of these, the third has been most widely discussed, and has given rise to the centum-satem dichotomy of Indo-European. Porzig's conclusions are characteristic of his approach. He does not believe that the Indo-European homeland was divided into a centum and a satem area, but rather that relatively early, about 1500 B.C., an innovation spread from the Indo-Iranian speakers by which the palatal allophones of Indo-European velar stops became spirants. We have here then an example of expansion by diffusion, and in this sense it is valid to speak of Indo-European satem dialects.

The morphological items are (1) /r/ verbal endings in the middle voice, (2) indication of time relations in the verb, (3) formation of the genitive singular of *o*-stems, and of the dative-instrumental plural, and (4) indication of durative action. The criteria used to establish relations between individual dialects are too numerous to list.

Through his examination of these items Porzig finds that Indo-Iranian, Celtic, and Italic share no innovations; forms which they have in common belong to the oldest Indo-European material. Hence Indo-Iranian and Celtic are assigned to peripheral areas. Baltic and Slavic also give evidence of peripheral location. Innovations on the other hand can be found in two groups of dialects: Celtic, Latin, Oscan-Umbrian, Germanic, and (as far as materials are available) Illyrian give evidence of a long period of geographical contiguity; Indo-Iranian, Baltic,

Slavic, Greek, Armenian for many items, and (as far as materials are available) Thracian and Phrygian show other, though fewer, common innovations. Although the evidence is limited, Hittite, Tocharian, and Albanian are to be classed with the eastern group. But dialects from the eastern and western groups are not without common innovations, notably Germanic, Baltic, and Slavic among themselves, and with them also Tocharian. The time of the innovations can be approximated through lexical evidence. If, as between Greek, Thracian, and Phrygian, five of the twelve innovations precede the development of palatal to spirant, two show old ablaut relations, three show old types of word formation, and two refer to simple social relations (cooking and grinding), these can be assigned to a much earlier period than the seven Germanic-Celtic innovations, of which five refer to a more advanced social situation (paved floor, wagon, travel, harbor, free), even though the spread of these is relatively old—earlier than the loss of /p-/ in Celtic and the consonant changes in Germanic. On the other hand the absence of characteristic innovations may enable us to determine when the speakers of a dialect left their old area of influence. Greek did not share the shift of palatal stop to spirants because, when that shift took place, its speakers no longer lived in areas adjoining the other eastern dialects. In spite of Porzig's disclaiming such a goal, and the absence of a chart in his text, we can thus construct a chart of the early locations of the Indo-European dialects—quite easily on the basis of Porzig's last brief chapter, or even from the material presented here.

Since Porzig's book brings together the results of previous research in Indo-European, one might almost say the results achieved at the end of an era, it remains for us to examine the validity of his evidence and the likelihood of further data to support or extend his conclusions.

Two of Porzig's four conclusions derived from morphological evidence are based on developments of the Indo-European verbal system. In his discussion of these, Porzig's ideas are based on Brugmann's. While his conclusions are in general correct, a more valid analysis of the Indo-European verb might have given Porzig more material for conclusions on innovations in the dialectal verb systems. I agree with Porzig and Brugmann that tense was not indicated in the Indo-European verb; the verb consisted of a relatively large number of nonpredictable formations from roots (examples in the handbooks); for the Sanskrit root *ṭr-* 'pass', for instance, Whitney lists six possible present formations (*táratī, tiráti, tilartī*, etc.), besides five aorists, a perfect, and a future; see also WP 1.732-3. In Hittite, similarly, we find for 'sleep' an unextended form *še-eš-zi*, a form in *-sk-* (*še-eš-ki-iz-zi*), and a form in *-iya-* (*še-eš-ki-ya-aḥ-ḫu-ut*); see Sturtevant, *Comp. gr. Hitt. lang.*² 122. These extended forms presumably differed in meaning, although the differences are not always readily determinable; the Indo-European differences in meaning we can most simply refer to as differences in aspect. When, in the individual dialects, verb systems were developed with distinct paradigms, like Latin *intrō intrāvi intrātus intrāre*, the existing Indo-European forms were often used to mark the categories desired (forms in *s*-suffixes were in some dialects used as aorists) or were treated as separate verbs (like *trā-* in Sanskrit, *intrāre* in Latin). None of the dialects has maintained the Indo-European

system. In Germanic, Celtic, and Italic it was abandoned in favor of a system indicating tense; in Indo-Iranian and Greek, of a complex system indicating tense and aspect; in Slavic, of an aspect system, with tense as a secondary feature; in Baltic, of the reverse. This is only a superficial sketch, presented to suggest how structural shifts might be used as well as the phonetic and lexical innovations that buttress most of Porzig's arguments.

Porzig undertakes to examine some of the verbal shifts, but not with marked success. After noting that the difference between 'sit' and 'sit down' and similar pairs was indicated by differing stems, simplex and compound, or by different roots, he says (91): 'Der Begriff "sitzen" wird ausgedrückt durch lat. *sedeo sedēre* as. *sittiu sittian* lit. *sėdžu sėdėti* aksl. *sězda sědėti*. "Sich setzen" aber heisst lat. *sīdo sīdere cōnsēdī* got. *gasitan gasat* as. *gisittian* lit. *sėdu sėsti* aksl. *sęda sęsti*. Die germanischen Formen sind Neubildung von *gasat* aus, das dem lat. *cōnsēdī* entspricht. Das Perfektum der Wurzel **sed-* verlangte also sowohl im Lateinischen als auch im Germanischen ein Präverbium. Das bedeutet, dass sie von Haus aus perfektiv war.' The reverse seems true; if **sed-* was a perfective root, it would not have needed a perfectivizing prefix. And the suggestion (87) that Proto-Indo-European had a present, aorist, and perfect system is surely more valid for Sanskrit and Greek than for Proto-Indo-European.

The evidence that he cites to determine the relative position of various dialects, on the other hand, is well supported. Of the 32 items used to fix the relation between Italic and Germanic, few are dubious. Porzig suggests an Italic-Germanic isogloss by not admitting, as some other scholars do, a relationship between Lat. *vēr*, OIcel. *vār* 'spring', and the words in other dialects derived from IE **wesr*; he finds another in the designation for 'furrow' by Lat. *sulcus* and OE *sulh*, though only Walde-Pokorny gloss *sulh* as 'furrow', while Bosworth-Toller do not. But even if these two items are omitted, Porzig's evidence for the relationship of Germanic and Italic is impressive.

Commendable though Porzig's book is, we can look forward to more precision in relating the Indo-European dialects when structural criteria are applied. When these are used, the breakdown of the Indo-European resonant system may seem more significant than many of the consonant changes reported in the past. Further precision may be expected when the laryngeals are discussed; Porzig has only a passing reference to the laryngeal theory. For the suprasegmental phonemes, we lack at present even adequate descriptive data; but already we can amplify our understanding of the dialect relationships by noting such common innovations as the Italic, Germanic, and Celtic initial stress accent, which Porzig does not touch on.

Elements omitted may have been left out after careful deliberation, but one might have expected some discussion of the origin of IE *so sā tod* as explained by Sturtevant, on the basis of which an isogloss can be drawn between Hittite and the other dialects. Hittite, in fact, seems to be relatively neglected; there is no discussion even of the data presented as long ago as 1947 by M. Dillon, *TPhS* 1947.15-24, which point to a peripheral position for Hittite and Celtic: the use of verbal particles, the infixation of pronouns, and the expression of the relative.

Minor differences of opinion will always be met with in such a book; but one

must object to Porzig's statement that language mixture existed in England after the Norman conquest (61). Jespersen has pointed out convincingly, in Chapter 5 of his *Growth and structure of the English language*, that the great period of Romance influence did not come with the Norman conquest, but only 200 and more years after it.

For such a study as this the bibliography is enormous; Porzig's coverage is admirable. Certain errors, such as the repeated reference to Sturtevant as H. Sturtevant (even 16, where only the first edition of his *Comparative grammar of the Hittite language* is listed) and the location of *JEGP* at Ithaca, N. Y. (14), indicate greater familiarity with continental materials than with American. F. Specht's *Die Herkunft der Griechen und Römer und ihre Sprachen*, *Lexis* 3.69-74 (1952), the second edition of Sturtevant's grammar, and the sketch by George L. Trager and Henry Lee Smith Jr., A chronology of Indo-Hittite, *SIL* 8.61-70 (1950), may have appeared too late for inclusion.

It is hardly necessary to mention that some of Porzig's locutions show signs of haste; thus (89), 'Den Ursprung der Endung *-i* hat J. Wackernagel in altindischen Adverbialformen auf *-i* gefunden', where Porzig surely does not suggest that the Latin and Celtic genitive in *-i* is to be derived from Indic rather than from Proto-Indo-European. Further, it is best to avoid explaining the varying root forms **kabh-* **ghabh-* **ghab-* **ghap-* and possibly **kap-* by ascribing them (108) to 'verschiedene Articulationen einer Lautgebärde für "zupacken" ... Man kann noch genauer sagen, dass damit das Zupschnappen des Hundes gemeint war, dem man einen Bissen zuwirft.' We know too little of Proto-Indo-European to state which patterns seemed onomatopoeic to its speakers, and we have given up the romantic notion that the 'primitive' Indo-European speakers lived nearer the creative springs of poetry and evolved new locutions at will, which their descendants dully maintained.

On the other hand we cannot protest too vigorously against vivid locutions, such as one now finds even in carefully edited linguistic journals, possibly in the mistaken notion that they enliven the text. The sentence (66) 'Jede [Einzelsprache] von ihnen hat längere Zeit ihre besonderen geschichtlichen Schicksale erlebt, die ihre Gestalt bestimmt haben' tells us nothing. The locution (108) 'Auf dieser Grundlage haben die italischen Sprachen und das Germanische das Resultativum geschaffen' continues an old though not commendable practice of speaking about languages as though they were active forces. When he writes (157) 'Die Veränderung der Bedeutung ist für die griechische Geistesart kennzeichnend', the author can mean no more than that semantic change is frequent in Greek. To relate this to Greek Geistesart, known only from this and other linguistic phenomena, merely adds an intermediary which we cannot otherwise deal with.

Because of its collections of data, its coverage of previous work, and its careful method, Porzig's book is highly useful. If Porzig's successors are equally cautious in their method and their use of materials, we can look forward to more and more precise determination of the geographical positions of the Indo-European dialects.

Proto-Indo-European phonology. By WINFRED P. LEHMANN. Pp. xv, 129. Austin: University of Texas Press and Linguistic Society of America, 1952.

Reviewed by HENRY M. HOENIGSWALD, *University of Pennsylvania*

New data from Hittite and a methodology refined through advances in linguistic theory have suggested this 'reformulation of PIE phonology'. By way of an introduction the author sketches for us the classical 19th-century reconstruction of Indo-European. He stresses the crucial role of Germanic in this history, stating further that Proto-Germanic 'must be considered one of the most conservative IE dialects', the investigation of which 'may therefore yield important information for a solution of some of the remaining phonological problems of PIE'. Granting that Germanic is more archaic in certain respects than is commonly recognized, it has certainly innovated in other respects (fewer? more? weightier? less important?), and its alleged archaicism should not by itself be used to favor Germanic evidence in reconstructing a given form. It is pleasant to report that Lehmann, in the body of his study, has not succumbed to this danger.¹ As a trained and productive Germanic scholar he has merely made full and legitimate use of his erudition. IE studies have always needed other influences to offset the traditional Greco-Sanskrit bias.

The classical reconstruction has among its weaknesses those that are due to the insufficiency of data (for purposes of the comparative method, that is), though others are no doubt due to flaws in method. When 19th-century scholars went wrong they were often merely inconsistent, i.e. they failed to come up to their own standards. Had they been more consistent, they would have been, by and large, good 'structuralists', to use Lehmann's own word. True, their methodological uncertainty would sometimes prevent them from finding the best possible formulation: where they believed and protested that they were phoneticians, they were in fact phonemicists at a time and at a stage when the term did not yet exist, and the discipline on the synchronic level could not yet have existed. There was therefore superfluous quibbling about mere notations (e.g. a suggestive morphophonemic notation as against the more concrete phonetic). Far worse was the hostility which kept de Saussure's work from being really accepted. But there was hardly the thorough misunderstanding of language implied in Lehmann's too-harsh words. Brugmann was not a phonetician when, according to Lehmann, he should have been a phonemicist.² He was the latter, no matter what he called it himself. He may list the IE diphthongs like *ei* as phonemes along with the 'units' *e i y p bh* etc., and this may indeed have been unfortunate; but he of all linguists should not be seriously accused of having failed to understand the

¹ Weighing evidence pro and con is notoriously difficult. Counting features one by one results in illusory exactitude; while in any less explicit attempt at evaluation our deep-seated interest in archaisms will play us tricks: we are apt to forget mere innovations. Thus the Tennessee mountain folk are popularly supposed to speak renaissance English.

² Indo-Europeanists, working mostly with alphabetically written literary records, have had phonemic analyses of a sort made for them. It is an interesting fact that the one neogrammarian most seriously concerned with methodology was August Leskien, one of the very few scholars familiar with contemporary and not necessarily literary material (from Baltic and South Slavic).

systemic relations between *e*, *i*, *y*, and *ei*. That Hindu phonetics had lured him into an occasional unnecessary duplication like **n* **n̄* is true enough, but scarcely important.

On p. 1 *o* is misprinted for *n̄*. Proto-Celtic *p* is not a happy example to use in describing the nature of the Celtic stop systems (3), in view of the loss of IE *p* in all of Celtic, and its reappearance in only a part of the group later. For *pat-ōs* (3 fn. 5) read *pa-rōs*; for *o* (4) read *o*.

Lehmann then restates 'Brugmann's' reconstruction (Ch. 2). The restatement is different 'primarily in not including schwa, diphthongs, aspirated voiceless stops, or palatal stops.' There is also an *o* more or less equivalent to the old schwa secundum. Lehmann seems to reckon also with a distinctive accent for PIE, described as changing from phonemic stress to (non-phonemic?) stress to phonemic pitch to 'free pitch accent, like that of Vedic' (8), an obscurity which I shall take up later on. Lehmann seems to find that the location of these accents in the morphemes of the lexicon remained constant; only their phonetic character, and the degree to which they are tied up with other features (vowel contrasts, for instance), underwent variation in passing from older to more recent stages of Proto-Indo-European. There is a long effort to disclaim the existence of diphthongs as phonemes per se, and a résumé of Edgerton's semivowel ('resonant') theory. The structure of the root is then described, much along Benveniste's lines.³ There is, finally, a section on IE meter (i.e. Homeric and to some extent Indic), in which consonants, semivowels in 'diphthongs', and vowel length (introduced here on p. 20 as *X*, without comment) all function somewhat alike, suggesting a phonological reconstruction in which they come out as members of the same classification. The rather more specific argument used by Kurylowicz, and also by Maurer, is not discussed.

In 8 fn. 1, read *o* for *o*. Gk. *bíos* is not from *g^wtwos* (13). For *procas* 'wooer' (16) read *pro-cus*. Metricians will take exception to the statement (20) that the 'accented part' of the hexameter foot consists of a long syllable. P. Maas, for instance, simply identifies certain locations in the line as 'longa'.

The next chapter, on the laryngeal theory, is a useful account of work in this complex field. Included is Kurylowicz' splendid, half-forgotten explanation of the exceptions to Brugmann's (and Pedersen's) law: 'PIE *o* before a non-syllabic semivowel in an open syllable yields Indo-Iranian *ā*.' It is Skt. 1st sg. *cakāra* vs. 3rd sg. *cakāra* because in the 1st-person form the stem syllable was not

³ The well-known limitations existing between root-initial and root-final consonants (only in Benveniste's sense?) are not quite fully stated here; but see 17, with a reference to Meillet, *Introd.* 157. Not only are (a) identical consonants and (b) a voiceless and a voiced aspirated stop incompatible, but so are (c) two voiced unaspirated stops even if they are not identical, and, it seems, (d) any two stops in the same place of articulation. Following Z. S. Harris's observation that this seems to give the voiced unaspirated stops a central position in the system, one could perhaps formulate the situation like this: the zero grade of a root contains two different consonants (listed: *b d g s y w r l m n* and the laryngeals) and further, for *b d g*, one of these two: B(reath) and U(nvoicing). Either one of the latter may occur before the first, between the first and the second, or after the second consonant, and will affect the contiguous *b d g*. Thus *Ubd* = *ped*, *bUd* = *pet*, *bdU* = *bet*, *bdB* = *bedh*, *Ubm* or *bUm* = *pem*. U and B may or may not be identical with two of the laryngeals.

originally open: *cakára* is from **k^wek^wolXe* (Hitt. *-hi*), *cakára* is from **k^wek^wole* (Hitt. *-i*). Similarly, *jandyati* from **gonXeyeti* (hence not *†jāndyati*, which would presuppose *†goneyeti*), *genX* being a set base. This incidentally puts the survival of laryngeals, in this environment, into Indo-Iranian times. There is probably no other explanation which would fit the facts better.

For *láos* (26) read *laós*. The article from *Lg.* 17 (quoted 33) is by W. M. Austin. *we* etc. becomes *e* etc. in Attic (and other dialects) but not in 'Greek' (33), a fact which has a certain importance for the dialect chronology to be reconstructed within IE.

Next Lehmann examines a number of special problems from Germanic: Verschärfung ('PGmc *-w-* was lengthened after any short vowel when reflex of a laryngeal followed *-w-*; PGmc *-j-* was lengthened after *i* when reflex of a laryngeal followed *-j-*, and after *a* when reflex of a laryngeal preceded or followed *-j-*'); PGmc. *g* and (*k*)*k* corresponding to PIE */w/* (tentatively explained, in some disagreement with Austin, by a difference between laryngeals, *X*₁ and *X*₂); laryngeals after *r*, *l*, *m*, *n* (*r_eX* > *ru* as in OHG *hiruz*, while *X* following other consonants > *a*); the OHG *r*-preterits (like *anasteroz* from *-stozan*, with *-r-* from *Xw* and *Xy*, and preservation of the laryngeal throughout Proto-Germanic into OHG, where it merges with *r*.⁴ Lehmann also draws attention to Schulze's observation that Alemannic *āmar* and *enēr* without *j*—as contrasted with *jesan* 'ferment'—seem to correspond to cases in which Greek has rough breathing rather than *z-*: *āmar* : *hēmeros*, *enēr* : *hós*, but *jesan* : *zēō*). The results are summarized in Chapter 8, where an early stage of Germanic is credited with *i e a u ī ē ō ū f þ x b̥ d̥ z p t k s z h r l m n w j X₁ X₂*. Finally, Lehmann turns to *ē̃*, the difficult high vowel of Germanic (OE *ē*, Goth. *ē*, OHG *ea*, *ie*, etc.) and tries to explain it as a Gmc. development based on cases of *eXy*, i.e. full-grade but not lengthened-grade *ēi*.

Intervocalic *w* was not lost 'in Greek', see above (36). Lat. *dēfrutum* 'must' with short *u* may still stand in the same relation to Thrac. *brátos* (provided we trust the circumflex in the tradition of the gloss, for which there is not the slightest necessity) as that of other short-vowel zero grades of heavy bases to long-vowel zero grades (Wackernagel, *Ai. Gr.* 1.92 ff.). One could see in *bhrut* the sequence *bhr_ewX₁t-*, in *bhrūt* thesequence *bh_e_orw_eX₁t-*, as implied in *Lg.* 28.291 f. This, to be sure, presupposes a somewhat different view of *e* from that propounded by Lehmann. How can OE *cēn* be equated with Russ. *sosná* (72), considering the initial stop?

As has just been hinted, laryngeals in sequence with semivowels appear to have been involved also in the twofold representation of so-called initial *y-* in Greek, viz. sometimes as *z-*, sometimes as *h-*; at least, all other explanations so far attempted have come to grief.⁵ Sapir and Sturtevant thought that an initial voiceless laryngeal plus *y* accounts for the cases with *h-*, while *y-* pure and simple

⁴ This is less implausible phonetically now that W. G. Moulton, *JEGP* 51.83 ff., has found evidence for uvular *r* in German long before French influence played a role; see also J. Mendels, id. 52.559 f.

⁵ *zugón* from *tod-yugom*, *to-dyugom* is implausible not only (75) because there were so many other cases besides the nom.-acc., but also because the article is a late development. In early Greek *tod* was a demonstrative pronoun, and the phrase *tod yugóm* was even rarer than *to zugón* was later in Attic.

accounts for *z-*. They were able to back up their first statement with excellent reasons. For the second, Lehmann now proposes to substitute the formula γy (i.e. voiced laryngeal plus *y*) > *z-*. He cites evidence for γ in *zōstós*, and evidence for either γ or $?$ in *zeiat* 'spelt'. Initial ζ [with one possible exception] has its origin in PIE clusters, *dy*, *gy*, *ry*, all of which are groups of voiced obstruent plus *y*' (79). This is an extremely attractive suggestion. It must still be asked what happened to straight initial *y-* without any kind of laryngeal; the answer may well be that it did not occur, except perhaps when followed by another semivowel.⁶ Since /*y w r l m n*/ have syllabic as well as nonsyllabic allophones, their word-initial occurrence would make zero-grade forms with initial *i u r l m n* at least theoretically possible in all cases except where another semivowel follows (/yu..., not /iw... according to Edgerton's rules). Where such forms turn up, say in Skt. *iṣṭá* (full grade *yaj-*) or IE **n-* 'un-' (full grade *ne-*?), many scholars have already come to set up an initial laryngeal before the semivowel—if only on the general principle that initial syllabics did not originally occur—*i* and *n* no more than *e*. (Actually there is often positive evidence of one kind or another for the presence of laryngeals in such cases.⁷)

The rough breathing should not be called an 'aspirate' (74). In Homer *z* makes fully as much position as does *s* before stop. The exceptions are three proper names with an iambic sequence in the first two syllables so that another short syllable is required before the word; in other words Lehmann's case is even better than he makes it (76). That Skt. *yas-* 'ferment' had an initial laryngeal is probable from *yeṣati*, which can only be a reduplicated form, **ya-is-a-ti* (cf. *avocat* < **a-va-uc-a-t*; 78). For *diā-leiō* (78) read something like *djā-tejō*.

Finally, there are the voiceless aspirates (Chapter 11), regarded since de Saussure's day as clusters of voiceless stop plus a voiceless laryngeal, perhaps a specific one. As Lehmann points out, *kh* (unlike *k*, *g*, *gh*) is not subject to palatalization in Indo-Iranian; hence it was still a cluster, the laryngeal keeping the velar stop from contact with *y* in Skt. *khyāti* 'he sees' and a few other cases. While the conclusion is not absolutely cogent, it does simplify the picture far more efficiently than do the author's somewhat parallel conclusions with regard to diphthongs. Together with the earlier discovery of the survival of laryngeals after resonants, it has great force. The clusters did in fact become 'unit phonemes' to all intents and purposes in both Indic and Iranian, as witness their alphabetic treatment. One might regard this latter step as minor. The important fact is that in Indic (more clearly here than in Iranian) those particular clusters survived as a special entity of some kind, while all the other IE dialects let them fall together with one or another of the existing unit phonemes.

The last four chapters (Laryngeals in PIE; The PIE Phonemic System; The

⁶ However, initial *y-* occurring in this position seems to have resulted in Gk. *z-* also, as Sapir and Sturtevant thought. If *Xyug* were set up, the root, with the infix present seen in Skt. *yunākti* etc., would become a monstrosity. See Benveniste, *Origines* 1.159 ff.; A. Martinet in *Word* 9.287.

⁷ Lehmann himself, Sturtevant (*CGHL*², Vol. 1), and others are not consistent in writing laryngeals in forms with so-called initial vowel. A master's thesis by J. A. Reif (University of Pennsylvania, unpublished) makes it probable that semivowels were very largely preceded by an initial laryngeal.

Allophones of the Laryngeals; Development of the PIE Phonemic System) present Lehmann's final view. Once again there is much criticism (e.g. of Brugmann's and Hirt's notations), restatement, and summary. The troublesome doublet Gk. (a)*thánatos* : *thnētós* is blamed by Lehmann (in agreement with others) on accent: when the accent did not fall on the next syllable there was , between resonant and laryngeal; when it did, this , was lost (89). The lengthening effect of a laryngeal on preceding *e a o* and on preceeding *i* and *u* (syllabic allophones of /y/ and /w/) in closed syllables had already taken place in (late) PIE, or *eX* etc. before consonant or pause had already merged with lengthened grade *ē* (though this was apparently not so in the case of *eXy*; see above). Instead of schwa (or of the three 'Greek' schwas), Lehmann reckons with *x*, *h*, *ʔ*, and *γ* (95). The voiced laryngeal *γ* changed 'the timbre of a contiguous *e* only when it coalesced with it', so that the result was *ō*, somewhat unlike the *a*-colored laryngeals *x* and *h* with their effect on contiguous vowels under all conditions. This is a most important difference between Lehmann's and Sturtevant's positions on 'normal-grade *o*'. Vowel *e* occurs only accented, , only unaccented (!).

By way of completing the picture, two familiar cruces, not here connected with the laryngeals, are briefly examined. One is *þ* and its family, set up to account for the correspondence between Gk. Celt. Tokh. Hitt. *t* with a spirant of the other languages (Gk. *árktos*, Skt. *ṛkṣa* 'bear')—a correspondence found only after a velar stop. Perhaps *kþ* does not 'contrast MINIMALLY [emphasis mine] with a succession of velar and dental obstruent phonemes' (100); but this is, after all, small comfort in view of Skt. *uktá*, where the contrast, though not minimal, is unmistakable. If it were not, no more would be needed than to reconstruct *kt* in *ṛkṣa* and posit a conditioned sound change from *t* to sibilant in most IE dialects. As it is, Benveniste's effort to set up an additional guttural series, with its morphophonemic advantages so far as the build of the root is concerned, deserves to be taken seriously.

The other perennial question has to do precisely with the three guttural series. The fact that no historical IE language has more than two does not in itself forbid reconstructing three; otherwise the Semitic sibilants could never have been reconstructed without a knowledge of South Arabic, the only Semitic language to preserve all the original contrasts. Still, there are indications suggesting that the palatals and the 'pure velars' (i.e. both the nonlabialized series) were at one time allophones determined by the segments of sound following them. Lehmann is convinced of this, and he supports his judgment with a convincing and sensitive structural argument (101-2). The fact remains that it is not easy to explain away all the etymologies in which *k'* and *k*, so-called, find themselves in contrasting sets of correspondences. The labiovelars are kept in the system labeled PIE by Lehmann, although others have been equally prone to see in them consonant groups consisting of a velar stop and the nonsyllabic allophone of /w/. The allophones of the four Sturtevantian laryngeals are tentatively narrowed down, by means of a good deal of ingenious reasoning and arguing from the nature of the sound changes conditioned by them, as follows: [x] for /x/, [ɣʷ] for /γ/, [h] for /h/, [b] or [h] for /ʔ/ (108).

This late PIE as arrived at by the comparative method is further open to

internal reconstruction of earlier stages. Morphophonemic relations, morphologically unique relics, and statistical peculiarities (like the rarity of /b/ recently treated by Pedersen, *Dan. Hist.-filol. Medd.* 32.5) are suggestive. Lehmann obtains the following picture (109): 'The changes in the pre-IE non-obstruent system are associated with changes in the accent system. These were two: 1. phonemicization of pitch accent; and earlier: 2. phonemicization of stress accent. Both periods of distinctive accent were preceded and followed by periods of non-distinctive accent. In the PIE phonemic system pitch accent was non-distinctive; it could stand on any syllable of the word and was not correlated with vowel timbre or vowel length. In constructing the earlier vowel systems we must relate the changes of accent and accompanying vowel shifts with other vowel developments, notably those in the neighborhood of laryngeals.' The passage is quoted here to give an idea of the extraordinary difficulty which Lehmann has put in the way of those who wish to follow him in his obviously important, original, and far-reaching conclusions. Any feature characterized as the pitch accent is in the quoted passage, would normally be regarded as phonemic or at least—following a terminology vaguely recalling *Lg.* 25.278 ff., whereby distinctive accents are called phonemic only when they form an elaborate system, prosodic when only the varying location of one feature is concerned—as distinctive; yet we are told to consider this pitch accent nondistinctive. From here on a cloud hangs over the last chapter. Qualitative ablaut (*e/o* etc.) is said to be generally accompanied by differences in pitch accent, and to be the result of a sound change whereby $\acute{e} > e$, but $\grave{e} > o$. When the two levels of pitch fell together, the erstwhile vowel allophone associated with the lesser pitch became phonemic. This took place before the laryngeals were lost—a proposition with which few will quarrel, in view of the tendency (to which Lehmann's book itself is a contribution) to find more and more surviving laryngeals in the historical languages other than Anatolian. Before this sound change the vowel phonemes were /e e:/ with *o*-colored and of course also *a*-colored allophones (induced by certain laryngeals). /e:/ 'developed from /e/ when standing in an open syllable and when the following syllable lost its syllabic status' (111). Lehmann quotes Streitberg's famous study, of which such a shrewd judge as E. Schwyzler (*Gr. Gr.* 1.355) said it was brilliant but surely wrong. In an early stage of IE /e/ occurs in accented syllables, /e:/ in unaccented ones. The phonetic character of the difference between the vowels, as we think of them, makes it probable then that THAT accent was one of stress, and that the difference between /e/ and /e:/ had not always been phonemic either. So Lehmann arrives, as have other students of IE ablaut and accent before him, at the picture of a language with one phonemic vowel, and stress. Some have gone further and reduced these two entities to one. This is easy if one regards the location of *.* as determined by the sequence of consonants in the absence of stress (or, if one prefers, of *e*), in other words if *.* is a nonphonemic outcropping of voice similar to the French schwa as it has been described for some French dialects. Since Lehmann does not openly subscribe to such a view, it is hard to see how he, too, can posit an oldest, pre-stress stage of PIE, with obstruents, resonants ('semi-vowels'), laryngeals, and non-segmental syllabicity (113). What seems to have been neglected (if we have been able at all to do justice to Lehmann's thinking)

would seem to be the fact that throughout all the purported changes in the accentual system, the location of the various peaks, variously described as syllabicity, stress, (higher) pitch—the last two both ‘phonemic’ (or ‘distinctive’) and otherwise?—is constant, inasmuch as it is that which is reconstructible from Greek, Vedic, Verner’s Germanic, and Slavic, and inasmuch as this feature agrees with the ablaut data. Here one suspects Lehmann of being a phonetician where he ought to be as much of a phonemicist as Brugmann was!

Hirt’s symbol \mathfrak{z} is curiously misprinted (86 and passim) as a dotless $\dot{\mathfrak{z}}$. Where does Greek have ‘ou’ for IE u and \bar{u} (86, table)? Is Lehmann alluding to Beotian orthography? Read OCS \mathfrak{z} for \mathfrak{z} (ibid). *gélasma* etc. are not compounds (93). Read *estóresa* for *estorea* (93). Read *molein* for *molein* (94). Read *pibati* for *pibāti* (107; also 84, 118). ‘Over an east-central innovating area the back vowels /a/ and /o/ coincide’ (114): at 101 fn. 2 it is quite rightly pointed out, however, that the ‘Gmc. development ... may be dated after the operation of Grimm’s Law’ and has no direct connection with what went on in Indo-Iranian, Baltic, and Slavic.

Aside from its several handsome contributions to special problems, the present work must be rated as a courageous effort in a difficult field, in which it takes verve and an independent attitude to achieve anything. It would be unjust to say that in such a field any effort at all must be welcome: Lehmann’s is by all odds one of the most serious efforts that have been made. Nevertheless, stimulating as his presentation is in its broad outlines, some of us will be even more grateful to him for his seemingly smaller results, and also for showing us, implicitly, where there is detailed work still to be done—for instance, in connection with the relation between syllabicity, conditioned sound change, and word-boundary; see above on word-initial ‘y-’. In his attempt to give us an overall picture we see Lehmann wrestling with a task that may well be beyond the Indo-Europeanist’s ability at the present time; we see him draw precarious conclusions from scanty and ambiguous evidence, and make claims for the special efficacy of a structural method of reconstruction which, in the nature of things as they have developed in modern linguistics, cannot be so radically different from traditional work as he wants it to be. He will earn our special thanks by telling us in the future more about certain matters which in this volume have suffered from over-condensation or incomplete treatment. We need all the discussion we can get.

Buddhist Hybrid Sanskrit grammar and dictionary. By FRANKLIN EDGERTON. (William Dwight Whitney linguistic series.) Vol. 1: Grammar, pp. xxx, 239; Vol. 2: Dictionary, pp. [ix], 627. New Haven: Yale University Press, 1953.

Buddhist Hybrid Sanskrit reader. Edited with notes by FRANKLIN EDGERTON. (William Dwight Whitney linguistic series.) Pp. ix, 76. New Haven: Yale University Press, 1953.

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To review this *Grammar and dictionary* and the *Reader* of Buddhist Hybrid Sanskrit (BHS, as the author abbreviates it) is a privilege and a pleasure. We may at last congratulate the author on having brought to a successful—or rather,

a brilliant—completion the work of nearly twenty years, and ourselves on having received a tremendous flood of illumination on the texts of Northern Buddhism. This immense literature—containing such important texts as the *Mahāvastu*, the *Saddharmapundarika*, and the *Lalitavistara*, many of them enormously long, most of them of the greatest importance in the history of Buddhism in Tibet, China, and Japan—has always caused difficulty in Sanskrit studies. It was taken to be written in Sanskrit—but what Sanskrit! It was fairly standard doctrine (if usually only implicit in the editing of the texts) that the composers were attempting to write Sanskrit, but knew the language so badly that they could not keep their vernacular out of it (cf. Edgerton, *JAOS* 72.192 [1952] and *Reader* v-vi). Now it is clear (and Edgerton has hastened to acknowledge that Lüders saw it previously; *ibid.*, and *Gramm.* 6 n. 16) that just the reverse is the case. The composers wrote or attempted to write in a Middle Indic (Prakrit) language, but they and/or the copyists knew Sanskrit and its prestige too well and could not keep Sanskrit out of it. There resulted in the manuscripts a progressive Sanskritization of the language of the oldest works in this tradition, and when new works were composed, there often resulted a closer approximation to classical Sanskrit, depending on chronology or on the audience for whom the works were composed. The end result for those texts that were originally in Middle Indic was texts with verses in a relatively Middle Indic form (since Sanskritization of metrical forms, though possible [*Gramm.* §1.42-3], could not go very far, and since, moreover, the verses in some sense possessed a sacrosanct character that in theory did not admit of change), but with the prose Sanskritized to a greater or less degree. In the latter case one type of result was the poems of Aśvaghoṣa, composed apparently for an audience learned in Sanskrit, and, apart from elements of its vocabulary, written in almost pure classical Sanskrit.

Edgerton saw what was necessary if this language of the Northern Buddhist texts was ever to be soundly understood and handled. He states the need explicitly in *Gramm.* §1.59: 'What I have tried to do, then, is to collect and classify the non-Sanskrit forms and words, only, which are contained in BHS. It seems to me that this is what has always been needed; and no attempt has previously been made to supply it.' The task, I know from conversations with him, was greater and longer than he expected when he began it twenty years ago. A lesser scholar would surely have quailed when the magnitude of the undertaking became apparent. So much the more hearty should be our congratulations. The achievement puts in his debt all who are interested in the linguistic history of India, all who are specialists in Buddhism in any language of Asia (so I am assured by my colleague, Ferdinand D. Lessing)—in fact, all Indologists. It ranks with the few really great events in Sanskrit studies in the last century and a half—Müller's edition of the *Rgveda*, the great St. Petersburg lexicon, the completion of Sukthankar's critical edition of the first book of the *Mahābhārata*.

It would be tempting to expatiate on many features and implications of the work, for Edgerton has brought illumination to numerous vexed questions about early Buddhism, its languages, and its texts. But a few remarks must suffice.

The *Dictionary* (to be referred to for short in what follows as *Dict.*) dovetails with the Indian plans for a great thesaurus of Sanskrit, of which it will be one

of the subsidiary works. Edgerton makes it clear (*Gramm.* §1.57) that he has 'excluded ... all words which are used in standard Sanskrit with the same meanings [as in Buddhist texts]', and has included in the *Dict.* only words that are not known in classical Sanskrit texts, and specifically Buddhist meanings of words that are known in classical Sanskrit with other meanings. At times there is parallel evidence in Jain Sanskrit texts. A similar work, both grammar and dictionary, is needed for the Sanskrit of Jain texts. (I am informed that this has been undertaken in India.)

Another point involves the limitations on quotations and text references in the work. Edgerton says (*Gramm.* §1.68) that, though he had read all the texts (a tremendous matter in itself!) and made collectanea from them, 'in the case of the commoner words and forms, only samples could be included in the dictionary and grammar, but I have tried to make them typical and abundant enough to give an approximate notion of what the totality would show.' Elsewhere (*Gramm.* xxi) he says: 'If I could take the time to repeat the whole process, that is to restudy carefully and minutely all the texts included in my plan, I am certain that these volumes could be enlarged and brought nearer to completion.' This would indeed have been welcomed by those who must read the difficult texts involved, but it would surely have taken another twenty years and we must be content with the very generous results that we have. The *Dict.* could not have been a concordance to the texts. However, it is unfortunate that the plan of publishing the *Reader* was adopted only after *Grammar* and *Dict.* were finished. Some references had already been included in these latter to the text excerpts included in the *Reader*, but it would have been very useful if complete, or practically complete, references to everything in the *Reader* had been given and if all major difficulties in the *Reader* excerpts had been discussed somewhere, either in *Grammar* and *Dict.* or in a fuller commentary than is given in the notes in the *Reader*; not all of us can read the material with the facility that Edgerton must have acquired.

Perhaps the name given by Edgerton to the language should be mentioned. He has consistently called it Buddhist Hybrid Sanskrit, a cumbersome name, which for convenience he abbreviates to BHS. I have no better name to suggest. 'Buddhist Prakrit' is shorter, but ambiguous and unusable since there is already a Buddhist Prakrit, that of the Khotan Dharmapada, even if this might better be called 'NW Prakrit'. 'Buddhist Middle Indic' is certainly no less cumbersome and at the same time omits the specification of Sanskritization. Perhaps after all Edgerton's name is the best possible.

Short of a complete reexamination of all the BHS texts, including those that have been published since the completion of this work and that Edgerton was unable to use at all fully,¹ a reviewer is unable to do much more than make random notes, and so thorough has Edgerton's work been, that little can be

¹ Waldschmidt's publication of the Mahāparinirvāṇasūtra fragments was used to a very small extent and an extract from it appears in the *Reader*; it is reviewed by Edgerton, *JAOS* 72.190-3 (1952). A translation of the Rāṣṭrapālapariṣecchā was reviewed by Edgerton, *JAOS* 73.169-70 (1953).

found that escaped him. I shall first discuss at length two major points, and then add my random notes.

(1) One of the problems that has engaged Edgerton from the beginning of his work on BHS has been that of the Prakrit underlying BHS. He grappled with it as early as 1936 in *BSOS* 8.501-16, and came to the conclusion there (516) that the 'Prakrit, on which Buddhist hybrid Sanskrit was based, was a dialect closely related to both Ardhamāgadhi and Apabhraṃśa, but not identical with either.' Lüders had earlier, in 1916, suggested Māgadhi. Hiān-lin Dschi argued for Old Ardhamāgadhi, with one feature at least due to relatively late influence of a Northwestern Prakrit. In *Gramm.* §§1.78-105, Edgerton examines the matter again, both his own early view and those of Lüders and Dschi (the latter also in §§1.24-9), and comes to the agnostic conclusion (1.105) that (1) BHS has some features that are either unique or virtually so; (2) it has some features in common with Pali, but on the whole its morphology is unlike Pali; (3 and 4) it has a few features in common with Apabhraṃśa, but there are striking differences and its general character is more in line with Prakrit as a whole ('common Prakrit' or 'common Middle Indic'); it is consequently older than Apabhraṃśa; (5) 'the Prakrit underlying BHS was certainly not identical with any Middle Indic dialect otherwise known to us.'

It is very easy to agree with these propositions, so clearly are they established by Edgerton. It is possible, however, to point out one very specific feature that BHS shares exclusively with NW Prakrit, and to add NW Prakrit as one of the dialects that shares one other feature. Interpretation is reserved until after the details have been presented.

In *Gramm.* §2.67, Edgerton notes 'the frequent occurrence of *t* for anusvāra (or BHS *n*) before *s*'. Senart had noted it earlier in the introduction to his edition of the Mahāvastu (1882-97), but had not accepted it in his edited text, suggesting that it might be merely a graphic error. Edgerton, however, finds it not merely in the Mahāvastu in those hundreds of places where Senart meticulously recorded it in his critical notes, but also sporadically in other BHS texts (e.g. *Lalitavistara* and *Suvarṇabhāṣottamasūtra*). Exx.: *mātsa* for Skt. *māṃsa* 'meat' (Mv. iii.269.8); *viḥatsyase* for Skt. *viḥaṃsyase* (LV 335.1); the very common Mv. optatives (used also as aorists) with endings *-etsuḥ*, *-etsu* (§§29.18, 19, 32.95-99, 103, 104; superseding *JAOS* 57.25 ff.), e.g. *bhavetsuḥ*, *akaretsuḥ* (AMg. and BHS [rarely] have forms in *-esu*, *-emsu*); and even a few aorists in *-itsu(h)* (§32.28; cf. the optative in §29.19) and *-ātsuḥ* and *-atsu* (§32.75-76). Edgerton does not note parallels to this BHS phonological trait elsewhere in Middle Indic.

It occurs, in fact, in the Prakrit of the Khotan Dharmapada (MS Dutreuil de Rhins), of the Niya (Shan-shan) documents, and (in one probable example) of an inscription at Peshawar. All these documents belong to a NW Prakrit of the first few centuries A.D., apparently the administrative and literary language of the Kushan empire and Central Asiatic offshoots thereof. The decipherment of a difficult Kharoṣṭhī character as having a value *ts* or *tś* in the Dharmapada and the Peshawar inscription seems (so far as I have discovered) to be due to Sten Konow as early as 1929 (*Kharoṣṭhī inscriptions* [*Corpus inscriptionum indicarum*,

Vol. III, Part I], p. cv); he also published the matter at greater length in 1931 in *BSOS* 6.405-9. T. Burrow, in dealing with The dialectical position of the Niya Prakrit, discussed the matter for all the documents involved in *BSOS* 8.419-35 (esp. 423, 427) in 1936, and again in 1937 in *The language of the Kharoṣṭhi documents from Chinese Turkestan* 18 f. The editio princeps of the Prakrit Dharmapada (Senart, *JA* 9.12.193-308 [1898], and another section by Oldenburg in St. Petersburg in 1897) did not recognize this phonetic value, nor did Barua and Mitra's 1921 edition based on Senart. The most recent transcription of the text by H. W. Bailey in 1945 (*BSOAS* 11.488-512) does, however, recognize it and transcribes it as *tṣ*. Exx.: *ṣatsara-* (Dh. A⁶) and *saṃṭsara-* (Pesh. inscr.) = Skt. *saṃsāra-*, *ahīṣa-* (Dh. A⁸) = Skt. *ahimsā-*, *ṣatsana-* (Dh. A⁹) = Skt. *saṃsanna-*, *bhametṣu* (Dh. B34) = BHS (Mv.) *bhavetsuḥ*, *maṃṭsa-* (Niya) = Skt. *māṃsa-*. The phonological development assumed is *ms* > *nts*. In the Dharmapada dialect anusvāra then completely disappeared, either in pronunciation or only graphically; Burrow seems to posit the latter: 'anusvāra is consistently not written in this ms.' Clearly in the BHS texts the anusvāra is gone phonologically and not merely graphically.

This trait is common only to BHS and NW Prakrit. Pischel does not record it, and Mehendale (*Historical grammar of inscripational Prakrits* 314 [Poona, 1948]) records it only for the Peshawar inscription already mentioned.

A thorough tabulation and examination of all forms in the Prakrit Dharmapada is still a desideratum in spite of the frequent treatments of this important document (so also Edgerton's judgment, *BSOS* 8.510 n. 1). Most statements about its language seem to have been based on random inspection. It is probably for this reason that Edgerton has failed to note in §§1.95-6, 2.72, 8.3, 22, 31 ff., that BHS *-a* as the ending of the nom. acc. sg. of *a*-stems is paralleled, not only in Ap., AMg., and Mg., but also in the Prakrit Dharmapada and in the Niya materials (for the latter, see Burrow, *The language of the Kharoṣṭhi documents* 22). The desiderated close study of the language of the Dharmapada fragments might show that only the acc. sg. masc. and the nom. acc. sg. neuter commonly have the ending *-a*. Alleged examples of *-a* in the nom. sg. masc. are in any case few; I have not made a complete examination but note the following. *aśoka* in A¹⁶ is probably a clear instance. In A¹⁵, however, *pagasana* (Bailey *pagasana*) is not clear. The verse in Bailey's transcription is:

apramada-rada bhodha ṣadhami supavedide
druḍa udhvaradha atvaṇa paga-ṣana va kuñiaru

'Be (pl.) devoted to heedfulness in the well-propounded good law! Pull yourselves up from the evil way, as an elephant that is sunk in the mire!' The final syllable *ru* is restored. This follows closely the Pali Dhammapada reading *paṅke sanno va kuñjaro*. But why should not the final syllable of *kuñiaru* be restored rather as *kuñāra*? *paga-ṣana va kuñāra* would then have normal nom. pl. masc. forms ('like elephants sunk in the mire'), by attraction to the plural verb form *udhvaradha*. Short of the complete grammatical study of the fragments, it will be impossible to be quite sure how to take this verse and whether the nom. sg. masc. in *-a* of

a-stems is to be fully accepted for this Prakrit dialect. However, *-a* as the ending of the acc. sg. masc. and the nom. acc. sg. neuter is well attested.

After Edgerton's earlier treatment of this type of case forms in various Prakrits, either in texts or as described by the grammarians (*BSOS* 8.510, *HJAS* 1.66 ff., *JAOS* 59.369-71), it is impossible to treat the coincidence between such BHS forms and those of the Prakrit Dharmapada and the Niya documents as unequivocal evidence that BHS is closely connected with the NW Prakrit. Yet, the exclusively shared phonological feature already pointed out makes it necessary to give some weight to this morphological feature also. When the desiderated linguistic study of the Prakrit Dharmapada has been undertaken, it may be that the one clear isogloss and the other more dubious item will be backed up by one or two more features that will make even closer the relationship between BHS and the NW Prakrit, even if identity can never be established because of the many differences between the two. Edgerton, in *BSOS* 8.510 n. 1, said that to posit identity would be 'premature, to say the least'; the search for isoglosses, however, should still be made.

(2) The BHS texts presented to Edgerton a critical problem of the utmost difficulty, as they had to their editors. We may agree with him that, in default of the thoroughgoing grammatical description that he himself undertook for the first time, no editor had possessed any standards by which he could judge what he found in the manuscripts. And we may regret with him that the progressive Sanskritization that all copyists and late redactors had introduced into the Middle Indic of the texts, had wrongly been carried further by the modern editors.

The critical principle that it is imperative to follow is that first enunciated by Lüders (cf. *Gramm.* §1.40) and adopted by Edgerton (*ibid.* and *Reader* v): 'any non-Sanskritic form presented in the mss. must, in general, be regarded as closer to the original form of the text than a "correct" Sanskrit variant'; this is of course a special case of the principle of the *lectio difficilior*. The principle is to be followed whenever a text occurs in more than one ms. or a passage in more than one version. It will not, of course, lead an editor back to the *verba ipsissima* of the composer, except in favorable places, since it is only the verses that show a high proportion of Middle Indicisms in some only of the texts, and even the verses have often had Sanskritisms introduced that now appear in all the mss. available. As for the prose, it is only the prose of the Mahāvastu (and two other small fragments) that is not rather thoroughly Sanskritized, and even the Mahāvastu prose has undergone some Sanskritizations that appear in all mss. The principle, however, is surely to be applied wherever possible, even in the realization that the resulting text is likely to be far from the original; it will at any rate be closer to the original than the text of the editions.

A difficulty presents itself, however, that hardly admits of solution. Gross copyists' errors are frequently demonstrable, e.g. in verses where copyists' Sanskritizations of unfamiliar, Middle Indic forms vitiated the meter, and in other places where more ordinary negligence produced a *vox nihili* or a word that is demonstrably incorrect in the context. An example of the latter kind will suffice: *Reader* 40 gives Udānavarga xvi.23, in which the only ms. for the passage

reads *deṣaś*, a clear mistake for Middle Indic *doṣaś* = Skt. *dveṣaś* (in a context in which it is coupled with *rāga*); the mistake is patent, and application of the critical principle enunciated leads to the emendation *doṣaś* rather than *dveṣaś* (there are numerous parallel passages with the Middle Indic form). But often enough in this 'peculiar' linguistic realm, a 'mistake' that a Sanskritist would class as such according to Pāṇini's rules for correct Sanskrit, may well be perfectly sound, either because it is explainable as Middle Indic and paralleled elsewhere in the BHS corpus, or because, since BHS is not the same as any other Middle Indic dialect and had features that were sui generis, it is the usual form in BHS. In other words, when all is 'peculiar', how can one identify a copyist's error? Unless subjectivism is to run riot in deciding what non-Sanskrit forms are to be accepted and what rejected, it would seem wisest to accept anything as correct, if it cannot be demonstrated to be wrong, and to list and classify everything in the proper place in the *Grammar*. This Edgerton has in general done.

There are, however, some passages in the *Reader* where strict application of his critical principles in all their implications would seem to rule out emendations that he or earlier scholars have made. That sheer weariness in dealing with an enormous mass of material has led to this result seems to be shown by occasional lack of consistency between *Grammar* and *Dict.* on the one side and the *Reader* on the other. I list examples taken from section 12 of the *Reader*, stanzas from the Udānavarga (Ud.) which is known in its BHS form from several sets of fragments of considerable age, found in Central Asia by the Mission Pelliot and edited by N. P. Chakravarti.

iv.1. The best ms. reads *pramāda*, emended by Edgerton to *pramādo*. The ms. form is a good nom. sg. masc. according to §8.22. The reason for emendation is probably to make the verse conform to the rules of classical meter; but note that the second pāda of the anuṣṭubh śloka has a short third syllable also in Ud. iv.18, xi.5, xi.7, xvi.15, xvii.5, 6, xvii.9, and similarly in the fourth pāda in xi.1, xx.2. When this syllable ends in *m* and is a word-final syllable preceding a vowel at the beginning of the next word, it might be objected that it is really long, as ending in an anusvāra (*m̐*) which has been wrongly written as *m*; but this objection will not hold in iv.1 itself, nor in xi.7, xvi.15, xvii.9, xx.2. The fact is that we do not know the norms of the śloka in the Ud. and other BHS texts, and should not emend to establish classical or epic norms. The same remarks apply to the emendation of *kuṭhāri* to *kuṭhārī* in viii.2 following the variant reading of a less well-considered ms.; the *Reader* says that the reading of the best ms. is metrically bad, but the *Dict.* reads *kuṭhāri* and says 'metr. indifferent.'

vii.5. The best ms. reads *bahūm*, another the correct Sanskrit *bahu*; Edgerton emends to *bahum*. Why is it decided that the reading of the best ms. is a mistake of the copyist? In §12.30 end, Edgerton records the form with the remark that if genuine it is isolated, and is probably an error for *bahum* (an otherwise attested form). My own inclination would be to keep the ms. form, perhaps a contamination of *bahum* and *bahū* (§12.28).

vii.12. *etes* is emended to *etais*. The ms. reading might be kept as an example of *e* for *ai* (§3.67), unparalleled, to be sure, in the instrumental pl. of *a*-stems; all the other instances recorded in §3.67 seem just as 'peculiar', but all would be, of

course, good examples of the Middle Indic treatment of Skt. long diphthongs. The Pali *ete* can hardly be read here unless the pl. of the Pali *tayo kammapathe* is substituted also in the following words of Ud. (*trayaṃ karmapatham*).

x.15. Since *ṣṭirbhūt°* is the reading of the best ms. both here and in xxi.4, one might have expected retention of the forms rather than emendation to *ṣṭābhūt°* in both places. The very general confusion of nom. sg. forms in *-i*, *-ir/s*, *-ī*, and even *-īr/s*, in stems in *i*, *ī*, *in*, might well have lead to *-ir-* instead of *-ī-* in the compounds of *a*-stem adjectives and nouns with verbs. In the *Dict.* the reading of xxi.4 only is recorded; there seems to be no record in the *Gramm.* The statement in the *Dict.* (s.v. *ṣṭābhūta*) that the adjective has been noted only as an epithet of Buddha, needs modification since in x.15 the word applies primarily to a lake (*hrada-*) and perhaps secondarily to the person who is *dhīra-* and *prājña-*.

xi.1. The reading *naprahāya*, i.e. *n' aprahāya*, should certainly have been kept according to the critical principle stated and §4.21, rather than the Sanskritic *nāprahāya* of the less important ms.

xi.3, 12. Two instances of *brāhmacārya-* as read by the best ms. have been emended out, as well as *brāhmacārya-* in xvii.3. In this ms. fragment there are no counter-examples of *brahmac°*. Should not the *brāhmac°* forms be kept, as hyper-Sanskritisms, or as formed on the analogy of *brāhmaṇa-*? In §3.11, *pāṭirāja-* (Skt. *prati-*) is accepted, though, as in *brāhmac°*, *ā* is metrically indifferent.

xi.10. *otata*. Whatever the interpretation of *sālam*, neuter gender for masc., or nom. sg. masc. in *-am* (§8.26), *otata* agreeing with it must be nom. sg. and could be classed in §8.22 or §§8.31 ff. Thus, rather than saying: 'prob. read *°tam*.'

xv.8. *jāgāryam* is kept in the *Reader*, but the *Dict.* thinks of emending to *jāgāryām*, unnecessarily alleging a misprint or error of tradition.

xvii.3. The best ms. reads *°kraujam*, Chakravarti emends to *°kraumjam* (in Edgerton's note "*°kraumjam*" is omitted after 'possibly' by a misprint), Edgerton emends to *°krauñcā*. Chakravarti's form is very possible (§2.28), denasalization as in the ms. is rare (§2.73), but Edgerton's emendation seems to go farther than necessary.

xix.3. *tasya* is emended to *tasyā*, since a long syllable is required and the following word *sprhayanti*, according to the regular tendencies of the dialect (§2.2), should be evaluated metrically as if it began with a single consonant. But §2.4 seems to speak against neglecting what is actually written, and in fact the *Dict.*, s.v. *tāḍṛ(n)*, quotes this passage with *tasya*. If this line of argument is accepted, then in xviii.4 also the emendation of *vatsa* (Middle Indic morphology) to *vatso* (Sanskrit, for metrical reasons) before *kṣīrapako* is unwise.

xx.1. Ms. *saṅgaḥ* is emended to *saṅgāḥ*. But similar emendations are disapproved in §8.83.

In a few other passages that I have noted there seems to be failure or hesitancy to follow the critical principle of keeping a Middle Indic form of the mss.

Reader 16, vs. 14. The best ms. (or mss.?) reads *piṇḍacaryā*, the others *°ryām*. Certainly the acc. in *°ā* must be kept. The suggestion in the note, that *°ām* be read, must arise from the temptation to interpret *°ā* as due to loss of anusvāra

by a copyist's error; the principle of the *lectio difficilior* would seem to be very clearly applicable here, unless one is to become entirely subjective in deciding what is and what is not to be regarded as Middle Indic in the BHS texts.

Reader 17, line 1 of text. The reading of the mss. *Ājñātakaunḍinyā* has been emended to °*nyo*, with a suggestion in the note that it might be kept. There is a respectable body of parallels for -ā in the nom. sg. of *a*-stems (§8.24).

Reader 17, 5th line from bottom of text, *triciṅvarā* is read instead of the reading of the mss. *triciṅvare*. Footnote 13 says that 'elsewhere this cliché regularly reads °rā.' However, *triciṅvare* is, according to §8.80, a rare Middle Indic form for nom. pl. masc; there would seem to be sufficient reason for keeping it and regarding *triciṅvarā* as in some sense a secondary Sanskritization of a *lectio difficilior*.

(3) My random notes contain a few other comments on the *Udānavarga*, including some on inconsistencies between the volumes.

viii.1. For *abhūtavādīr*, nom. sg. masc. of stem °*vādin-*, the *Reader* gives a reference to *Gramm.* §10.32. There similar forms are given only from *i*-stems. It is said that 'the only certain cases seem to have -īr for -ir m.c., before initial vowel.' The present instance is metrically indifferent before *n-*.

x.5. *ārṇavam*. The *Dict.* entry takes it to be an adjective with the noun *saraha* in one passage quoted and also in Ud. xvii.7 (where only *s-* is left of the word). In x.5, the word *ārṇavam* is clearly a noun, as also probably in xvii.7 in the less important ms.

xi.3. The clear instances of neuter sg. nom. forms in -ā and -ām might have been entered in *Gramm.* §8.38.

xi.10. *dviṣa* nom. sg. *Gramm.* §§18.78, 79 class it as < Skt. *dviṣan*; *Dict.* lists *dviṣa-* as *a*-extension of Skt. *dviṣ*, in explaining *dviṣo* of MSV ii.17.1.

xvii.9. *āpaś ca sarvato bhavet*. A clear case of stem *āpa-*, to be added to §15.13; correct *Dict.* statement: 'clear case noted only in cpd. *āpa-skandha*.'

xix.1. Read *kaśābhisprṣṭo*, i.e. *kaśā-abhisprṣṭa-*.

Elision of *a-* according to the regular Sanskrit sandhi is not indicated by apostrophe in Ud. xi.10 (*yo sāv*), xxi.4 (*eko smi*, °*bhūto smi*); these are only misprints, since in other verses in the *Reader* the apostrophe is regularly used (another such misprint on p. 14, vs. 4 *sarvajagato sya*; cf. vs. 3).

Notes on *Gramm.*

§1.38. 'In some respects, at least, this Sanskritized spelling [of BHS verse material] is mere window-dressing, and misrepresents the actual pronunciation, which was Middle Indic.' Edgerton's evidence is derived from the metrical treatment of Sanskrit initial consonant clusters as if they were their Middle Indic single-consonant equivalents. E. H. Johnston had already in his 1936 introduction to *The Buddhacarita* (xc-xci) suggested Middle Indic tendencies in the pronunciation even of such comparatively impeccable Sanskrit as that of Aśvaghōṣa's two poems. His evidence was derived not from meter, but from punning passages in which some groups of non-identical Sanskrit words were treated as if they were identical, as they were in fact in Middle Indic (e.g. Saundarananda ii.45 treats *śakya*, *śākya*, and *śakra* as if they were all pronounced *sakka*). Johnston's suggestion should certainly be reexamined in the light of Edgerton's demonstration.

§7.33. A useful reference is John Brough, 'Thus have I heard ...', *BSOAS* 13.416-26 (1950).

§10.15. The nom. sg. in -i of masc. i-stems provides, of course, an automatic solution of the difficulty in the form *maṇi* in the famous formula *oṃ maṇi padme huṃ*; any other attempt at a solution is unnecessary.

Notes on *Dict.*

abhi-ni-√śvas = *ni-√śvas* is noted in pw., but only from *Jātakamālā* (e.g. 83.14, 96.14); it might have been recorded.

Ājñāta-kaunḍinya. Is *Ājñāta*- not = *Ājñātṛ*? This might have been given here and in *Gramm.* §13.4.

upanāmayati. This verb is recorded by our lexica as occurring only once in non-Buddhist Sanskrit. Another occurrence in Edgerton's sense 5 is found in an anthology verse by a Buddhist (*bhadanta-Ravigupta*), recorded in Vallabhadeva's *Subhāṣitāvali*, vs. 155 (Bombay Sanskrit Series, vol. 31; I owe my knowledge of the verse to John Brough, *Selections from Classical Sanskrit literature* 70): *upanāmayati karo 'nnaṃ rasāṃs tu jihvāi 'va jānāti* 'the hand presents food, but only the tongue knows the flavors'.

ṛlla and *ṛllaka* 'prize-fighter (?)'. These were read by Burnouf and Senart, the latter in many passages in *Mahāvastu*, with the suspicion that there was involved a graphic error for *jhalla(ka)* 'fighter with a club'. Edgerton thinks the ms. reading 'too common to emend'. It is to be noted that in every instance recorded by Edgerton the forms with *ṛ* follow a vowel, i.e. do not show the usual Sanskrit sandhi. The akṣara *jha* is, as a matter of fact, extremely rare in Sanskrit and there are three different forms given for it in our grammars. What form it had in Senart's mss. is unknown to me, since I have no access to Johannes Klatt, *De trecentis Cāṇakyaē poetae indici sententiis*, in which there is discussion of the particular form of alphabet involved. Is it possible that in this alphabet (or perhaps in some earlier alphabet in which the *Mahāvastu* was handed down) *ṛ* and *jha* had forms so similar that from *jhalla* a vox nihili *ṛlla* was derived by mistaken graphic interpretation?

pravisarpin. Better 'gliding forth in all directions'. Only recorded for *Jātakamālā* 96.20 f., which I cannot resist quoting: *viṣṇubhamāṇā iwa pravisarpibhiḥ śikharabhujaiḥ pariṣvajamāṇā iwa cā 'nyonyam* '(clouds) seeming to be expanding with summit-snakecoils (*śikharabhujaiḥ*) that were gliding forth in all directions, seeming to be embracing each other with their peaks like arms (*śikharabhujaiḥ*)'. This is given as an instance of *kākākṣigolakanyāya* 'the analogy of the crow's eyeball', the crow being supposed to have but one eye which moves from one side of the head to the other, as the instrumental *śikharabhujaiḥ* in this double simile belongs both to the preceding and the following simile.

ploti and *pilotika*. No really cogent passage has been found to demonstrate the meaning 'cord, bond'. Edgerton's interpretation is as much a guess as is A. Foucher's 'voile' in his interpretation of the *Mahāvastu* passage given in the *Reader* 31 line 2 (*La vie du Bouddha* 226). I can only add (thanks to my pupil Alex Wayman) that the confusion noted by Edgerton in Tibetan between *rgyun* 'stream, continuity' and *rgyu* 'cause' (reproduced in the Chinese 'having cut off causes' for *chinna-plotika*) is thrice confounded by the entrance into the matter

of Tibetan *rgyud* 'string, cord' (as e.g. in representing Sanskrit *saṃtāna* 'continuity-series'). This confusion is evidently involved here too, but gives no certain help in solving the meaning of *ploti*.

bhadanta, Pali *bhad(d)anta*, *bhad(d)ante*. The theory of PTSD and Lévi, that these are < Skt. *bhadraṃ te* (= *pax tecum*), is mentioned, but not the corresponding plural forms Pali *bhaddaṃ vo*, Prakrit Dharmapada (A²⁷) *bhadraṇu*.

yāvat, *yāva*. Under (2), *yāvaṃ* might well have been recorded; cf. *Reader* 35 line 11, *pūrvavad yāvaṃ*.

rathakāra. If I understand the editor of the Udānavarga correctly, this text at ii.11ab reads in a combination of fragments: *rathakāra iṃ carmaṇaḥ parikartum upānaḥam*. This is parallel (I follow Chakravarti) to the Pali Jātaka IV.172: *rathakāro va cammassa parikantaṃ upāhanaṃ*, and the Prakrit Dharmapada (C^o 40): *radhe'aro va camaṣa parikica wahaṇa*; cf. also Jātaka VI.51: *kadāhaṃ rathakāro va parikantaṃ upāhanaṃ*. All intend: 'like a *rathakāra* cutting out a shoe'. It seems hardly possible to avoid interpreting *rathakāra* in these passages as meaning 'shoemaker' or 'leatherworker', and so in fact it has usually been interpreted. The word is not in *Dict.*, nor are the Pali occurrences treated in the *Pali Text Society's Pali-English dictionary*.

lagna (1). The compound *lagnabhayam* 'fear of being stuck' is probably to be considered as of the type seen in Pali *āgatakāle* 'at the time of (his) having come', *ṭhitatṭhāne* 'at the place where he stands'. Cf. the treatment in Hans Hendriksen, *Syntax of the infinite verb-forms of Pāli* 152-4 (1944), where it is said that the peculiar feature of the type is that the perfect passive participle which occurs as first member of the compound is 'not an attribute to the substantive' which is the second member. It is, of course, an attribute, but differs from the usual attributes in 'descriptive' compounds in that the noun which is the second member of the compound is not the subject of the participle in the underlying syntactic construction (i.e. this underlying construction is not 'the time has come', 'the place stands', 'the fear is stuck'); the subject of the participle in the underlying construction ('he has come', 'he stands', 'he himself has stuck') is represented, when it is represented, by a dependent genitive form. A Dravidianist is irresistibly reminded, in spite of differences of detail, of the very common Dravidian syntactic construction in which the verb that ends a predication is given an adjectival form and the whole predication is then used as attribute to a following noun; e.g. Kota (*Kota texts* 10.3) *tičn kaḍ aṭa-d ka-lm* 'the period (*ka-lm*) when people had not discovered (*kaḍ aṭa-d*; adjective form of negative paradigm) fire (*tičn*)'; (ib. 9.141) *vitvd eṛtn* 'the place (*eṛtn*) which they were to sow (*vitvd*; adjective form of future paradigm)'; (ib. 9.142) *a-m unčvd unyp* 'the thought (*unyp*) which we (*a-m*; subject of predication) have thought (*unčvd*; adjective form of past paradigm)'. In the older Indo-Aryan languages it is only Pali compounds of the kind mentioned at the beginning of this note and this lone BHS compound (lone, at least, to my knowledge) that seem Dravidian-like. Of the modern Indo-Aryan languages Marathi is described (Jules Bloch, *La formation de la langue marathe* 260 f., §263) as having a much more Dravidian-like construction of this kind, in that it has the subject of the verbal form in the nominative; the verbal form, however, is used apparently only with postpositions, e.g. *tujhī āi vārlyā-pāsūn* 'since (*pāsūn*) your (*tujhī*) mother (*āi*; subject)

died (*vārlyā*). Bloch (*L'Indo-aryen du Veda aux temps modernes* 328) mentions in passing and without details that Marathi, Oriya, and Sinhalese have independently and with differences of detail acquired such constructions. Presumably the Middle Indic compounds and the three modern types of construction are all based on the pan-Dravidian construction.

vyañjana. The equivalent *vyañjanaka* (Reader 29, vs. 9) has not been recorded.

satya-vacana. The reference to Burlingame's article on the 'act of truth' might have been supplemented by a reference to W. Norman Brown, The basis for the Hindu act of truth, *Review of religion* 5.36-45 (1940-1). *satyādhiṣṭhāna* occurs in the *Jātakamālā* (I have noted 12.19, 96.13).

samūhati, *ūhata*. Edgerton seems to favor connection with Sanskrit *sam-ud-√hr*. But the Pali 3sg. imperative *samūhantu* (see Andersen's *Reader*) looks rather to *√han*.

Notes on *Reader*.

14, line 5 after vs. 5: *prasvasantaṃ* has not been mentioned in *Dict.* (s.v. *praś-vasati*) or in *Gramm.* §2.63.

17, line 13 of text: *samācaratha* (which is Senart's reading) is either a misprint for *samudācaratha* (*Dict.* s.v. *samudācarati*), or, if to be accepted with the meaning of *samudācaratha*, should have been listed in *Dict.*

A few misprints have been noted. *Dict.* s.v. *viśreṇī-kṛtvā*: in the translation of Ud. xi.12, the word *carati* is omitted. 'Saundarānanda' is read in various places (e.g. *Gramm.* xxix; *Dict.* s.vv. *āśu*, *āśrava*, *jana*), but not universally (e.g. *Dict.* s.v. 2 *nāma*), for 'Saundarananda'. Winternitz seems consistently to have misspelled this title of Aśvaghōṣa's poem.

A word must be said about the excellence of the manufacture and the relatively low price of the books.² The stately volumes of *Grammar* and *Dictionary* were printed in Denmark; are contracts with good European printing houses the only solution to the scholar's problem of combining expert printing in difficult subject matters with relatively low printing costs? The only criticism that could be made is that both *Grammar* and *Dictionary* are printed in 8-point type (with some passages in 6-point in the *Grammar*). Small type for dictionaries is not unusual, but a grammar in small type is hard on the eyes and constant use does not improve the matter. The saving in the costs, however, must have been considerable and must be taken as justification for the inconvenience.

In closing this review, I must refer again to the superlatively great achievement of the author. The volumes will join his long series of distinguished works on every Indologist's shelves and work desk.

A grammar of Manichean Sogdian. By ILYA GERSHEVITCH. (Publications of the Philological Society, Vol. 16.) Pp. xiv, 308. Oxford: Basil Blackwell, 1954.

Reviewed by P. TEDESCO, *Yale University*

Gershevitch studied Sogdian at the University of London under W. B. Henning; the present book was his doctor's dissertation. It was finished as early as 1943, but publication was delayed until 1954.

² \$15 for *Gramm.* and *Dict.*, \$2.50 for *Reader*.

Manichean Sogdian, represented only in the collections of Berlin, is the type of Sogdian which became known first: it was the dialect of the brief samples of Sogdian published by F. W. K. Müller, along with the Middle Persian and North Iranian Manichaica, in his *Handschriftenreste ii* (*Abh. Preuss. Ak. Wiss.* 1904), on the basis of which Andreas defined the new language as Sogdian. But more than twenty years passed before further material in this language was laid before the public: a few short pieces in Waldschmidt-Lentz's *Die Stellung Jesu im Manichäismus* and *Manichäische Dogmatik* (*APAW* 1926 and *SPAW* 1933). That is why in the older grammatical treatments of Sogdian, by (in chronological order) Gauthiot, me, and Benveniste, Manichean Sogdian is practically not considered.

Larger texts were first offered in Henning's *Manichäisches Bet- und Beichtbuch* (*APAW* 1936); in a rich commentary, he also dealt with many problems of the language. In the following years Henning published numerous Manichean Sogdian texts in quick succession, mostly in *BSOAS* (e.g. Vols. 11, 12, and 13) and *JRAS* (e.g. 1944 and 1945)—publications which, except for *The Book of the Giants* (*BSOAS* 11, 1943), all appeared after the conclusion of Gershevitch's book; a great part of the material (about half, as Gershevitch informs me) is still unpublished.

The language of these Manichean Sogdian texts in Manichean script, most of which he had to study in photographs, is the special object of Gershevitch's book; and except for Henning's commentary to the *Bet- und Beichtbuch*, this is all primary work. But Gershevitch also considers the other forms of Sogdian—Buddhist, Christian, and Secular, so that his book has become a general grammar of Sogdian, in fact the most comprehensive and up-to-date grammar that we have. Gershevitch has a thorough knowledge of the subject and shows good judgment. Moreover, the authority of the book is greatly increased by the fact that it was written in close consultation with Henning, who also contributed some ideas which he has not yet published elsewhere.

The phonology is historical and comparative throughout; in the morphology, the historical explanation, without striving for completeness, goes as far as our present understanding allows; of the syntax, only some characteristic features are noted.

After so many years and such great progress in Sogdian studies, it is only natural that Gershevitch should sometimes differ from my explanations of 1923 and 1925, in *Zeitschr. f. Ind. u. Iran.* 2 and 4, even on major points; but I am not sure whether he is always correct. While I had admitted (*ZII* 4.102) both monosyllabic and dissyllabic inflected stems, respectively of one and of two short syllables, Gershevitch (§484) recognizes only monosyllabic ones. Forms like *sywnw* acc. 'word', *myδβy* nom. (pl. *myδβt'*) 'minister', and *žwxšqy* nom. (pl. *žwxšqt'*) 'disciple', which I had read **saxwanu*, **marδeβi* (**marδeβta*), and **žuxšaki* (**žuxšakta*), he interprets (§§222, 509, 520), with syncope, as **sxunu*, **marδeβi* (**marδeβta*), and **žuxški* (**žuxškta*). Now syncope has certainly played a great role in Sogdian, especially in the later phase of the language, but I do not think that these forms had the value assumed by Gershevitch at the time of the operation of the rhythmic law, i.e. probably around the beginning of the Chris-

tian era. Nor do I believe, at least for that time, in readings like *ptsayd-* 'arranged', *ptyušt-* 'heard' (§509 and §517), etc. It is true that Gershevitch (§§528-9) quotes several dissyllabics of two short syllables which have no inflection, like *βxtm* 'godliest', but these may either be later forms, of the stage when short finals had been lost in the light stems also, or special factors may have been at work here. But the general exclusion of dissyllabic inflected stems seems to me to force the facts. It is perhaps noteworthy that Old English, which in its treatment of Proto-English final *i* and *u* offers an exact parallel to the Sogdian rhythmic law (ZII 4.162; e.g. neut. pl. *hofu* 'dwellings' : *word* 'words'), also has dissyllabic inflected stems, but strangely enough these are not the short-shorts but the long-shorts: we have *reced* 'buildings', but *hēafodu* 'heads'.

Gershevitch rejects my explanation of the endings of the *aka*-stems Buddh. *-y* (alternating with *-k*, *-k*, and *-kw*) and *-w*, Man. *-yy* (and *-yh*), and Chr. *-ē*, in the type Buddh. *zt'y* (beside *z'tk* and *z't'kw*), Man. *z'tyy*, and Chr. *zātē* 'son'. I had read *-y* as *-ē* and had derived it from the nom. and gen. endings **-aki* and **-akē*, by loss of the *k* before the dropping of the finals. But Gershevitch (§960 with fn.) finds this loss of intervocalic *k* 'unparalleled in Sogdian' (which it is); he reads the ending as short *-ē*, and assumes (§251) that 'after the loss of all endings, the final *k* of **-aka-* disappeared', *-ak* then becoming **-ē* as in Modern Persian.

I cannot accept this theory. First, Sogdian should not be viewed from Persian, a Southwest Iranian language, but from Northeast Iranian; and in Saka the *aka*-stems have the nom. and gen. in *-ai* (from **-aki* and **-akē*), the acc. in *-au* (**-aku*), and the voc. in *-ā* (**-aka*), of which endings *-ai* and *-au* are apparently identical with Buddh. Sogd. *-y* and *-w*. Second, Gershevitch's view is clearly refuted by the *aka*-stem voc.-abl. in *-'*, like Chr. voc. *banṭā* 'servant' beside nom.-gen. *banṭē* (cf. Gershevitch §1253 and 1255). The fact that original **bandaki* and **bandaka* give *banṭē* and *banṭā*, shows that *-ē* and *-ā* contain the old endings, and mean *-ē* and *-ā*. If all endings had been lost, and then *-ak* had become *-ē*, the old **-aka* of the voc. and abl. could not have become *-'*. In the same way Buddh. *-w*, which of course also makes difficulty for Gershevitch, is the old acc. in **-aku*; but here the original case value is no longer preserved. Third, I doubt that short *ē* would have been regularly written *-y* and *-yy* in Buddh. and Man. Finally, the long *-ē* is directly attested by Armen. *margarē* 'prophet' = Buddh. Sogd. **m'rkr'y*, recorded in the pl. *m'rkr'yt* (ZII 4.163). The special development of intervocalic *k* in the suffix *-aka-* is due to special conditions of word extent, perhaps accent, and the semantic insignificance of the *k* in the suffix. In fact, the Persian change of *-ak* to *-ā* also is confined to the suffix; Av. *taka-* 'course' gives Pers. *tag*. Incidentally, I do not see that the loss of final *k* is better supported in Sogdian than that of internal *k*.

For the *aka*-stem feminine in *-ch*, Gershevitch replaces my explanation with one by Henning. I had proposed that the *č* might have arisen in the oblique cases, e.g. the gen. **-akayāh*, **-akayē* having become **-akyē*, whence by palatalization **-ačē*; but in view of the predominance of the nom. and acc. in the feminine I never was quite satisfied with this theory. Henning takes a quite different approach. He points out that in Sanskrit the *aka*-adjective regularly has a fem. in

-ikā-, and that *ikā- in Sogdian gave -ča, with palatalization of the *k* by the PRECEDING *i* (as in the Slavic type *otec* 'father' from *otikos), cf. *knč 'girl' (attested gen. *knčy*) from Av. *kainikā*-, etc. (Gersh. §247-8). I think this is the correct explanation.

There are many other points on which I might comment, by way of objection or commendation; but enough of details. This book is the most comprehensive and advanced grammatical treatment of Sogdian that exists, a great credit to its author and to his teacher.

Sardische Studien: Das mediterrane Substrat des Sardischen, seine Beziehungen zum Berberischen und Baskischen sowie zum eurafrikanischen und hispano-kaukasischen Substrat der romanischen Sprachen. By JOHANNES HUBSCHMID. (Romanica Helvetica, Vol. 41.) Pp. 137. Bern: A. Francke AG. Verlag, 1953.

Reviewed by ERIC P. HAMP, *University of Chicago*

This monograph, the Vorwort proclaims (7), started out to be part of a book review of Max L. Wagner's *La lingua sarda* (Bern, 1951). The reader may rest assured that the present review will grow to no such proportions, not the least of the reasons being the fact that this reviewer cannot lay claim to the vast body of erudition displayed by Johannes Hubschmid.

1. The author's object is to track down and organize pre-Latin loan material in the Romance and Mediterranean area. In many regions, this task is complicated by the presence of known pre-Latin non-Italic IE remains; Sardinia is claimed to be especially favorable for the job of extending our knowledge and penetration of time-depth: 'Da nun Sardinien erst durch die Römer indogermanisiert wurde, fehlen im Sardischen vorrömische, indogermanische Zwischenschichten. ... Vorromanische Wörter ausserhalb Sardiniens mit Entsprechungen im Sardischen müssen vorindogermanischen Ursprungs sein, sofern die sardischen Wörter nicht erst durch die Romanen nach Sardinien gebracht wurden' (16).

An exhaustive investigation of the pre-Latin elements of Sardinian is disclaimed (18); the classes dealt with are Pflanzennamen (19-38), Geländebezeichnungen (38-69), Fauna und Körperteile (69-73), and words with pre-Latin suffixes (73-88). With these specific word-groups treated, there follows (89-121) a more general discussion of the supposed pre-IE substrata of the Romance area. General conclusions and methodological reservations are succinctly set forth (122-6), and, within the detailed set of indexes (127-37), there is included (128-33) a useful alphabetical tabular display of all word-groups discussed, showing representative Sardinian, Berber, Basque, Caucasian, and Romance forms. A total of 68 base-morphemes and 11 suffixial morphemes (a few of which could turn out to be complexes) is discussed and assigned to the pre-IE 'Mediterranean' substrata.

2. Numerous forms of great intrinsic interest to the linguistic history of Europe, in sundry of its phases, are cited, and raise points which would provide the makings of a whole series of miniature essays. But these would disproportionately prolong the present review, and are typical of the sort of extended and

more leisurely discussion that is best carried on in its specialized place, and that such a book as this ought to provoke. Instead, I propose to dwell, in this review, on points of more general methodological purport.

3. The first point that suggests itself is the general method for identifying and assigning, classing, and disposing of forms such as those which this monograph has as its focus of interest. The closest thing found in Hubschmid's book to a statement of the general method occurs on page 124: 'Bei der Erforschung von Wortfamilien vermutlich vorindogermanischen Ursprungs haben wir drei Etappen zu unterscheiden: 1. die Aufstellung verschiedener, miteinander etymologisch zusammenhängender Wortgruppen; 2. den Nachweis, dass diese Wörter nicht indogermanischen Ursprungs sein können; 3. die Zuweisung von Wortfamilien zu bestimmten vorindogermanischen Sprachen, aus der sie ursprünglich stammen.' As a loose summary of some of the main observations that result from the study undertaken, this passage has a certain information value; but as a procedural statement of analytical stages and ascertainable goals of enquiry, it will simply not do. As for the first point, within a single language this operation forms a part of descriptive morphemics, and should be completed before any comparative work is attempted; outside a single language, or between languages, this process is not a single step on a par with the other stages referred to. As stated, the second point is analytically meaningless; one can of course demonstrate only genetic relationship to a given family (and hence the reverse only by well established exclusion), and not genetic nonrelationship. Regarding the third point, the author remarks: 'Viel schwieriger ist die letzte Aufgabe.' We may well agree, and the more so with his formula.

Instead, the following analytical steps are suggested as being applicable, logically consistent, and more susceptible of realization. A. Find the residual items in a given language that do not yield genetically to the comparative method; to this class will belong all non-inherited loan material, though it may also include isolated inherited items. B. Disclose the system, or systems, of their internal structure; this will indicate internal coherences and subclasses of perhaps different origins, and may reduce the number of entities to be explained. C. Assign all possible items to supposed source languages by finding phonetic/phonemic isomorphisms elsewhere in other linguistic structures. If these items match a whole known structure, they belong to that language or its ancestor; if they match other residua, the items collectively belong to a third language. If this third language turns out to be unknown as a whole structure, the entire result naturally has a much more tenuous probability. This is further reduced by the degree to which a sparseness of coherent structure is disclosed in the hypothetical language; and further by the amount of dependence that the total argument shows on extra-linguistic cultural systems and on geographical distribution, both of which tend to have relatively low and nonreversible correlations with linguistic systems. There are undoubtedly other variables that should also be introduced into the estimate of probability for the result.

It should ultimately be possible for us to calculate numerical values for all the complex relationships just referred to, so that for a given complex of data we may know, when we are finished with the projected linguistic relationships,

whether we are to any significant degree entitled to any opinion at all. In this fashion we may avoid endless argument and the rehashing of essentially discouraging data. Indeed, such a procedure could relieve the pressure for certainty where certainty is unattainable, and the demand for unswerving belief and absolute acceptance of a thesis that intrinsically has at best only low-probability properties. Too often scholars behave as if their entire reputation rose and fell with absolute acceptance or rejection of a thesis. It would seem more reasonable if merit were recognized on the basis of such characteristics as originality, thoroughness, and technical refinement, regardless of the probability of the thesis advanced; any well reasoned discovery or hypothesis of a given magnitude has equal claim to praise provided its author is aware of and makes clear the probability that the nature of the data yields. We may depend on various utilitarian and analytical considerations to ensure that problems are tackled somewhat in order of their degree of probability. Perhaps the principal criticism that can be levelled at Hubschmid's work is that he seems to be imperfectly aware of the probability characteristics of his network of considerations; he certainly does not make them plain.

The decision referred to under C above, whether the material under scrutiny matches a whole known structure or other residua, is often a difficult one. Just as Swadesh's glottochronology tends to indicate that some parts of the lexicon (it matters little that we cannot as yet specify for each language or culture area with exactitude which part this is) are retained with great and surprisingly constant tenacity, it also points to the likelihood that some lexical areas get replaced very rapidly. If, as successive languages invade an area, some areas of terminology are kept in a constant state of turnover, it is thinkable that, when we contemplate a given portion of the lexicon of a language and think we see intrusions in the structure, we may be peering several layers down all at a sweep, or, what is more disturbing for our analysis, the data may represent several levels of varying depth at some layers' remove. Flora and fauna seem in our experience to be especially vulnerable in respect of such replacements. Reasoning along this line and bearing in mind the categories dealt with by Hubschmid, we get a jolt as we read (123), 'Sie sind so zahlreich und beziehen sich auf KONKRETE BEGRIFFE [emphasis Hubschmid's], so dass lauter Zufälle höchst unwahrscheinlich wären.' Where on the basis of agreements with Basque he argues (118) for a relationship of the substratum to Basque, we may also wonder whether these items may not themselves be loans in Basque. The fact that such items appear to have a limited Basque-like morphology may not mean much; we know how such loans get reshaped to the borrower's morphology. We may then ask when we can ever claim to reach the bottom of such questions. Perhaps we can only claim to stratify reshapings, and that only under favorable circumstances. A sensitive and well-argued essay in this direction is Hubschmid's pre-IE alignment of Sard. *mara* 'swamp' (wrong gender, with due regard to geographical distribution, to be derived from Lat. *mare*) with the putatively earlier-borrowed prototype of the North and West European IE forms for 'sea' (positing a semantic development parallel to that in Germanic represented by OE *mere*). But

whether we can safely assign the ultimate source for these forms is another question.

4. We may now properly enquire into the sorts of errors encountered in the procedure sketched above. Since the entire operation depends on a prior exercise of the comparative method within all of the linguistic families discussed, overriding errors or looseness in the recognition of strict correspondences (Lautgesetze), in the assignment of proto-allophones to each correspondence, in the grouping of proto-allophones by the principles of complementary distribution and pattern congruity on the proto level into proto-phonemes, and in the recognition of strict stage-by-stage reconstruction of bifurcating proto-levels, with each succeeding structure disclosed within its own level—such errors or looseness will seriously affect the data used in all the succeeding steps. It is therefore disturbing to find imprecise statements such as these: 'Die Geminale -gg- konnte im Berberischen leicht zu -g(q)- werden' (26; see also the incompletely clarified Berber forms cited on 72); 'Das anlautende r- bekommt im Sardischen häufig einen vokalischen Vorschlag' (69); 'die vorromanischen Varianten *gal(l)ōne, *kalōne' (71); 'einen vorromanischen Wechsel von rr : (l)l und g : k' (72). In the discussion from which the last citation is taken one has the slippery sensation of touching too many things over too great a time-depth.

In some matters (e.g. 'der Wechsel von rr : r' (47), 'der Ablaut e : a und der Wechsel von ts : s' (48), 'Assimilation des vortonigen e an das a der Ableitungssilbe' (70)—cf. Nuorese *tanake* < Lat. *tenāx* (74), and the statement 'Das vortonige o entwickelt sich im Sardischen öfters zu a' (86)—all with a comfortably noncommittal terminology) it is difficult to say whether we are faced with Lautgesetz, morphophonemics, or borrower's phonemic adaptation. In well understood fields it is required that we state such relationships precisely; this requirement cannot be waived in the case of fuzzily demarcated fields of enquiry.

Certain important specific categories of errors which may be introduced into the procedure outlined above are labeled below with matching captions:

A. The failure to locate cognates within a family will affect the roster of residua isolated. An example of the elimination of candidate cognates is the author's ingenious argument (63-5), on grounds of gender distributions within Romance, regarding Sard. *mara* 'swamp', referred to above. When cognate languages are lacking, as for Basque, surrogate arguments may sometimes be framed on the basis of internal structure. Thus, Hubschmid remarks (59): 'Bask. *pentoka*, *pantoka*, *pantupa* sind nicht, wie Schuchardt annahm (ZRPPh. 11, 477-478), romanischen Ursprungs und zu vergleichen mit nprov. *pendo*, gask. *pento* "pente" (rom. **pendila*), da die Suffixe -ka und -pa bei einer solchen Hypothese schwer erklärbar wären.' But suffixes, if productive, can be added to new (loan) bases, as Hubschmid himself points out (73): '... deren Stamm teils vorrömisch, teils lateinisch ist.'

B. Much confusion and waste of time in perceiving the structure of the items under consideration can be caused by incomplete analysis. For example (71): 'In oberitalienischen Mundarten ist **garra* mit einem l-Suffix erweitert worden'; but how its status is assured Hubschmid does not tell us. If we had a complete

summary description of the place of such suffixes in the relevant dialects and in residual items, we could see in an instant how far the morphemic cut was justified and with what number of items we were dealing. The author quite rightly points out (75): 'Das Studium der sardischen Ortsnamen, vor allem von Namen, die mehrmals vorkommen oder sich auf bestimmte Geländeformen beziehen, dürfte ungeahnte Erkenntnisse über die Wortbildung des Paläosardischen vermitteln.' And, we may add, the very first operation with such a corpus should be an exhaustive internal morphology, not just an antipasto sampling for citation purposes, which seems to be the common habit. In such an analysis, wherever possible, multiple solutions should be overtly given; the rejected analysis on one level frequently turns out to be the dominant structure on another level. A long and tantalizing list of suffixes is given (87-8), where there appears to be much of value, but where only an example or two occurs for each item; such a list is relatively meaningless without further attestations and without some indication of completeness or the limits of a class of forms.

A further source of error in such studies is the purely statistical malady of poverty in numbers; and mathematical correction in the final result would ultimately have to take care of this difficulty. In any study like the present one, this difficulty is understandably a marked source of embarrassment, and can become acute; but the trouble must be faced squarely.

C. The main fault that intrudes in the search for isomorphisms is the failure to follow the very object of enquiry by yielding to the impulse to jump at isolated likenesses. All comparative work on linguistic structures, whether of genetically inherited or of loan material, like most problems of scientific taxonomy, rests on the recognition of maximal near-isomorphisms; a single disconnected point will simply not constitute a configuration, and if that is all that is left of an earlier pattern it can never be demonstrated that the pattern was in fact ever there. Because of allowance that may be made for phonemic adaptation in the case of loans, greater tolerance is permitted in the degree of fit with residua than with inherited material; but in principle, for supposed loans, too, a patterned matrix of sound correspondences must be sought. Hubschmid would appear to agree with this point of view when he says (123): 'Im Gegensatz zu Forschern, die nur einzelne Wörter aus einigen miteinander nicht nahe verwandten Sprachen herausgreifen ohne eingehende Detailuntersuchung, habe ich mich bemüht, so weit als möglich den gesamten, mir zugänglichen Wortschatz der betreffenden Sprachen mit allen ihren Mundarten durchzusehen.' Yet it is with some alarm that we find ourselves requested (109) to leap from sundry Romance forms to a mélange of forms in Slovenian, Armenian, Caucasian, Basque, Sanskrit, Welsh, Burushaski, and several Berber dialects!

5. Hubschmid traces his corpus of residua to two separate substrata: 'das eurafrikanische Substrat' (93-103) and 'das hispano-kaukasische Substrat' (103-19). These substrata are set up mainly on the basis of vague geographical distributions among the residua and supposed allied forms, and not on the basis of any matrix of patterned structural properties. Amongst the evidence for the Eurafrian entity, much rests on a very few tenuous lexical identifications in Berber and West European dialects. The Hispano-Caucasian entity is raised on a

quicksand of guesses stretching from Iberia through Europe, the Aegean, Anatolia (with a possible detour across North Africa; see 119) to the Caucasus: much of the fuel seems to come from the old unsystematic ruminations about Basque and Georgian and the like. As soon as somebody can show a tentative set of phonemic correspondences, serious linguists will listen with interest. If this cannot be done, let some of the armchair globe-trotters go out into the field, settle down on some one dialect for a couple of years (say, among the Basques), and collect some descriptive data to work with.

En route to the Caucasus, the attempt is made (105) to relate the name of Sardinia to Lydian *ai Zápōeis*. But what about the Lydian forms in *šfard-* (Friedrich, *Kleinasiatische Sprachdenkmäler* 9.11 and 22)? Under the rubric 'Lautliches' (106) we find naive and superficial typological irrelevancies where we might look for succinct and businesslike fundamental observations: 'Charakteristisch für das Hispano-Kaukasische ist das Fehlen eines *f*-Lautes. [Likewise Mongolian, Iroquois, and Proto-Indo-European.] Das anlautende *r* bekommt im Sardischen häufig einen vokalischen Vorschlag ... Beachtenswert ist auch die dem Sardischen und Baskischen gemeinsame Anaptyxe ... Typisch für viele Substratwörter ist schliesslich die Verdoppelung oder Längung inlautender Konsonanten.' The author remarks that 'das Baskische kennt, von *rr* abgesehen, keine Doppelkonsonanten' (106); is it not rather that Basque has two distinct unitary *r*-like phonemes, one of which happens to be written with the digraph?

Though he thus travels from Iberia to the Caucasus, Hubschmid enters a sober caution (117) against comparing distant items before neighboring dialect relationships have been brought into line. He points out that Bouda's comparison of Basque *apho* 'toad' with Svan *apxw* 'frog' is unjustified, since other Basque dialects show *zapo*, related to the claimed substratum form that when borrowed gave Span. Port. *sapo*. One would like to see more alleged Basque-Caucasian etymologies reinvestigated in this fashion.

For Sard. *bākkū* 'forra, gola di montagna' the author not only adduces various West European names for vessels and containers in **bakk-*, but goes on to say (44), 'können diese Wörter nicht getrennt werden von georg. *bako* "Geschirr" und seiner Familie.' The careful linguist is justified in asking, Why not? On the other hand, we find (79) a rewarding attention to patterned correspondences in the observation that there appears to be a correlation between accent and consonantism in Basque/Romance *-órru*, *-úrru*, vs. *-oru*, *-uru*, *-aru*.

6. There are a number of linguistic families included in this study for which our knowledge is still miserable, both as to the descriptive facts and as to understanding the genetic facts through comparison of close relatives; yet these groups seem at times to be dealt with as if we knew all about them. Our comparative knowledge of the languages of the Caucasus is notoriously bad, despite the fact that a start has been attempted by Trubetzkoy, Sommerfelt, Dumézil, and various native scholars; yet (118-9): 'Ebenso sicher ist es, dass die Iberer nicht afrikanischen Ursprungs sind; dies geht eindeutig hervor aus der nahen Verwandtschaft des Baskischen mit den kaukasischen Sprachen und aus der Tatsache, dass im Kaukasus auch **Iβn̄pes* als Volk bezeugt sind.' We should be aware by now of the danger in lighthearted manipulation of ethnic names. Our under-

standing of the interior comparison of Basque and Berber dialects is by strict standards mediocre. The last word has still not been said on the distribution and distinctiveness of the Proto-Basque consonants, and some statements (60, 81) reflect this state of affairs. We may agree heartily with the statement (93): 'Es ist auf jeden Fall wichtig, dass bei den berberischen Wörtern immer die Verbreitung angegeben und geprüft wird, ob eventuell nah verwandte Formen in den übrigen hamito-semitischen Sprachen, im Kuschitischen, Ägyptischen und Semitischen vorhanden sind.' And now, with Greenberg's views on the projected Afro-Asiatic grouping in mind, we must probably look even further before we are safe. It is a pity that Hubschmid does not observe his own caution. Our knowledge of Elamite and Sumerian relationships is pitiful, yet we read (103): 'Die im Elamischen und Sumerischen, d.h. in mit dem Kaukasischen verwandten Sprachen bezeugten Wörter ...' Our control of Etruscan is in many respects still imaginary, but we note the bland statement (80): 'Einem baskischen *-f*-Suffix entspricht also, wie es scheint, wenigstens in der Function, ein etruskisches *l*-Suffix.' One can guess what he means, but he has not said it.

Even on the matter of Indo-European, Hubschmid appears complacent where our knowledge is shaky. After citing some Phrygian, Etruscan, and Iberian names, the author is able to conclude (74-5): 'sodass die Erklärung von sard. *-ake* aus dem weiter verbreiteten mediterranen und vorindogermanischen Substrat nicht mehr in Zweifel gezogen werden kann.' It should be noted (a) that the exact status of IE *-k-* suffixes is still a problem, and (b) that the earlier linguistic layers of Asia Minor are still under such active scrutiny that many views are very much open to change. Wagner's caution, quoted in a footnote there, is much more to the point and healthful for future scholarship.

Once again, in the conclusion, we find the claimed relationships of Basque, Caucasian, Elamite, and Sumerian mentioned as if they were real pieces of knowledge—and this directly after the sound warning (122), 'Doch darf man daraus nicht schliessen, dass das Vorberberische und Baskische zur selben Familie gehören.' It would seem that for Hubschmid all unknown riddles have to be related.

An example of the author's boldness in etymologizing may be found in the discussion (80-5) of Nuor. *kóstike*, Logud. *kóstige*, via Basque, Gaulish, and Greek. Many interesting points are touched upon, and one admires his ingenuity, but the swift pace leaves one puffing for breath. After this sort of brisk exercise, the sober methodological warnings and reservations at the end (123, 124, 125) sound a bit hollow, and almost self-defeating. If we follow some of the cautions, perhaps no conclusion is possible.

Pyrenäenwörter vorromanischen Ursprungs und das vorromanische Substrat der Alpen. By JOHANNES HUBSCHMID. (Acta salmanticensia, Serie de filosofía y letras, Vol. 7, No. 2.) Pp. 81. Universidad de Salamanca, 1954.

Reviewed by GORDON M. MESSING, *Washington, D. C.*

The present booklet on pre-Romance words attested in the Pyrenees is the latest but, one earnestly hopes, not the last of a series of far-reaching studies

on pre-Romance vocabulary written or projected by the Swiss scholar Johannes Hubschmid. In *Praeromanica* (Bern, 1949) and in *Alpenwörter romanischen und vorromanischen Ursprungs* (Bern, 1951) he was chiefly though not exclusively concerned with pre-Romance items from the Alps; his *Sardische Studien* (Bern, 1953) explored Sardinian from a comparable viewpoint.¹ All of these works show certain common traits. Hubschmid is methodical and has drawn extensively upon published and unpublished dialect materials, some of which he has himself collected. His reconstructions of the underlying etyma, drawn from this elaborate documentation, are in a sound linguistic tradition. It is only with his more tentative interpretations, superimposed on the immediate results of comparison, that one can occasionally quarrel. An example may make this clearer. In *Praeromanica* 18-26 he cites a considerable number of dialect words from Alpine districts meaning 'white grouse' and divides them up, not at all unreasonably, among three variant bases **kalabria*, **gelabria*, and **gelabrio*. He then tries to fit these bases into an Indo-European framework by deriving them from a putative **kalalblia* 'white-spotted' < **kal-*, cf. Lat. *cālidus* 'having a white forehead', and **-alblia*, cf. Lat. *albus* 'white' etc. A compound of this sort, in which both elements mean 'white', is at least somewhat unusual semantically. The etymology of Lat. *callidus* (the spelling of the glosses) or *cālidus* (Ernout-Meillet) is itself fairly uncertain. The second part of the alleged compound, though possible, is unattested. Finally, the dissimilation necessary to account for **kalalblia* in its proposed subsequent development, though permissible, adds still another link to a chain of improbabilities. In short, Hubschmid's manipulation of conjectural bases or 'roots' as the last step in an otherwise impressive argument leads him, as here, to questionable results.

This is a general criticism which holds for the work under discussion. Hubschmid's research has been painstaking as usual and his knowledge is vast; here and there his ultimate conclusions appear to go too far. For example, he catalogs in minute detail a family of words cognate with the river names *Garonne* and *Gironde* (14-7) in order to reconstruct a 'Gaulish' base **garuna*, **garunna*, **geruna*, **gerunna* (and 'hypercorrect' **gerunda*). The reconstructions are fully in accord with the linguistic evidence. But why 'Gaulish'? Because, by a somewhat circular argument, Hubschmid boldly if not rashly identifies these bases with the known Gaulish base meaning 'crane' (*garanos*, cf. Gaulish *tarvos tri-garanos*); then, having made the identification, he calls his river names 'Gaulish'. There is actually no compelling reason to accept this etymology, which, in the absence of all confirmatory evidence, may rest on a random and partial resemblance. Incidentally, Hubschmid's account of the fluctuation in stem vowel is unintentionally puzzling. Even a careful reader will be induced to suppose that Gaulish, in addition to the stem **gar-*, possessed another stem **ger-* cognate with Gk. *gēranos* 'crane'. Evidently, he does not mean this at all, since a little further on (18) in a different context he speaks of the Gaulish *a* for expected *e* which occurs occasionally before *r* 'as in **garun(n)a* beside **gerun(n)a*'. The nub of the comparison lies in his justification of the considerable discrepancy seen

¹ [See the reviews of these works, respectively by Whatmough in *Lg.* 26.298-9 (1950), by Whatmough in *Lg.* 28.268-9 (1952), and by Hamp in *Lg.* 30.488-94 (1954).]

in the formation of *garanos* and Gk. *géranos* on the one hand and the bases reconstructed from his own material on the other. He postulates that Gaulish must have had another independent word for 'crane' which had the form **garun(n)o* or **gerun(n)o-*. This series of speculations warrants further comment:

(a) The contrast in stem vowel between Gaulish *garanos* and its congeners (Cornish, Welsh, Breton *garan* 'crane') and Gk. *géranos* etc. is expressly defined in Pedersen, *Vergl. Gr. d. keltischen Sprachen* 1.38-9; *garanos* is a notable example of an occasional shift of IE *e* to Common Celtic *a* under phonetic circumstances rather hard to pin down. We may take it as reasonable that *garanos* is from earlier IE (but not Celtic) **geranos*. Yet if this change is attested, and in this very word, as Common Celtic, why should forms in **ger-* occur in Hubschmid's supposedly Gaulish base?

(b) The assumption of two independent words for 'crane' is likely only if both are otherwise attested (like OHG *chranuh*, Mod. Germ. *Kranich* beside OHG *krano*, Eng. *crane*). This is not the case here, since the anomalous form **garun(n)o-* or **gerun(n)o-* means 'crane' only by hypothesis.

(c) Hubschmid elsewhere remarks (45) that 'ein vorindogermanischer Wechsel von *a* : *e* ist häufig'. Does not the occurrence of this same variation here, in a formation which seems opaque from the Indo-European standpoint, suggest the equally feasible deduction that this is a non-IE rather than a Celtic word?

Another example is particularly striking, the name of the Alps. He indicates that the Alpine dialects use the word *Alpes* to mean 'Alpine meadows', a meaning also attested in medieval Latin. He takes this to be the original meaning of the word, and posits a Gaulish **alpis* 'meadow' < IE **al-* 'nourish'; **alpis* would originally have meant 'fodder (for cattle)'. To the reviewer it seems on the contrary more likely that the dialect form shows a later specialization of meaning. Shepherds, taking their flocks 'up to the Alps' could have employed this expression in a technical sense to mean 'up to pasture'. The formation of Gaulish **alpis*, if this assumption is well-grounded, contains a *p*-increment which is possible in IE (e.g. Gk. *drépō* 'pluck' beside Gk. *dérō* 'flay') though rather rare; cf. Hirt, *Idg. Gr.* 3.259 on 'Wurzeldeterminativ *p*'. As it happens, no such *p*-increment is attested from this base, though there are numerous derivatives with a dental increment (like Lat. *altus* 'high') or an *m*-increment (like Lat. *almus* 'nourishing'). Hubschmid is not the first to attempt an etymology of this tantalizing name. The Alps have been interpreted as 'high mountains' (Servius on *Aen.* 4.442) or 'white mountains' (Paul. Fest. 4). In all probability they do not bear an Indo-European name at all (see Walde-Hofmann s.v. *Alpes*; Pokorny, *Idg. etym. Wb.* s.v. *albho-*); the widely dispersed place names of similar formation cited by Vittorio Bertoldi (*Colonizzazioni* 147) make this even more reasonable.

The above reconstructions do not carry conviction. The recent study of Venetic by Madison Beeler makes one doubt the appropriateness of Pokorny's term 'Veneto-Illyrian' as appropriated by Hubschmid; and indeed some of the words which he so categorizes are fairly tenuous. Nonetheless, his main theses seem sufficiently well established. There are, namely, Celtic elements in the Pyrenees, some of which are found also in the Alps (this seems to echo the theory of the conservatism of mountain fastnesses so much stressed by Toynbee); some of

the pre-Romance vocabulary of the Pyrenees, sometimes found also in Basque, duplicates pre-Romance items of the Alpine vocabulary. It might have been interesting if Hubschmid had attempted to correlate the second of these theses with the results attained by A. Tovar in his *Estudios sobre las primitivas lenguas hispanicas* (Buenos Aires, 1949); Hubschmid could also perhaps have consulted Luis Michelena's *Apellidos Vascos* (San Sebastian, 1953) as a further guide to Basque place names.

In conclusion, two matters of detail deserve brief discussion.

(a) With reference to a cluster of plant names found in the Pyrenees (43) Hubschmid cites Gk. *gérinthoi* 'peas' and further compares Georgian *gorwela* 'type of pea'. The Greek form, a lemma in Hesychius, appears also as *gélinthoi* and *gdlinthoi*, with the characteristic wavering between *a* and *e* often associated with 'Mediterranean' words (cf. the remarks above apropos of this same phenomenon). Since in all cases these words are glossed by *erébinthos*, Hubschmid should have translated his example as 'chick-peas, Kichererbsen'. Yet in any case there is considerable room to doubt that these words are related either to the Georgian form or to the other supposed congeners cited by Hubschmid (e.g. Prov. *garouto* 'Lathyrus cicera' < pre-Romance **gariutta*). As so often in the glosses, *gélinthoi* etc. almost surely stand for **wélinthoi*, with gamma for digamma. This would simplify their indubitable relation to *erébinthos* and to other lemmata similarly glossed: *rhábinthos*, *lébinthos* (see Walde-Hofmann s.v. *ervum*; *rhábinthos* is apparently not in Liddell-Scott-Jones-McKenzie, while *lébinthos* is cited as *lebínthioi*). No doubt all these words are also related to Lat. *ervum* 'vetch', Gk. *órobos* 'chick-pea', since the variations merely betoken different attempts to render a foreign word.

(b) Similarly, Prov. *gaudre* and related words for 'ravine, torrent' (44-5) may represent a pre-Romance **gabatro-*, but one is a little unhappy when Hubschmid connects these words with Lat. *gabata* 'plate, wooden container'. The latter word, probably borrowed from Semitic, seems to have too restricted a range of meaning to be of any use in this context (cf. Walde-Hofmann s.v. *gabata*).

The above remarks may indicate some differences of opinion. They are not intended in the least to detract from the very solid results embodied in this as in all of Hubschmid's prior works.

Lateinisches etymologisches Wörterbuch. By ALOIS WALDE, 3rd ed. by J. B. HOFMANN. (Indogermanische Bibliothek, 2. Reihe: Wörterbücher, No. 1.) Vol. 2, fasc. 21, *vēnus-zōnātim*, pp. 753-851. Heidelberg: Carl Winter, Universitätsverlag, 1954.

Reviewed by GEORGE S. LANE, *University of North Carolina*

We have before us the last fascicle of this 3rd edition of Walde, begun nearly a quarter-century ago in 1930. I need not repeat the general remarks that I made in my review of the fascicles immediately preceding (*Lg.* 30.110-4), except to remind the reader that differences of opinion on this or that etymology can arise, and minute corrections of cited forms can be undertaken, only after long and intimate daily use of the book. Moreover, the differences of opinion will

be that only and no more; for in most of the etymologies to which they apply, there is no plausible clear-cut solution.

As before, I believe that linguists will be best served if I pay particular attention to the Tocharian forms cited by Hofmann; for this is the first time that such forms have been employed on a large scale in what will doubtless be a standard reference work for the next generation of Indo-Europeanists. In dealing with Tocharian etymology, one must bear in mind that the phonology, especially the vocalism, is still highly uncertain, and must await (particularly as regards Kuchean, or Tocharian B) a great deal of research in metrics and accentuation. Furthermore, many 'ghost words' have arisen in earlier transcriptions, and innumerable mistranslations have occurred; even now the meaning of many words is uncertain or unknown.

First, a general remark. There is remarkable inconsistency in the orthography of the cited forms, especially in the use of *ś* and *ç*, *ş* and *š*. I think it would have been wise to take *ś* and *ş* as standard; certainly the same form should not be transcribed in both ways. In the remarks that follow, forms will be cited as they occur in the text before us; but all corrections will be made, without comment, in the 'standard' orthography used by Sieg, Siegling, and Schulze. References are to page and line; italic numerals refer to lines counted from the bottom of the page.

773.10: for *wšāšle*, vbl. adj. I to *wās*- 'dwell, rest', read the regular form *wsaš-šalle*. The pres. ind. sg. 3 is *wsaššām* (Krause 290). 775.3: Toch. B. *wātsi* is not quotable as a variant of *wātsi*; rather, *wātsi* and *wassi* (*wasy*). The imperf. indic. sg. 3. is *yāššitār* (Krause, loc.cit.). 789 below middle: read Toch. A masc. *wu*, fem. *we*. 809.1-2: read B *šaišše* (for *saišše*) 'Welt, Leute' (not simply 'Leute'). A *šoši* occurs also in the cpd. *ārkišoši* 'Welt'. For A *çol* read *šol*, and for B *çaul* read *šaul*; apparently the dot under the *l* is intended for the line used in narrow transcription. For B *çayam* 'er lebt' read *šaim*, and for *çamtsi* 'Leben' read inf. *šaitsi* (Krause 295). 815.14: for *esām* read (A) dual *esām*. The etymology is of course highly dubious. 824.14: for *weššām* read B *weššām*. The preterit—sg. 3 A *we* (*weñā-m* with enclitic pronoun), B *weña* (*wñā-ne*)—is certainly from the same root. 834, second paragraph: include (probably) A *wāl-* (pret. mid. sg. 3 *wālat*), B *wāl-* (subj. mid. sg. 3 *walatār*) 'cover, hide'. Same paragraph: B *wlāwoymar* means something like 'möge ich mich beherrschen', rather than '... abkehren'; cf. pret. ppl. in *š(e)k indrinta wawlāwau* 'der immer die Sinne gezügelt hat' (Sieg, *Udānālankāra-Fragmente*, No. 26a7 with translation). In the same paragraph, include (probably) A *wāl-* (pret. mid. sg. 3 *wālat*), B *wāl-* (subj. mid. sg. 3 *walatār*) 'cover, hide'. If any etymological connection is to be suggested for *wlāw-*, it should rather be with *walo* 'king': Lat. *valeō* etc. 840 s.v. *ūrīna*: add B *war* 'water'.

Aside from these corrections there is little that has met my eye. The typesetter seems still to be having difficulty with the choice between *æ* and *ae* (thus, 786.26: read *witwære* 'Witwe'). 785.23: for OS *frūcan* read *brūcan*. There is considerable fluctuation in the writing of Hittite *w*; thus, 753.14 *yenzi*, 758.19 *yerite-*, 825.5 *valḫ-mi*, 775.last *weš-*. Probably the last of these forms represents the most usual orthography.

One more observation, a general one. It would seem that the time has come for writers of etymological dictionaries to take account of the so-called laryngeal hypothesis. On many points, to be sure, there is no complete agreement yet—for instance, on the exact number of laryngeals to be assumed, on the existence of an *o*-colored laryngeal, and especially on the extent to which 'by-products' of the laryngeals are to be recognized in the development of the several IE languages, above all Greek. But there is general agreement already that the IE long-vowel series result from the lengthening of normal vowels by loss of a following laryngeal, that 'shwa' results from a laryngeal either directly or by contraction with 'shwa secundum', and (I believe) that long syllabic sonants result from a contraction of short sonants with laryngeals. Most scholars, moreover, probably accept the theory that the IE voiceless aspirates derive from simple voiceless stops plus a laryngeal (at least in Sanskrit), less rarely from voiced stops plus a laryngeal. As for the Sapir-Sturtevant theory of preaspirated PIE *hy* and *hw* (from laryngeal plus sonant), I do not know how widely this has been accepted.¹ Personally I find it most enticing, since it clears up the problems of initial spiritus asper vs. zeta and of spiritus asper vs. lenis in the case of initial diagrapha. (A case in point is the etymology of Lat. *Vesta*, Gk. *ἑστία*, 772-3. The assumption of an original root **Aew-*, with extended forms **Aew-s* and **Aw-es*,² would satisfactorily explain all the related groups.)

German scholars have in general been slow to accept the laryngeal hypothesis and to contribute to the solution of the problems which it involves. That the so-called laryngeals existed in the parent speech—call it Proto-Indo-Hittite or Proto-Indo-European as you will—has surely been proved long ago; it remains only to agree on how many they were and on what 'secondary' effects they had in the IE languages. This goal can be reached only by the continued efforts of all competent Indo-Europeanists. Since German scholars will no doubt assume in the future, as they did in the past, a leading role in Indo-European studies, it behooves them not to ignore, in the revival of these studies, the result of a quarter-century of progress, even though it has been achieved mostly in other countries.

The Latin language. By L. R. PALMER. Pp. ix, 372. London: Faber and Faber Ltd, 1954.

Reviewed by ERNST PULGRAM, *University of Michigan*

Anyone called upon, in the course of normal university instruction, to teach a class in the history of a language finds himself at once confronted by the problem of an unevenly prepared audience. Some students are, or wish to become, experts in linguistics; others center their interests on literary, cultural, 'philological' studies, and come to the lectures for background knowledge in a cognate

¹ On this and other controversial points see the excellent summary by E. Polomé, *Zum heutigen Stand der Laryngaltheorie*, *Revue belge de philologie et d'histoire* 30, 444-71 (1952).

² Using Sturtevant's cover symbol for an *a*-colored laryngeal (see his *Indo-Hittite laryngeals* 32) and Benveniste's formulation of the IE root (see his *Origines de la formation des noms en indo-européen* 147 ff.).

field. To steer a course between the Scylla of boredom for the first and the Charybdis of incomprehension for the second group requires of the professor an adroitness and patience not easily come by. Something has to be done, and I shall be glad to let myself be piloted in my next attempt by using Palmer's *Latin language* as a text. I daresay it is to a great extent a distillation of lecture notes and teaching experience, born with a split personality out of its parent's harassment. But its vices are also, for practical purposes, its virtues. In any event, the situation is not as bad as might be implied by the author's dismal promise (vii) that the book 'is not primarily addressed to specialists', to say nothing of the blurb on the dust jacket which cheerfully avows that 'no knowledge of the principles of linguistics is assumed'. The truth fortunately is that, while for learned references on difficult and disputed points of historical Latin grammar one may well turn to Buck, Sommer, Devoto, Niedermann, Kent, or Leumann-Hofmann, Palmer will do excellently—and this is said without condescension or reproach—as a textbook for the future linguist. At the same time, though classicists and scholars of Roman antiquity who are innocent of linguistics should not expect to pick up all of linguistic theory and terminology on the run through the book, they may well be led, under proper guidance in the classroom and with Palmer as a text, onto paths they have not trodden before, and learn to traverse the new terrain with profit and pleasure.

Palmer's aim was to state the communis opinio; but often various views are cited and, in addition, the author remarks disarmingly (vii): 'I have not everywhere been able to conceal the fact that I have opinions of my own.' The book is divided into two major portions: an outline history of the Latin language (3–205), and a comparative historical grammar (209–341). Then follows a bibliography, which might well have been more extensive in breadth and depth (342–5), next an appendix of archaic Latin texts (346–57), a subject index (359–63), and an index of Latin words (364–72).

Among the seven chapters of the first part there are two (Chapter 5, The development of the literary language, and Chapter 7, Special languages—Christian Latin) which will be of greater concern to classicists and philologists in the wider sense than to simon-pure linguists—not because of the subject they treat, but because of the manner of presentation, which strongly emphasizes stylistics and literary critique. Nonetheless, they will be wholesome reading for the simon-pure also.

Chapter 1 deals with Latin and its relation to Italic and other Indo-European languages. Palmer believes neither in an Italic nor in an Italo-Keltic prehistoric unity, and his arguments are persuasive. The discussion of the place of areal linguistics and linguistic geography in comparative Indo-European work is excellent (23–9). There is a fundamental difference between applying the principles of these two disciplines to 'a study of the behaviour of dialects in centrally organized states in which a given set of social forces governs the patterning of the linguistic data' (28) and using them to explain 'a totally different set of linguistic facts, i.e. the relationships of mutually unintelligible languages scattered over immense geographical areas' (28), that is to say, for material 'attested at widely different dates and under different circumstances' (29). For the same

reasons the term 'isogloss' should be eschewed where it does not signify what Palmer calls 'connectedness by chains of mimetic processes' (30).

Chapter 2 sketches the Indo-Europeanization of Italy as it is gleaned, of necessity, from anthropological and archeological rather than linguistic evidence. Much in this field is still controversial, but the large outlines of the communis opinio which Palmer traces are beginning to emerge clearly enough, now that various pet theories of skull-measurers and local patriots have been consigned to a vast prehistoric limbo. The same chapter contains quick portrayals of the various dialects of Italy spoken at the dawn of the historic period and until the flood of Latin engulfed them: Illyrian, Venetic, Sicel (does Palmer equate Siculi and Sicani?), Etruscan, Greek, Keltic, Lepontic, Ligurian. In Palmer's terminology, by the way, only Latin-Faliscan and Oscan-Umbrian are 'Italic' dialects.

Chapter 3 treats the Latin dialects and the earliest texts. Reading material is provided in the appendix.

Chapter 4 is entitled Spoken Latin—Plautus and Terence. This, and the title of Chapter 6, Vulgar Latin, are unfortunate choices because they are misleading. As for Plautus, and even more for Terence, Palmer comes ultimately to the conclusion that their comedies cannot render real spoken Latin because of the exigencies and traditions, inherited from Greece, of the genre. Perhaps I misunderstand the title, but so may others. Concerning the term Vulgar Latin, there is no linguistic reality corresponding to it and it 'remains a shimmering mirage' (149). Then why choose this mirage, without quotation marks, to head a long and most important chapter? Palmer's views on spoken Latin agree, I am pleased to note, fully and in detail with my own—cf. *Lg.* 26.458–66 (1950)—except that I finally proposed to eliminate the name Vulgar Latin because, to use Palmer's own apt words, 'we cannot hope to seize so Protean a phenomenon in a firm terminological grip' (149). Now Palmer's arrangement of Plautine 'spoken Latin' in Chapter 4, followed by a long and very good treatise with numerous well-chosen examples on the development of the literary language in Chapter 5, again puts the 'Vulgar Latin' of Chapter 6 in the inappropriate position of a later aberration from good Ciceronian Latin. True, in his text Palmer several times mentions the continuity of the spoken tongue as distinct from the literary language, a dichotomy which in Latin is possibly more applicable than elsewhere because of the strong subservience to foreign influence in literature coming from Greece, supported by social and learned vested interests. But the sequence of chapters and the wording of titles are mystifying at this juncture and obscure an issue which the author knows very well how to elucidate. And when, all of a sudden, the term 'Low Latin' is used (177) without further definition, clarity and precision are not served. But Palmer's description of what I should call Late Spoken Latin, again with many examples, is excellent. It would have been a fine idea, and a good deed, to continue the appendix of archaic Latin with sample passages, serving a purpose other than examples in the text, from Plautus, Petronius, Pompeian graffiti, Apuleius, and other late sources, so as to indicate the continuity of the history of the spoken language, or of the language tout court, so far as it is possible to winnow it out of any kind of written evidence. (I do not of course mean to intimate that to me written language is

not language. But for the historical linguist dealing with Latin and its modern continuations, the Romance dialects, the spoken language furnishes most authentically the unbroken evolution which he preferably would rely upon.)

The last chapter of the first part of the book selects Christian Latin to show the trend of development a language undergoes when it is employed for special purposes by a specific group of speakers and writers. The Latin of the early Christians and the Church Fathers is often slighted in our textbooks. It is presented here in a manner mainly of interest to scholars who wish to examine language from a sociological point of view and who want to trace the effect upon language of new social forces and a new ideology which seek a style.

Part Two, the historical grammar, is divided into the traditional chapters on phonology, morphology, and syntax. All problems, including the abstruse and notoriously thorny and controversial ones, are illuminated concisely in clear and lucid prose. (See, for example, the section on ablaut and laryngeals, 233-5, or on the formation of verbal stems, 265-81.) To put it tritely, difficult subjects need not be clothed in recondite language.

Some of the phonetic statements could have been tidied up a little. 'Certain types of sounds ... function as vowels and consonants' (222) is not very neatly said; *l* is not a consonant but, in Palmer's own terms, a sonant (222); to change *u* to *i* takes not only unrounding but also fronting (216); while *n* is a sonant and can be syllabic, it is not appropriate, in normal phonetic terminology, to call the sequence *en* a diphthong (234).

At least a dose of historical phonemics, as distinguished from atomizing historical phonology, would have been a good thing here and there. Consecutive listings of phonemic substitutions like *eu* > *ou* > *u* and *ou* > *u* (218) need some kind of phonemic exegesis. If, after a consonant, *y* (that is, *ɨ*) becomes *i*, the subsequent statement that *-dy-* becomes *-iy-* (or *-iɨ-*) either is false or invalidates a sound law just established. Or does the smaller print label this an 'exception'? It should have been added that *ɨ* becomes vocalic *i* only after such consonants as were not subject to a previous tendency to palatalization by which they were neutralized before *ɨ*. (By the way, there is no reason why historical linguists should not accept the regular phonetic notations of the IPA alphabet: [j] is the semiconsonant, [y] is the rounded high front vowel.) A consideration of Latin historical phonemics would also have led to a classification of vowels into accented and unaccented, rather than according to their position in initial, medial, and final syllable (214-22), for the very good reason that the cause of the changes discussed lies phonemically in the accentuation and not in the position of the vowels. This brings up the larger problem concerning the relation of stress and quantity (211-4), which should be examined very carefully some day from a phonemic point of view. Present statements of the three-syllable law make stress a matter of quantity, specifically of that of the penultimate syllable. Hence stress is predictable, that is, not phonemic. If that is so, questions of Latin stress should be dealt with accordingly, not in the same terms as, say, the English stress accent. Conversely, if stress is phonemic, then what does it do to the quantity of the penultimate syllable, and Latin quantity in general? Or are both stress and quantity phonemic?

In Palmer's morphology, I like the classification into *infectum* and *perfectum* tenses which is consistently adhered to and leads to a partition of *dīcō*, *dīcam*, *dīcēbam* versus *dīxī*, *dīxerō*, *dīxeram*. Thus the old Proto-Indo-European aspect system and the new Latin tense system which evolved from it are well brought out, and fortified by illustrations from Greek, which often takes an intermediate position.

Syntax is a difficult item in any historical grammar, but is dealt with here in a fully intelligible manner. If the traditional terms like *partitive genitive* and *instrumental-sociative ablative* produce an uneasy feeling of inadequacy, the fault lies not with the terminology alone; for unless explanations are sought at deeper levels, the superficial changing of names puts the cart before the horse contributes little to a better understanding.

In scope and size Palmer's volume lies somewhere between Lindsay's *Short historical grammar* or Conway's *Making of Latin* and Buck's *Comparative grammar of Greek and Latin* or Sommer's *Handbuch* or the Stolz-Schmalz-Leumann-Hofmann handbook. It is therefore a welcome book which can be used advantageously as a good text. While an author's name and reputation are not a sufficient guarantee against major disasters, Palmer has certainly delivered what was expected of him.

Para o estudo da fonêmica portuguesa. By J. MATTOSO CAMARA JR. (Coleção 'Rex', No. 11.) Pp. 176. Rio de Janeiro: Edição da 'Organização Simões', 1953.

Reviewed by FRANCIS M. ROGERS, *Harvard University*

In an earlier book, Mattoso Camara provided the Portuguese-reading world with a general introduction to linguistics based on recent developments in the subject.¹ He now has given his audience a treatise on modern phonemics, with special reference to Brazilian Portuguese.² It is modest and tentative, as the author freely admits, yet it is most opportune, given the current widespread interest in linguistic matters in Brazil. (Mattoso Camara is 'Professor Regente' of Linguistics in the University of Brazil, Rio de Janeiro.)

Para o estudo da fonêmica portuguesa consists of three chapters, really three quite separate studies. The first two, 'Fonética e fonêmica' and 'Os fonemas em português', represent revised and slightly amplified versions of two articles previously published in the *Boletim de filologia* of Rio de Janeiro, a learned journal on whose editorial board Mattoso Camara has served since its establishment in 1946.³ These two articles represent, in their turn, two chapters

¹ *Princípios de linguística geral como fundamento para os estudos superiores da língua portuguesa* (Rio de Janeiro, 1942). A 'revised and augmented' second edition has just appeared (1954).

² He has also published a work on Portuguese stylistics: *Contribuição à estilística portuguesa* (Rio de Janeiro, 2d ed. 1953).

³ *Para o estudo da fonêmica portuguesa: Fonética e fonêmica*, *Boletim de filologia* (Rio de Janeiro) 3:10.71-99 (1949), the basis of Chapter 1; *Para o estudo da fonêmica portuguesa: Os fonemas em português*, id. 3:9.1-30 (1949), the basis of Chapter 2. The latter article was reviewed by Paul L. Garvin, *SIL* 8.93-6 (1950), and by Helmut Lüdtke, *Boletim de filologia* (Lisbon) 12.353-5 (1951).

of his doctoral thesis, presented to the Faculdade Nacional de Filosofia in Rio in 1949. The third chapter of the book under review, 'A rima na poesia brasileira', is a reworking of a paper he had previously read at a meeting in Brazil.

In the first chapter, on phonetics and phonemics, Mattoso Camara presents a succinct review of the subject, including a balanced account of European and American contributions (he studied with Roman Jakobson in New York in 1943). It is lucidly written for a specific audience, and contains excellent definitions. In the text the titles of all studies referred to are given in Portuguese, the bibliography at the end providing the original titles. Technical terms are likewise translated, with the original English, German, or French given in parentheses. Examples are 'sons de transição' (*glides*, *Gleillaute*), 'padrões fonéticos' (*sound-patterns*), 'pauta de funcionamento' (*functional pattern*), 'traços' (*features*), 'feixe' (*bundle*), and 'fatos de juntura' (*junction*). Although de Saussure, Trubetzkoy, Jakobson, Sapir, Bloomfield, and many others are quoted at length, it is curious that the author does not refer to the work of Armando de Lacerda, a Portuguese scholar who long ago attempted to introduce the concept of the phoneme into Portugal and has written about it in the Portuguese language. In an article published in 1938,⁴ de Lacerda cites Bloomfield, Marouzeau, Trubetzkoy, and Twaddell, and of course Menzerath and Fra Agostino Gemelli.

The learned world will undoubtedly be most interested in the second chapter of the book,⁵ for Mattoso Camara is the first native speaker of the Portuguese language to attempt a systematic phonemic analysis of that language in the light of modern principles.⁶ Adapting to his own language Hall's subdivision of modern Standard French into archaic, slow colloquial, and fast colloquial, Mattoso Camara uses as his basis the 'tense' colloquial language of Rio de Janeiro. (He prefers 'tense' and 'relaxed' to 'slow' and 'fast'.) He then proceeds to his analysis. At the end (114-5) he presents nine conclusions. I shall translate these literally, and add comments to each.⁷

- (1) There are seven syllabic vowels, characterized by three oppositions: [1] front articulation vs. back articulation, linked with [2] no rounding of the lips vs. rounding of the lips, and [3] minimum mouth-opening vs. medium grade 1 vs. medium grade 2 vs. maximum.

The seven vowel phonemes are (in the traditional Portuguese orthography) *i*, *ê* [e], *é* [e], *a*, *ó* [o], *ô* [o], and *u*, as in *ti*, *sê*, *sé*, *pá*, *avó*, *avô*, and *tu*. Mattoso

⁴ Estrutura fónica, *Biblos* 14.313-28 (1938).

⁵ This chapter has 14 sections, whereas the article of which it is a modification has 10. Two sections of the original version have been subdivided to make four, and two new sections have been added (A sílaba tónica; Gradação da atonicidade), obviously in response to Garvin's review.

⁶ The book recalls a similar volume on Spanish: Emilio Alarcos Llorach, *Fonología española según el método de la escuela de Praga* (Biblioteca románica hispánica; Madrid, [1950]). A 'corrected and augmented' second edition has just appeared, dated 1954, with no mention of the Prague school in the title.

⁷ These conclusions are substantially the same as those presented at the end of the original article. They do not include mention of stress, which in his text Mattoso Camara recognizes as distinctive (cf. *sábia* vs. *sabia*, *contem* vs. *contém*).

Camara thus rejects *â* (phonetically symbolized by [æ] in traditional works on Portuguese phonetics), which he considers an allophone of *a* when unstressed (*bramar*, *bradar*) or when stressed and followed by a nasal consonant (as in *bramo*, *bramas*, but not *brado*, *bradas*). He believes that the opposition *â-â* between the first person plural of the present tense (*bradamos*) and the preterit (*bradâmos*), an opposition which is preached by the grammarians, is artificial in Brazil, existing only in 'archaic' speech. He even doubts that *â* exists as a separate phoneme in Portugal.⁸

(2) There are two nonsyllabic vowels, which contrast, respectively, with the front syllabic vowel and the back syllabic vowel of the minimum grade of mouth-opening.

The two semivowel phonemes are [j] and [w]. These, according to Mattoso Camara (73-4), are isolated by contrasting *vou*, transcribed [vow], with *vôo* [vou], or *sois* [sojs] with *soes* (from the verb *soar*) [sois], or by contrasting *viu* [viw] with *vi-o* [viu] or *deu* [dew] with *dê-o* [deu], or again by contrasting *acentue*, *sue*, *arrue* (present subjunctives of the first conjugation), ending in [ui], with *estatui*, *possui*, *influi*, *ruí* (present indicatives of the third conjugation), ending in [uj]. I suspect, however, that these are highly artificial distinctions; I question whether the semivowels are really separate phonemes rather than positional variants of *i* and *u*.⁹

(3) The vowel system suffers neutralization only in oppositions involving mouth-opening.

The vowels and semivowels can be grouped according to place of articulation: *é*, *ê*, *i*, and [j] on the one hand, *ó*, *ô*, *u*, and [w] on the other, with *a* outside the two groups. The same grouping is arrived at if the criterion is lip-rounding. Another grouping is reached if the criterion is mouth-opening: *é* is opposite *ó*, *ê* opposite *ô*, and *i* opposite *u*. Mattoso Camara consequently lists the seven vowels in two series: *a*, *é*, *ê*, *i* (four grades of opening); and *ó*, *ô*, *u* (three grades). It is these latter contrasts which suffer neutralization in unstressed positions. Thus, in Rio, according to the author, in pretonic position *ê* corresponds to both *é* and *ê* (*levar*) and *ô* corresponds to both *ó* and *ô* (*tomar*). Neutralization proceeds one step farther in unstressed final position, where the front vowels are represented by the 'archiphoneme' *i* (*nove*) and the back vowels by the archiphoneme *u* (*falo*). The following paragraphs (77) are significant and of value to foreign teachers of elementary Brazilian Portuguese: 'We may there-

⁸ In the original article Mattoso Camara rejected *â* as a phoneme. In his review Lüdtké pointed out that this does not apply to Portugal, where there are eight stressed vowels, not seven. In the book under review the author attempts to answer Lüdtké by adding a new paragraph (71-2) in which he expresses the belief that the distinction is artificial in Portugal as well. I tend to agree with Mattoso Camara that the distinction is artificial as far as the oppositions in the first conjugation are concerned. On the other hand, such oppositions as *a noite* vs. *à noite*, *a manhã* vs. *àmanhã* clearly exist in Portugal, both continental and insular.

⁹ In general Mattoso Camara believes that the semivowel phonemes exist only in falling diphthongs. The only examples of rising diphthongs are words in which [w] follows [k] or [g]; cf. *quais* [kwajs] vs. *coais* [koajs]. Again I believe that the latter distinction is artificial.

fore say that in [Brazilian] Portuguese¹⁰ there are three sets of vowels, with 7, 5 and 3 phonemes respectively: (1) set of stressed vowels: *a, é, ê, i, ó, ô, u*; (2) set of unstressed vowels in general: *a, ê, i, ô, u*; (3) set of unstressed vowels in certain positions: *a, i, u*. Before a nasal consonant, the stressed *a* is replaced by its allophone *ã* (back or closed) and the contrasts *é-ê* and *ó-ô* are neutralized, as in unstressed position, giving rise to a set of vowels phonemically analogous to no. 2 above.'

(4) There is no opposition between an oral vowel and a nasal vowel, because the vowels which are regarded as nasal can be resolved into a vowel followed by the nasal consonantal archiphoneme.

On this controversial question Mattoso Camara takes as his point of departure a statement by Jakobson that 'the postulate of nasal vowels is only necessary in a language in which there is a distinctive contrast between a nasal vowel and a vowel followed by a nasal consonant'; the French contrast between *bon* and *bonne* is then adduced. As this type of contrast does not take place in Portuguese, says Mattoso Camara, he eliminates the nasal vowels as separate phonemes, regarding them as normal vowel phonemes coupled with the archiphoneme of nasality, which occurs phonetically as [m], [n], or [ɲ] (cf. *pombo, canto, cancro*).

Mattoso Camara fortifies his point of view with other arguments, some of them historical. One of the most convincing is the fact that the *r* in words like *honra, genro*, and *tenro* does not act like the normal intervocalic *r*, but is strongly trilled, as if following a consonant (cf. *guelra, palra, Israel*).

This whole question is debatable, as Lüdtke points out. Mattoso Camara's position seems to stand or fall on the contrast between French *bon* and *bonne*; I should like to adduce the pair *vim-vime*, as pronounced in one of the continental or insular Portuguese dialects which suppress final *e* or pronounce it only as a support vowel (in the French manner). In this case, there would indeed be a contrast in Portuguese between a nasal vowel and a vowel followed by a nasal consonant.

(5) There are 18 consonants, characterized by three types of oppositions: as to the nature of the buccal obstruction; as to the point of articulation in the mouth; as to the role of the vocal cords.

(6) The oppositions with reference to the point of articulation in the

¹⁰ In his text (77) Mattoso Camara does not include the word 'Brazilian'. Farther on (85; 14 in the earlier article) he discusses the diversity in vowel phonemes between Brazil and Portugal. The differences between *fechar* and *fichar*, *morar* and *murar*, which are maintained in Brazil by the contrasts *ê-i* and *ô-u*, disappear in Portugal, where the unstressed vowel phonemes are *i* and *u* respectively. Mattoso Camara recognizes that there is phonetically an [ə] in continental Portuguese (as in *levar*), but believes that phonemically 'it is a question of what we would today in phonemics call a neutralization of the contrast *ê-i* in unstressed position; this "mute" *e* is in complementary distribution with *i*, structurally symmetrical with an *u* of the back series' (85; 13-4 in the article). In his review of the article Lüdtke challenged this statement, presenting the series of contrasts in *legar* with [ə], *ligar* with [i], *lugar*, and *lagar*, and stating that the opposition [ə]-*i* is neutralized only under the influence of a palatal consonant (as in *fechar*). In this book, however, Mattoso Camara holds his ground.

mouth may be summarized in terms of three features: labial articulation vs. front tongue articulation vs. back tongue articulation.

These two conclusions may be seen at a glance in the following table, which is valid for the prevocalic and intervocalic positions:

	STOPS		FRICATIVES		NASALS	(LIQUIDS)
labial articulation	p	b	f	v	m	(r)
front tongue articulation	t	d	s	z	n	(l)
back tongue articulation	k	g	ʃ	ʒ	ɲ	(λ)

For the first two liquids, the contrast labial articulation vs. front tongue articulation is replaced by a contrast vibrant vs. non-vibrant. Mattoso Camara quite properly isolates the phonemes [ɲ] and [λ] on the basis of contrasts like *venha* vs. *vênia* and *olhos* vs. *óleos*, as well as *nhata* vs. *nata* and *lhama* vs. *lama*.

(7) The vibrant liquid is a single phoneme, and the single-flap *r* should be interpreted as an allophone due to intervocalic position.

Mattoso Camara was inevitably bothered by the fact that, whereas initial *r* is always of one type, there are oppositions involving intervocalic *r* and *rr*: *caro* vs. *carro*, *era* vs. *erra*, *lora* vs. *torra*, *mira* vs. *mirra*, *muro* vs. *murro*. He eliminates this problem by maintaining that the *rr* represents a postvocalic *r* followed by an initial (and therefore strong) *r*: 'Thus, we can admit that the postvocalic *r*, phonetically perceptible, because of the word division, in *ar roxo*, also exists potentially in *arrôcho*, although without phonetic actualization [realização fonética]; its phonemic presence is manifested merely by the maintaining of the strong sound of the following *r*, which phonemically continues to be non-intervocalic' (109). This is essentially the same paragraph which he included in his original article (25). Lüdtke disagreed on the matter of *r*, and argued for two phonemes, [r] and [rr], bringing the total of consonant phonemes to nineteen. Garvin labeled Mattoso Camara's reasoning far-fetched.

(8) The stops and the labial fricatives never occur in postvocalic position, that is, as implosive consonants. They exist only as explosives.¹¹

In contrast with the 18 consonant phonemes to be found in the pre- and intervocalic positions, only four consonants are to be found in postvocalic position: (1) the archiphoneme of nasality; (2) and (3) the nonpalatalized liquids *l* and *r*; and (4) a front-tongue fricative which, according to Mattoso Camara, is the archiphoneme both of the opposition between palatalized and nonpalatalized, and of the opposition between voiceless and voiced, that is, of [ʃ] and [ʒ] vs. [s] and [z], and of [ʃ] and [s] vs. [ʒ] and [z]. The latter archiphoneme, he writes in somewhat oversimplified fashion, is a palatalized fricative in the 'Carioca' pronunciation (that of Rio de Janeiro) as well as elsewhere in Portugal and Brazil, except that in final position before a word beginning with a vowel it is [z].

(9) The elements of a prevocalic consonant cluster (explosive, to use de

¹¹ This statement does not include the palatal nasal and liquid, which never occur in postvocalic position either.

Saussure's nomenclature) whose second member is not a liquid are always linked by open juncture, with a vocalic glide as an accompanying feature.

Mattoso Camara thus eliminates the apparent contradiction of having stops and labial fricatives in postvocalic position, as in *pacto*, *advogado*, *ritmo*, *oftálmico*. The fact is that within these clusters, as well as within similar clusters occurring initially (*clónio*, *ptérico*, *psicologia*), a vowel is developed in ordinary pronunciation, a sound which Garvin calls a 'subphonemic filler vowel'. The first member is thus a prevocalic consonant. Cf. *p(e)neu*, *ad(e)vogado*, *ab(i)soluto*, with *e* occurring, according to the author, if the stressed vowel is *e*, *o*, or a more open vowel, but with *i* if the stressed vowel is *i* or *u* (79).

This latter statement is, in my opinion, highly debatable. I feel certain that I have heard the pronunciation *ad(i)vogado*, and even a pronunciation with an affricate [dʒ] before the *i*. I find equally debatable the vowel harmony which he says (also on 79) obtains in words like *devia*, in which the unstressed *e* is [i] because of the *i* in the stressed syllable, as opposed to *dever*, with unstressed [e]. I have certainly heard *seguro* with [e], and also *depois* with [i], and even with an affricate [dʒ] before the *i*. Incidentally, Mattoso Camara does not discuss the affricates [tʃ] and [dʒ], allophones of *t* and *d* which are so characteristic a feature of Carioca speech. It is of course possible that he does not consider that they occur in the tense colloquial language which forms the basis of his study.¹²

The third chapter concerns rime in Brazilian poetry. The chapter is relevant to the general subject of the book, for, as is well known, rime very often presents evidence of value to phonemic analysis. In particular, it reflects phonemics as practiced, often unconsciously, by poets. Mattoso Camara makes the latter point excellently on 144, where, referring to Vicente de Carvalho, he writes: 'na sua rima fêz fonêmica como M. Jourdain fazia prosa'. An introductory section discusses the concept of rime in Brazilian poetry and also rime in the light of general linguistics. A second section discusses rimes which are apparently imperfect, and a third those which are actually so. The author defines his terms thus (125): rime is perfect when the identity includes all distinctive features (e.g. *pala-bala*), and imperfect when the series of sounds diverge in certain of their components, whether simultaneous or successive. In the case of simultaneous components there is a divergence of phonemes (e.g. *enlêvo-mancebo*). In the case of successive components, there is a phoneme which is present in one series and absent in the other (e.g. *festa-orquestra*). In Mattoso Camara's opinion the fundamental reason for the use of imperfect rimes is the following (126): 'It is the feeling on the part of the poets that a complete identity of distinctive features is unnecessary, as long as there are structurally and functionally weak

¹² Two previously published analyses of Portuguese, both known to Mattoso Camara and used by him, are the following: Robert A. Hall Jr., The unit phonemes of Brazilian Portuguese, *SIL* 1:15.1-6 (1943), and Occurrence and orthographical representation of phonemes in Brazilian Portuguese, *SIL* 2:6-13 (1943); Holger Sten, *Les particularités de la langue portugaise* (Travaux du Cercle linguistique de Copenhague, Vol. 2; 1944). Still another study, which must have been known to Mattoso Camara (though he does not refer to it) is David W. Reed and Yolanda Leite, The segmental phonemes of Brazilian Portuguese: Standard paulista dialect, published in Kenneth L. Pike's *Phonemics* 194-202 (Ann Arbor, 1947), as 'Sample descriptive statement B'.

features in each series of sounds (these weak features being due to the phonemic structure of the language).'

Among apparently imperfect rimes the author discusses the following, all of which are phonetically of great interest: *Argus-largos*, *cálix-vales*, *nave-clamavi* [Latin], *impele-te-satélite*; *visse-superficie*, *ricos-obliquos*; *cérulas-pérolas*; *vou-avô* (alongside *só-avô*), *beijo-desejo*; *luz-pavis*; ¹³ *face-desfaz-se*; *lute-Lilliput*. The section on definitely imperfect rimes is likewise of significance to the linguist, for these rimes explain his statement, cited above, concerning the existence of features which are structurally and functionally weak. In other words, there is what might be called a scale of phonemic potency. The distinction between intervocalic *b* and *v* is a weak one, as is the distinction between stressed *é* and *ê* and between stressed *ó* and *ô*. We thus find rimes like *enlêvo-mancebo*, *belas-estrêlas*, *êrro-ferro*, *hoje-foge*, and *cega-grega*. Moreover, certain phonemes are so weak that they may be ignored and the residue rimed with another word: *feita-orquestra*, *virgem-vertigem*, *esmaga-vagas*, *Julieta-estreita*, *chega-veiga*, and (of great importance for Mattoso Camara's belief in an archiphoneme of nasality) *fundo-tudo*.

This book should be required reading for all North-American teachers of Portuguese, whether in the high school or the college. The first chapter is an excellent general initiation into modern linguistics, and the third serves to sharpen one's powers of phonetic and phonemic observation. The second chapter is particularly fine as a simplified methodology for the teaching of the pronunciation of Brazilian Portuguese.¹⁴

Lengua española. By LUIS FLOREZ. (Publicaciones del Instituto Caro y Cuervo, Series minor, No. 3.) Pp. 301. Bogotá: Imprenta Nacional, 1953.

Reviewed by WILLIAM E. BULL, *University of California, Los Angeles*

The title of this book is misleading: the author does not pretend to offer a complete and systematic description of any aspect of the Spanish language. He

¹³ I disagree with some of Mattoso Camara's discussion concerning the development of yod before final *s*. He writes (140) that 'a final sibilant with noticeable palatalization occurs in almost all Portuguese-speaking territory'. I am prepared to accept this statement, which echoes what he stated earlier (see above under the eighth conclusion), but I do not agree with his next sentence (140-1) that the mechanical consequence of this fact is 'the production of a yod between the vowel of the syllable and the sibilant, with the consequent elimination of the contrast [a]-[aj], [e]-[ej], [o]-[oj] in a final syllable closed by *s*.' Curiously, in my own mind I associate this yod with Brazil, and more specifically with São Paulo. And São Paulo is one area, if I am not mistaken, where final *s* is not palatalized.

Here is yet another illustration of the need for a competent PHONETIC treatise on the standard languages of Portugal and Brazil, as well as linguistic atlases, or at least adequate dialectal studies, of the Portuguese language as spoken in the two countries. As for Brazilian phonetics, Mattoso Camara quite properly omits mention of two short works which have appeared in the last fifteen years, since neither is adequate for his purposes: Cândido Jucá (Filho), *A pronúncia brasileira: A pronúncia do Português no Brasil para uso dos estrangeiros e das escolas brasileiras* (Rio, 1939), in Portuguese, French, German, and English; and Christopher Stavrou, *Brazilian-Portuguese pronunciation including word-list with indicated pronunciation* (Philadelphia, [1947]).

¹⁴ I wish to thank my colleague Thomas R. Hart Jr. for reading a draft of this review and for several valuable suggestions.

is concerned, rather, with what is commonly called adult education by radio, and his purpose is to satisfy the curiosity of the lay Spaniard, specifically the lay Colombian, whose knowledge of linguistics begins somewhat short of an adequate definition of the parts of speech and whose interest is titillated by comments on personalities, the Great Books, and a magpie's bag of disorganized and aimless information—on a somewhat higher level, to be sure, than that of the average U. S. radio fan.

The volume is a collection of twenty-two short and semipopular radio talks ('con algunas reformas y adiciones') which, under the title of 'Philological disquisitions', were sponsored by the Instituto Caro y Cuervo of Bogotá and were broadcast by the author over the national radio of Colombia during the year 1952. The author, with his sights trained on a radio audience, ranges over a wide variety of subjects which exhibit neither a central theme nor a common purpose and whose treatment is too elementary to be of any significant value to either the trained linguist or the student of Spanish who has gone beyond an introductory course in linguistics.

The talks may be divided into two groups. Twelve might be called linguistic. The author begins with an elementary definition of terms—morphology, syntax, phonetics, etc.—but fails to mention phonemics (9-17). He touches upon the parts of speech (19-31), the obvious facts about the origins of Castilian (61-6), the language of *Quijote* (merely a résumé of Rosenblatt's *La lengua de Cervantes*; 75-90), diminutives (91-8), some well-known characteristics of Latin-American Spanish (107-21), a few substratum influences (123-31), Colombian pronunciation (173-9), gallicisms (189-204), anglicisms (205-14), vulgarisms (215-35), and archaisms (237-54).

The remaining talks might, by stretching the term a bit, be called philological. Only by resorting, however, to hyperbole can they be termed disquisitions. There is a discussion of the normative aims of school texts (33-53), comment on whether the language of Colombia should be called Spanish or Castilian (based on Alonso's *Castellano, español, idioma nacional*; 55-60), a brief and inaccurate history of the prose of the Middle Ages (67-73), a talk on correct and proper speech with some examples of common Colombian 'errors' (99-105), an annotated bibliography of fifteen outstanding books on the Spanish of Latin America (133-142), a description of the major works of Andrés Bello, Menéndez Pidal, Tomás Navarro, and Amado Alonso (143-160), a short history of the Spanish Academy and its Latin-American progeny with some comment on their aims (161-71), an excursion into rhetoric in a discussion of hyperbole in popular Colombian speech (181-8), some description of the works of Rufino José Cuervo, concerned mostly with his *Diccionario* (255-65), and, finally, what in radio parlance can only be called a plug for the Instituto (267-89). This consists of a short history of the institution, a description of its aims, a list of its officers and working members (repeated on the fly-leaf), a bibliography of its publications (including a list of the articles published by its *Boletín* from 1945 to 1951), and, rather peculiarly, nine testimonial letters praising the Instituto and its publications.

It is futile to expect that a popularization aimed at a radio audience will meet the standards of scholarship, and it seems quite as futile to extend this review

with a wearisome list of half-truths, linguistic misconceptions, and errors which, in the last analysis, would lead only to a fruitless debate on what popularizers should and should not do.

Altfranzösisch ço — neufranzösisch ça: Eine syntaktische Betrachtung. By ERNST ZELLMER. Pp. 23, offset. Frankfurt a. M., 1954.

Reviewed by URBAN T. HOLMES JR., *University of North Carolina*

The earliest example of *ça* which has been found in a literary text is in Chateaubriand's *Mémoire sur la captivité de M. le Duc de Berry*, dating from 1833; the pronoun occurs also in Hugo's *Les misérables* (1862).¹ Oscar Bloch was one of those who postulate that *ça* is a popular contraction of *cela*; he claimed that it began to be used in the 17th century.² The dating may be correct, but it is extremely doubtful that *ça* stands for *cela*. For the reviewer, the true etymology is obvious from references in Richelet's dictionary, s.v. *ça* 'hither, this way'.³ The last reads: 'Ça, se prend quelquefois pour cela, mais il est populaire et familier: donnez-moi ça.' Immediately before this is the entry: 'Ça, interjection pour exciter & encourager à faire quelque chose: ça travaillons. En ce sens on dit encore Or ça. Or ça, verbalisons, hâtons-nous de verbaliser.'

The expression *Or ça* was common enough in the Middle Ages. In the *Histoire de Guillaume le Maréchal*, a fine work by a mature and excellent writer working in 1226, the phrase occurs often.⁴ A transition stage between *ça* 'hither' and *ça* 'this object' can be inferred from such a 16th-century passage as this:⁵ 'Le prebstre, sans ça ne cy, vous espousera par bonne guyse tous deux à l'entree de l'eglise.'

Zellmer does not make clear to me what his opinion is concerning the source of the pronoun *ça*. He states positively (1): 'Das Fortleben des alten *ço* bis in die heutigen Mundarten wird dann den Hintergrund für Aufkommen, Verbreitung und Verwendung von *ça* im Zentralfranzösischen abgeben.' At the close of his study he adds (23): 'Der demonstrative Lautkomplex *ça* erscheint in nominalen und adverbialen Satzfunktionen.' I infer that he would agree with me that the pronoun *ça* is an extension from *ça* < *ecce hac* 'hither', which has been persistently influenced by the presence of *ço*, *ceo*, *cela*, and the like. In that case the reader does not need such an extensive survey of the latter forms. Zellmer leaves an unexplained gap between the many examples that he cites and their relation to *ça*; the material in the first paragraph of this review is not given in his treatment.

¹ Brunot-Bruneau, *Histoire de la langue française des origines à nos jours* 12.311 and 13.82 (Paris, 1948 and 1953).

² Bloch, *Dictionnaire étymologique de la langue française* (Paris, 1932). See also *REW*³ §3965.

³ My edition is dated 1803, but earlier editions go back to the 17th century. So also Dauzat, *Dictionnaire étymologique* (Paris, 1938).

⁴ Edition by Paul Meyer (Paris, 1891). A good example, v. 981: 'Or ça tuit al bon chevalier!'

⁵ Quoted from Huguet, *Dictionnaire de la langue française au seizième siècle* s.v. *ça* (Paris, 1925).

Some of Zellmer's statements call for comment. He mentions (1) that there is a 'Gegensatz von hellen und dunklen Vokalen für Nähe und Ferne': true, but how does it apply in the present case? (A clear example from Vizcayan Basque: *etxian* 'in the house' : *etxion* 'in the house there'.) Again he says (8), 'Die Verwendung von *ço* geht wie bei den adjekt. *icest*, *itel*, *icel*, etc., auf den Bedarf an einer Silbe beim Versbau zurück.' This is contrary to my observation. When *itel*, *icel*, and the like are used, the poet intends special emphasis. It is true that emphatic expressions wear out, so that cases may have arisen where the deictic *i-* is used merely as an additional syllable; but this can hardly have been the dominant reason for its use. Old French poets did not strain for perfection in their syllable count.

I do not think that Zellmer has clarified the development of the pronoun *ça* beyond what we already know. But his examples are useful, especially as a supplement to certain sections of Rydberg's *Geschichte des französischen schwa*.

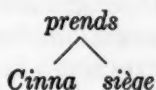
Esquisse d'une syntaxe structurale. By LUCIEN TESNIÈRE. Pp. 30, offset. Paris: Librairie C. Klincksieck, 1953.

Reviewed by HOWARD B. GAREY, *Yale University*

In this brief work Tesnière offers a system for representing the relations among the elements of a French sentence by a graphic system of lines, curves, circles, and other figures, reminiscent of the sentence diagrams that many of us used in school. It is sometimes hard to see, in the thicket of rules for the proper disposition of these figures, just what the premises are which underlie them. Only the first premise is clearly stated (3): the chain of speech is unidimensional, while the structure of a sentence is too complex to be represented by a straight line. There is much truth in this observation; but is the order of words the only physical feature of a sentence that has relevance to its syntactic structure? Certainly such audible traits as pitch, rhythm, tempo, and various stresses are at least clues to the analysis which goes beyond such physical features.

Tesnière's next assumption is that in any relation between two nuclei (except those joined by a coordinating conjunction) one of the nuclei is subordinate to the other. This resembles somewhat the Bloomfieldian notion that a phrase may consist of a head and its attribute, but omits the condition that if it is to be so analyzed the construction must be endocentric. According to Tesnière, in the sentence *Alfred chante*, *chante* is the superior or governing nucleus, while *Alfred* is inferior or subordinate. He does not tell us how he arrives at this conclusion; but it is a central part of his method to assume that the verb is the superior nucleus whenever it is connected with any other in a construction (4): 'Le verbe est le noeud des noeuds. C'est lui qui, directement ou indirectement, est le régissant de toute la phrase. ... C'est pourquoi, quand on établit un stemma [sentence diagram], il est de bonne méthode de commencer par le verbe.' Thus he regards subject, object, and indirect object all as special kinds of attribute to the verbal head, called *prime actant*, *second actant*, and *tiers actant* respectively. The other elements capable of subordination to the verb are the *circonstants*, classified as of time, place, manner, and so on.

Tesnière seems to ignore the possible importance of word-order, since, in making his stemmas, he reorganizes the sentence to accord with his vision of its structure. This method leads him to make statements which are either meaningless or untrue; for example (5): 'L'apostrophe [a noun in direct address] se comporte comme le premier actant [subject] d'un verbe à l'impératif: *Prends un siège, Cinna.*' Here he appends a diagram demonstrating that *Cinna* is the 'prime actant' of *prends*:



Now, what does he mean by 'se comporte'? In the order of words, in the phenomena of stress and pause involved in the use of apostrophe, in the verb form which accompanies such a noun, there is nothing which resembles the behavior of a 'prime actant' in a declarative sentence.

Tesnière has written a method of analysis of *syntaxe structurale*. The meaning of this term remains unclear. He does not describe the rules of order of the elements in a sentence; rather, he presents a method of denoting graphically certain relations between sentence parts. These are subordination (which he calls *connexion*), co-ordination (*jonction*), and change in form-class membership (*translocation*).

It is perhaps due to Tesnière's wish to be as brief as possible in a modest sketch of his views upon a very complex problem that so many ideas are presented without full justification. Where he has seemed to rely on intuition, it may be that he has simply skipped, in his presentation, the chain of reasoning which brought him to his results. His syntax, as far as one can see, is not solidly based on any previously worked out phonology and morphology; even word-order falls between two stools, while morphological considerations are brought into the syntax or left out apparently at will.

The stemmas limit the possibility of syntactic discovery, although Tesnière praises them for their 'rigueur et souplesse' (3); certain problems and lacunae exist only because there is no way in which the stemmas can deal with them. The stemmas become an end in themselves; some classifications, made on the basis of the stemmas, actually distract our attention from fundamental facts of sentence structure. The system ignores IC analysis, but is not an adequate substitute for it. In sum, Tesnière's method of graphic representation is clear up to a certain point of complexity; past this point it defeats its own purpose.

Language and history in early Britain: A chronological survey of the Brittonic languages first to twelfth century A.D. By KENNETH JACKSON. Pp. xxvi, 752. Cambridge, Mass.: Harvard University Press, 1953.

Reviewed by CALVERT WATKINS, *Harvard University*

Jackson's long-awaited book will undoubtedly take its place beside Pedersen's *Vergleichende keltische Grammatik* and the modernized Lewis-Pedersen as a—even as THE—standard reference work on the historical phonology of the Brit-

tonic languages from the Common Keltic period to that of Medieval Welsh, Cornish, and Breton. To employ Jackson's own expressive, if somewhat redundant description of an earlier book, 'this is an indispensable work of the first importance'. The criticism levelled below should not detract from this basic judgment. The title is an apt one, for Jackson's work is intended for Anglists, toponymists, and historians of early Britain in general, as well as for Kelticists; he has not only written a historical phonology, but has sought to relate the linguistic stages to their historical context. It is significant that the orientation of the book is toward chronology of sound-changes, absolute as well as relative, rather than to a description of the sound-changes alone. It stands as the most complete and systematic study of this kind yet made in the Keltic field.

The book comprises two parts, corresponding to 'history' and 'language' in the title. Part 1 (1-261) is concerned with the historical background of the Brittonic languages, and the linguistic and philological questions which are pertinent to this. About this section I have almost nothing but praise to offer. It is a very thorough linguistic history, encompassing all the techniques of historians to arrive at a result which is pre-eminently historical, rather than linguistic (or 'microlinguistic') in a strict sense of the word. But these results are invaluable to the student of Keltic, and this work is to be recommended as paradigmatic to all who seek to treat the history of a language. There are six chapters in this first part: 1. The Brittonic languages and the Breton migrations; 2. Written sources; 3. Britons and Romans under the Empire; 4. The British Latin loanwords in Irish; 5. The early Christian inscriptions; 6. Britons and Saxons in the 5th to 8th centuries. Each of these makes important contributions to Keltic linguistics. Deserving special mention are a carefully thought-out terminology for the chronological stages of the earlier Keltic languages, which the reviewer suggests might well be adopted by Kelticists in general; the analysis of the sound-system of the Vulgar Latin spoken in Britain; and the extension of Pokorny's relative chronology of the important Proto-Irish sound-changes.¹ In general the exposition of the genesis of loan elements and their relation to the historical and cultural situation is very well handled; Jackson is to be commended for his extremely thorough analysis of the English local names and the evidence they shed on the British language, evidence which has been hitherto either neglected or largely misunderstood.

Part 2 (265-705) is the linguistic section proper, the history of the sound-system. As stated above, this is oriented toward dating, and in this basic aim Jackson has indubitably succeeded. The relative chronology has been meticulously worked out on the basis of the linguistic evidence, and it is anchored by enough *termini ante* and *post quem* so that the absolute chronology cannot be fundamentally altered. But there are certain reservations to be made, and these apply in principle to the whole section under discussion. For despite an occasional use (and misuse) of the word 'phoneme' Jackson takes no cognizance of new methods either in descriptive or in historical linguistics. The work raises far more questions than it answers concerning the structural evolution of the British phonemic system and the relations between the units of this structure. The lack

¹ ZCP 12.415-26 (1918).

of attention devoted to these issues is in a sense the most serious drawback to this book, and to others similar in character if not in quality. One should not criticize Jackson for not doing what he never set out to do; but it should be pointed out that modern methods of linguistic analysis are not ends in themselves but new techniques, of proven efficacy in historical grammar. To ignore these is to jeopardize one's results. So for example Jackson dates vowel affection by *a* and *i* in final syllables somewhat before the loss of final syllables, on logical grounds. This is true phonetically, but viewed structurally the development of a phonemic contrast /*e*/ ~ /*ei*/ by affection—for Jackson contemporary with affection itself—could not have occurred until AFTER the loss of final syllables.² Jackson treats such developments as lenition, simplification of the geminates, and the spirant mutation as entirely separate, with a date for each; but in terms of linguistic structure they must be viewed as a complex of interacting phenomena. The quarter-century or less by which Jackson separates them in time seems for this reason a little artificial. Furthermore a structural (or structured) presentation is not merely a question of adherence to the newfangled. It is often one of credibility, in some cases even of intelligibility. The reviewer finds quite confusing the frequent introduction of strange new sounds—e.g. *ō*, *ū*, *ȳ*, *ī* in the section on affection—with little or no attention to how they fit into the system, to which ones are transitional and which ones permanent structure points, and to when all of this took place in relative time, the latter despite Jackson's careful attention to dating in other respects.

In all fairness to Jackson it must be stated that he does in several instances consider the phonemic situation of his postulated speech sounds, and successfully demonstrates that these were in fact minimally distinct units. But he does not carry the process far enough. An example is his discussion of Late Brit. /*e*/ ~ /*ei*/; this is typical both of what is good about the book—the extremely careful attention paid to accuracy and to detailed analysis—and of what the reviewer considers its basic shortcomings, as sketched above.

Briefly, the situation is as follows. During the late 5th or early 6th century A.D. the process of final *i*-affection resulted in a new phoneme /*e*/ which contrasted with the earlier /*e*/. At some later date this /*e*/ gave /*ei*/, by what is termed epenthesis. Internal *i*-affection, dated 7th or 8th century, resulted also in the formation of /*e*/, which was likewise diphthongized to /*ei*/ by epenthesis when the affecting sound was yod. There existed also in the British language /*ē*/ and /*ē̄*/, which by the 6th century were probably something like [ēⁱ] and [ē̄ⁱ]. These developed during the 7th and 8th centuries to /ui/ and /qi/ respectively. The history of /*ē*/ and /*ē̄*/ is to be linked closely with the 'new quantity system', the loss of phonemic vowel length, which is dated latter 6th century. Jackson's position on this (341) is that 'ēⁱ and ē̄ⁱ were not shortened at all, since their diphthongization had already begun', and that their later development to /ui/ and /qi/ was a direct one. This is tantamount to maintaining that they were preserved as (phonemic?) long vowels after vocalic quantity had become a function of environment. But such an awkward assumption is not necessary. We note that Jackson's conclusion regarding the date of epenthesis (/*e*/ > /*ei*/,

² See Martinet, Concerning the preservation of useful sound features, *Word* 9.1-11 (1953).

above) is that it did not develop until after the 6th century in the case of final affection, and definitely not until the 8th for internal affection. But since the results of epenthesis are exactly the same for both final and internal affection, there is no reason why we should not regard the process as taking place at the same time for both, viz. during the 8th century. Jackson shows that by this date his \bar{e} and \bar{e}' had already become /ui/ and /qi/; there could never have existed the possibility that \bar{e}' after the loss of distinctive length (6th century) would fall together with /ei/ arising from epenthesis in the 8th century. It is thus both simpler and more plausible to state that / \bar{e} / and / \bar{e}' /, phonetically [\bar{e}^i] and [\bar{e}'^i], became /ei/ and /qi/ after the loss of phonemic length, with the first element in fact shortened and the off-glide phonemicized, and that it was these diphthongal clusters which gave /ui/ and /qi/. The resultant history may be schematized in four chronological stages:

I		e	\bar{e}	e	\bar{e}
II		e	ei	e	ei
III	ei	e	ui	e	qi
IV ³	ei		ui	e	qi

This whole discussion is primarily to serve as an illustration. Those who are interested in the evolution of linguistic structures, and particularly in diachronic phonemics, will find Jackson's presentation a bit perplexing, and his inconsistencies often annoying. But it remains to his credit—and this could not be said about the work of a less careful scholar—that the descriptive facts are there. If the structuralist wishes to reorganize the material in a manner more compatible with his own linguistic tenets, he may; what is more important is that on the basis of Jackson's work he can. He need not cry for fuller or more accurate data.

In the remaining paragraphs I shall treat primarily questions of detail, cases where I believe Jackson's discussion should be augmented, modified, or rejected. A good many of these comments concern the Gaulish evidence on the phonological problems in question; my purpose in this is largely to correct the use of a term which is all too frequently misapplied in this work, namely 'Common Keltic'. It is unfortunate that Jackson did not consider Gaulish—the closest congener of early British—with the care and thoroughness that he devoted to Old Irish and British Latin, for it would have made unnecessary many of these remarks.

278. There are more than 'a few apparent exceptions in Romano-British names' to Jackson's statement that IE e before nasal plus stop gave i in Common Keltic. Note Gaul. Σερτιος beside *Sintacius*; *Venti*, *Venticus* beside *Vintio*, *Vintius*; *Tenconisius* beside *Tincius*, *Tincorix*; πεμπε-δουλα beside *pinpetos* 'fifth'; *Escengolatis*, εσκεγγαι, εσκεγγορ.ουι beside *Excingomarus*, εскиγγορειξ, εκσιγγος (with -κσ- for -σκ-). We should probably conclude that /e/ and /i/ were not distinctive in this position, but that either might appear phonetically. The change $e > i$ in any case may not be said to have been completed in the Common Keltic

³ Stage IV represents the Middle Welsh period, during which the close and open e fell together entirely.

period, as Jackson implies. He is furthermore inconsistent with his own statement in several instances, e.g. in writing Brit. **pempe* 'five' > W *pymp* (496), where one would expect the British form to be **pimpe*. The English river name *Derwent*, correctly derived by Jackson from PrW **derwént* < Brit. **derwéntiū* (Rom.-Brit. *Deruentio*), is enough to show that his rule cannot stand as it is.

306. Corresponding to the element *-yellaunos* (*-yall-*) in British and Gaulish are OW *-gwallawn*, MW *-wallon*, OB *-uallon*, which point to a base **yallānos*. Jackson follows Zupitza in regarding this as suffix-substitution; but in Gaulish we have also *Alaunus* : *Alanus* and *Vindausca* : *Vindasca*, which cannot be so easily explained away. This is in all likelihood linked with the Gaulish sound-change (under what conditions is not yet known) of the type CVuC > CVCC, cf. *Alaucus* beside *Alacca*.⁴ It would appear that in the Gaulish and British forms above a pattern *-vC-* has been substituted for the regularly developed *-vCC-*, a morphophonemic alternation which can be observed elsewhere in both languages.

336. It is probable that the change *dy-* > *d-* is in fact Common Keltic in date, as it is found in the earliest stages of all the Keltic languages; cf. W, OIr. *dall* 'blind', Ogam Ir. DALI (gen. sg.), Gaul. *Dalla* : Goth. *dwals* 'foolish'.

429. Jackson ascribes to Common Keltic the development, by assimilation, of geminated voiced stops across morpheme boundary, e.g. **ad-bero-* > **abbero-* > W *aber* 'confluence'. But Gaulish shows no evidence of this, and concrete examples of such clusters unassimilated, cf. *Adbitus*, *Adbucillus*, *Adgennius*, *adγevnopivγ*. I know of no Gaulish form with the prefix *ad-* before a morpheme in initial *d-*, though a few undoubtedly must have existed. If, as Jackson believes, the British assimilation is older than the 2d century A.D., for which time we have *-db-*, *-dg-* in Gaulish, then it must be regarded as a dialect feature of insular Brittonic. The mere presence of a similar development in Goidelic is not sufficient evidence for putting it back into Common Keltic.

512. Jackson has an unfortunate tendency to treat the principle of the regularity of sound-change in a rather cavalier fashion. For example he gives good evidence that *nd* > *nn* (i.e. nasal plus voiced stop) before the period of syncope: the fact that secondary *nd* syncopated from *nVd* is not assimilated, and the existence of the unproved Welsh name *Gwynnhoedl* < PrW **winn'hēdl* < Brit. **yindosaillos*. He states quite rightly that a PrW **wind'hēdl* would give W **Gwyntoedl*. But he goes on to give Baudiš' derivation of W *encilio* 'withdraw' from Brit. **andekil-* via Late Brit. **andegil-* > PrW **and'gil-* > **antgil-* by provection > **ancil-*, with subsequent affection of *a* to *e*. He comments that 'this example would seem to show that *nd* was not *nn* at the time of syncope', and gives the explanation that 'possibly in this word [NB!] there was enough trace of the *d* left to unvoice the *g*'. Linguistic change is a matter of a language system, not of this word or that word. The reader is left to ponder for himself why there would not be 'enough trace of the *d* left' in **winn'hēdl* to prove it to **Gwyntoedl*.

540. The assimilation of an IE cluster of liquid and *s* to geminate liquid is put by Jackson in the Common Keltic period. This is again unlikely, since Gaulish

⁴ See Whatmough, *Die Sprache* 1.126-7 (1949), *HSCP* 60.180 (1951).

presents several examples of these clusters preserved. For *-sl-* cf. *Atislius*, *Matislius*; for *-rs-* cf. *Carsius*, *Carsiaudia*, *Carsici*, *οερσικκος*. The last is taken by Pedersen (VKG 1.82-3) as *yer-siknos*, but there is no Keltic **sikno-*; the morphemic division is clearly *yersi-kno-*, cf. *λουκοτι-κκος* (: W *llygod* 'mouse'), *κρασσι-κκος*, and others. Note also that *οερσικκος* is early Gaulish, and that a century later we probably have the same morpheme in *Verrius*, with *-rs-* > *-rr-*. It is most likely that these changes took place in the several dialects at different times. The situation is quite parallel to the development of the same clusters in the various dialects of ancient Greek.

542. There is good evidence for *-ns-* > *-ss-* across morpheme boundary in Common Keltic times in the case of the prefixes **en-* and **kon-* in the British and Irish developments, and in Gaul. *essedum* 'chariot' < **en-sed-*. But the problem is complicated by the Gaul. name *Consuadullia* (*kon-* and *syādu-* : Gk. *ἡδύς* 'sweet'), which has an unassimilated cluster.

661. The derivation of the Rom.-Brit. local name *Rutupiae* from Early Brit. **ro-tupi-* (i.e. prefix *ro-* 'great') > **rōtupi-* is admittedly ingenious, but what is **tupi-*? Jackson gives neither assumed meaning nor etymology. In any case it should be kept apart from Gaul. *Rutuba*, which is better taken (with Sir Ifor Williams) as *rut-uba* : W *rhwd* 'filth'. Whatever its origin, *-uba* is a limitedly productive suffix in Gaulish, cf. *oltuba* (coin legend) beside the name *Olt-inius*.

The book concludes with a convenient six-page chronological table embodying the results of the dating of the sound-changes, with reference to the discussion in each case. There are three indexes: a subject index to Part 1, a fairly complete index of words and names discussed linguistically, and an index of inscriptions cited in the text. The publishers are to be congratulated on a first-rate job; misprints are blessedly rare, and insignificant where they do occur. I note the following: 132 for OIr. *ecsamli* read *écsamli*; 661 for Ausionus read Ausonius; 472 for W *penryhn* read *penrhyn*.

Participium universale im Slavischen. By JURIJ ŠERECH. (Slavistica, No. 16.) Pp. 44. Winnipeg: Verlag der Ukrainischen Freien Akademie der Wissenschaften, 1953.

Reviewed by JAMES O. FERRELL, *University of Michigan*

George Shevelov, writing under the name Jurij Šerech, has published in the West during the years since the second World War a large number of impressive works on Slavic linguistics, with particular emphasis on the East Slavic branch. His publications are characterized by an excellent command of the data, an enthusiasm for contemporary linguistic methods, and a determined search for the causes of structural evolution. As a result, he has firmly established himself as one of the leading Slavists of our period.

His *Participium universale im Slavischen* is a brief treatment of the very wide range of possible employments (particularly in respect to voice) of the participles in *-tyj* and *-nyj* in contemporary Ukrainian (together with comparative data from other Slavic languages, especially Serbo-Croatian and Slovak), an enquiry into the cause of the functional growth of these forms (which he finds in the loss of

contrasting tense and voice forms that existed in earlier stages of Slavic), and an attempt to discover the historical causes that led to the simplification of the whole system of participial oppositions as they existed during the period of Common Slavic and also in Indo-European—a change which Shevelov attributes to the growth of a new aspect system. The monograph is primarily of interest to specialists in Slavic; they will find it stimulating and informative.

The best part of the monograph may well be the description of the forms and functions of the participles in present-day Ukrainian. Here the author's authority is evident at every step; he is able to document almost all of his statements with convincing fresh quotations from Ukrainian writers. In the comparative sections there is also new material, but it is considerably scarcer. For the most part the author has contented himself with incorporating the examples and, to a certain degree, the conclusions of his predecessors. The historical sections are rather unevenly successful: though the author makes a good case for some of his causal factors, his statement of those that originally simplified the participle system in Slavic leaves the reader less than fully convinced.

Despite the excellence of the work as a whole, there are some points, mostly peripheral, that require comment. First there is a general question of style: to whom is Shevelov addressing his work? If, as one might suppose, it is primarily to Western linguists with an interest in Slavic, the author should have provided glosses for quotations in languages presumably unknown to his readers. While a knowledge of Russian and Old Church Slavic is probably to be assumed in them, and a knowledge of Polish, Czech, and Serbian is not unlikely, a knowledge of White Russian, Ukrainian, Slovak, Slovene, and Bulgarian is surely far from probable. (Still, it may be unreasonable to expect a work published by the Free Ukrainian Academy of Sciences to treat Ukrainian as an exotic language.)

As for content, one might wish that in the list of attributes of the participle, drawn from several grammarians (7), Shevelov had been more specific in telling us just what each of these grammarians is describing. Is it the participle as a general linguistic concept? the participle in a system assumed for Indo-European? the participle in Proto-Slavic? One has no right to assume that all of these are identical.

The author contends (8) that in Ukrainian the participle in *-lyj* was formerly little used, but was subsequently revived and is now coming to be more widely employed. The basis of this statement, if I understand his arguments (29) correctly, is approximately that if the active participle had not disappeared or nearly disappeared, the past passive participle of Common Slavic would not have taken on its universal character (i.e. undetermined as to voice and tense) in Ukrainian. But there are valid objections to this inference. It is questionable that the deverbative in *-lyj*, in Ukrainian or Modern Russian, can be described as either active or participial. In modern Russian and Ukrainian, an essential criterion for calling any form participial is that similar deverbatives form a class which in some of their occurrences are associated with the accusative of the direct object or with the instrumental of agency; while the basic criterion of activeness is government, in at least some cases, of the accusative of direct object. (Intransitive verbs need be classed with active verbs only because they show certain formal or

functional similarities.) Now the forms in *-lyj* satisfy neither of these criteria. *Mutatis mutandis*, one might argue that if transitive verbs form only participles in *-tyj* or *-nyj* and intransitives only participles in *-lyj*, one is justified in regarding the second form as supplementing the first. But this view cannot be correct: the author proves that *-nyj* and *-tyj* forms are used with intransitive stems. The evidence indicates simply that the historically articulated deverbatives in *-lyj*, at some point in their history in Russian and Ukrainian, lost their transitivity and hence their participial character, and have never regained it.

A reviewer is prone to attach undue weight to those sections of a work that touch upon his special interest; in this case that is the Russian participle and gerund system. What the author treats more or less in passing, will therefore occupy a prominent place in this review. However, since the author follows generally accepted grammatical tradition and since this needs revision, it may be well to discuss the author's statements.

In listing for Russian a number of deverbatives in *-myj* and *-nnyj* (or *-nyj*) formed from intransitive stems (16), the author draws on the works of Buslaev, Potebnă, and Vinogradov. Following these models, he allows himself to fall into an atomistic approach; indeed, this section recalls Nekrasov and Aksakov, who quote such grammatical data to prove that tense and voice do not really exist in Russian, and take delight in exhibiting the wonderful untidiness of their language. Commenting on Vinogradov's statement that many passive participles have lost their passive meaning and that morphological correlation between the forms of the passive and the active is half obliterated, Shevelov speaks in a similar fashion:

Certainly it is not easy and often it is quite impossible to determine the limits of these phenomena in reference to the participle in the Russian literary language, in which the old written-language traditions, often Church Slavic from various epochs of its use, have become mixed with the phenomena of the evolution of the living Russian colloquial language. One must simply set up an alphabetical list of the corresponding verbs and participles.

It is one thing to state the historical factors that lie behind the system of participles in Russian, but quite another to suppose, because of these factors, that literary Russian is composed additively of several systems. An alphabetical list of corresponding verbs and participles would certainly be useful, but as a preliminary rather than a final step in description; in particular, it would help us to determine what items are participles and what items are not participles though perhaps etymologically derived from participles or formed of homonymous morphemes.

In discussing (21) tense contrasts, or the lack of them, in the passive participle system, the author quotes the following sentence from Vinogradov: 'The meaning of time preserves its expressiveness only in the related forms of participles in *-myj* and *-nnyj* from bases of the imperfective aspect.' Shevelov adds:

This is quite seldom. In Modern Russian sentences of the type *Kora, svetom ne pronicaemaâ, pokryet tvoi oči* (Radišev, *Putešestvie: Spasskaâ Polest'*) are now impossible 'since the passive participles have almost completely lost

their distinctions of time' (Roman Jakobson, *Zur Struktur des russischen Verbs*, *Charisteria Mathesio* 82 [Praha, 1931]). If we can thus accept the 'universalization' of time, we can likewise accept the 'universalization' of voice.

This can scarcely be correct. Shevelov has reversed the meaning of the scholars whom he is quoting. Present passive participles can, in fact, be freely formed from most productive imperfective verb types, whether prefixed or not. It is the past passive participle which is restricted to the unprefixed imperfective types. Istrina gave the first accurate account of the imperfective present passive participle in respect to its productivity (*Leningradskij gosudarstvennyj pedagogičeskij institut im. A. I. Gercena, Učenyje zapiski, Kafedra russkogo äzyka* 20; 1939); in the course of her article she showed not only that the formation of the present passive participle is possible from most productive types of imperfective, but that such forms are now increasingly used. In Panova's *Kružilica* there is the sentence *Klaviä pošla vverx na lestnice, soprovoždaemaä ženšinoj v očkaz*; and there are similar examples in Virta's play *Zagovor obrečennyx* and in Oleša's *Zavist'*. The point that Vinogradov and Jakobson are making is that past passive participles are not normally formed from imperfective prefixed verbs and that tense contrasts in passive participles are therefore normally limited to unprefixed imperfective verbs. In Shevelov's defense it must be admitted that traditional descriptions of Russian (including Ušakov's in the *Tolkovyj slovar'*) tend to underestimate the productivity of present passive participles.

Such minor faults do not seriously impair the considerable positive contributions of Shevelov's work, or detract from its merits. These are, above all, originality of approach, variety of material, and liveliness of style.

The semantic spectrum of the Russian infinitive. By A. G. F. VAN HOLK. Pp. xii, 110, with folding chart. Leiden: A. W. Sijthoff's Uitgeversmaatschappij N. V., 1953.

Reviewed by FRANCIS J. WHITFIELD, *University of California, Berkeley*

The author, who has written a special study on the 'Gesamtbedeutung' of the actor-infinitive construction in Russian, *Word* 7.136-43 (1951), here extends his investigations to cover all the uses of the Russian infinitive. The structure of the 'semantic spectrum' is—in Roman Jakobson's terminology—the 'Hierarchie der Sonderbedeutungen'; the work is described as a first attempt to establish the principles according to which such a structure can be ascertained. In setting the problem, the author states (1): 'Just as a phoneme, i.e. an ensemble of simultaneous distinctive characteristics, is susceptible of various contextual differentiations, the total of which may be said to constitute the *phonic spectrum* of that phoneme, so any morpheme-content (which is defined by its relative status in some system) is susceptible of various contextual differentiations which together make up the semantic spectrum of that morpheme.' In Hjelmslevian terms, which I think can be used here without distortion of the meaning, the task is one of substance-analysis—specifically, analysis of content-substance.

Since the author's primary aim is to demonstrate the application of a pro-

posed methodology, a fairly large part of the book—the first three chapters—is quite properly devoted to general questions of linguistic structure and to principles of linguistic analysis. It is only a pity that more space was not available to make clear the author's premises, because at times he has evidently been forced to compress his statements to the point of real obscurity. For example, it is certainly confusing, at the very outset of the first chapter (7), to be referred to Hjelmslev for the meanings of the terms 'form', 'substance', 'content', and 'expression', and then, in the very next sentence, to be told that 'the minimal forms of expression are the *distinctive features*' (author's italics, with a reference to Jakobson, Fant, and Halle). Such an implied identification of units that have been reached by very different methods of analysis surely requires some supporting argument, if any could be found. As it stands, it only serves warning on the reader of possibly shifting ground ahead.

The second chapter reviews the distribution (defined as 'the totality of the possible employments') of the Russian infinitive. Together with examples of the several constructions in which the infinitive appears, there is discussion of word order, intonation contour, productivity, stylistic color, and other features that will later be used to discover the hierarchy of meanings. And here the conscientious reader may find himself held up for lack of adequate preparation in the first chapter. What basic principle of analysis is the author assuming, for example, when he says (45): 'A few nouns constitute a semantically indivisible item with a verb, e.g. *davat' slovo* "promise"?' Certainly a semantic division of this 'item' is not so far-fetched but that the author might have explained why he considers it inadmissible. The fact that it can be translated by a single English word is not, I suppose, meant to justify the assertion, but it would be easy to carry away that impression. Again, in what sense is the term 'intransitive' being used when the infinitive in a certain construction is said (63) to acquire 'a slightly intransitive meaning'? Here also, a persuasive TRANSLATION has been given ('the window must be opened' for *okno otvorit'*), but the author's gradations of transitivity remain unclear and I find no help in the comparison with *okno otvorjaetsja* 'the window is being opened'.

The last chapter is by far the clearest in its statement of the principles by which the results of the analysis are to be ordered (89): 'one starts from the assumption that every grammatical form has a well-defined form of content (sememe) which, in a given set of positions or contexts, gradually shifts from a maximally conform ("central") variant to a minimally conform ("peripheral") variant.' General rules are laid down for ascertaining relative peripheralness, and with their help the material collected in the preceding chapter is projected on a two-dimensional diagram that is meant to reveal the sought-for structure of the semantic spectrum. The reader's satisfaction with this final result will inevitably depend in large measure on his evaluation of the preliminary analysis and of its underlying assumptions. These latter, as I have tried to indicate, are not always easy to discover; but despite this drawback, the author shows real ingenuity in 'expanding the realm of form at the expense of the realm of substance' (110), and his short study furnishes many suggestions for an orderly attack on the problems of linguistic content.

Ukrajins'ka mova ta jiji stanovyšče sered inšyx slov'jans'kyx mov. By P. KOVALIV. (Slavistica, Proceedings of the Institute of Slavistics of the Ukrainian Free Academy of Sciences, No. 20.) Pp. 48. Winnipeg: M. Shashkevych Society, 1954.

Reviewed by HORACE G. LUNT II, *Harvard University*

Ukrainians justifiably resented both the tsarist legal measures aimed at keeping the Ukrainian language out of public life, and the scholars (such as Sobolevskij) whose works furnished a 'scientific' justification for the use of only Great Russian. The constant use of the epithet 'Little Russian' doubtless grated on Ukrainian sensitivities, even though many users of the term may have intended no slight. But in spite of the political attitudes of officialdom and some scholars, informed scientific opinion from the time of Miklosich (*Vergleichende Lautlehre*, 1852) has treated Ukrainian as a separate language, and indeed the Imperial Russian Academy bestowed on Hrinčenko the Kostomarov prize (1905) for his fundamental *Dictionary of the Ukrainian language*. The Soviet regime, with all its centralist tendencies, the frightful purges of 'nationalist deviationists' (among whom were numbered the outstanding Ukrainian scholars), and its insistence on keeping Ukrainian as close to Russian as possible, has never suggested that the language was not a separate entity. It is thus a surprise to find in Kovaliv's essay an English epigraph, attributed to an American and dated 1952: 'The claim of Ukrainian to the status of an independent language is almost realized. Differences between it and Russian have always existed, and linguists should give it due attention.' But these irresponsible sentences set the tone of the booklet: this is a political pamphlet rather than a serious linguistic study.

Kovaliv is less interested in a discussion of linguistic facts than in demonstrating that Ukrainians are not and have never been Russians. He thus typifies certain nationalistic attitudes which this reviewer has run into repeatedly in various areas of the Slavic world, but which remain incomprehensible to him. Why MUST a separate nation have a separate language? (The reviewer considers his native western American dialect a variety of English, but in no way feels that this changes the reality of his American nationality: both attitudes are subjective and are unlikely to be modified by any sort of quasi-mathematical argumentation. Conversely, the multilingual Swiss and Belgians seem to consider themselves nations.) In this particular case we have no quarrel with the accepted thesis that Ukrainian is now as separate and independent as any modern language which is almost surrounded by closely related languages. But the question is inextricably bound up with another: granted the independent existence of a separate language in the present, why MUST this entail a separate existence a thousand years ago?

The expansion of the Slavs was a long process (500 years? 900?), and as the area grew, so did dialect differences; but still the linguistic unity was such that innovations could affect the whole area or very large sections. Not until the 10th century were geographical distances sufficiently complicated by political circumstances to impede the transmission of many such impulses, but even then the dialects continued in many ways to follow parallel paths of development.

Geographical and political proximity meant that in the east the diffusion of new developments or simply the common paths of development continued for a longer period than elsewhere, so that it is only with the greatest difficulty that we can distinguish in the numerous East Slavic texts of the 11th and early 12th century features which are identifiable as northern and southern. It is not until about the middle of the 12th century that it becomes fairly easy to classify relatively short portions of text on this basis.

This is entirely in keeping with the nature of the information supplied by comparing the modern East Slavic dialects. No feature which is characteristic of all Ukrainian dialects and not found elsewhere is demonstrably older than the loss of the Proto-Slavic reduced vowels. The disappearance of these ubiquitous jers has been called the last event of Proto-Slavic, for the process itself is the same throughout the Slavic world; but the times at which it happened varied from south to north and the phonemic adjustments which resulted in different areas produced truly new linguistic structures. In East Slavic, the manuscript evidence unmistakably identifies the time of the loss of the jers as mid-12th century, and at once one of the most specific of Ukrainian features appears: the compensatory lengthening of *e* in a new closed syllable produces a diphthong *ie*, identified by the scribes with the phoneme represented by the letter *ѣ*. It is more difficult to date morphological innovations, but such structurally important developments as the single adjectival declension (almost identical with the pronominal declension) are clearly common East Slavic.

To this reviewer these facts tell us nothing about how the speakers of these ancient dialects felt in terms of group loyalty: the data affirm neither the thesis that the eastern Slavs were one tribe (or nation) nor the separatist view. They do not explain the linguistically tinged conflicts between Kiev and Muscovy in the 17th century. And most emphatically they do not deny the modern Ukrainian language with its vigorous national literature in the last two centuries.

Most of Kovaliv's booklet is devoted to a fumbling, inconclusive review of some of the many theories about Proto-Slavic unity and the chronology of the emergence of the separate Slavic languages. The presentation emphasizes efforts to glean from isolated statements some demonstration of the prehistoric existence of both Ukrainian and Byelorussian and thus to deny the validity of the concept of East Slavic. But when faced with fundamental linguistic features unmistakably common to all East Slavic dialects, Kovaliv (like the writers he favors) reacts with the flat assertion that they developed independently in the different languages—just how, he does not suggest.

It is only the final ten pages which seem to justify the title of the booklet, for here Kovaliv turns specifically to the problems of Ukrainian in its relation to the other Slavic languages. Kovaliv's method is primitive, his facts are shaky, and the results are meaningless. Citing the Ukrainian development of seventeen Proto-Slavic phonetic features from an earlier biased study by a Ukrainian, he makes a table illustrating their presence or absence in the other Slavic languages. (By lumping the two Sorbian languages into one and omitting Macedonian altogether, Kovaliv treats these minority languages in precisely the way that careless or prejudiced writers have treated Ukrainian.) He then comes up with

totals which are supposed to show degrees of relationship; but even these loaded figures demonstrate the especially close bonds between Ukrainian, Byelorussian, and Great Russian, and thus the validity of the term East Slavic.¹ Aside from the standard prehistoric isoglosses (**tj*, **tort*, etc.), the choice of features is inept. For example, item 4, described as the retention of PSI. 'soft *č*', item 5, PSI. 'semi-palatal and palatal' consonants hardened before *e* and *i*, and item 6, the hardening of labials in all positions, all reflect one set of problems: with the loss of the jers and the concomitant dislocation of the whole phonemic system, what features lost or assumed phonemic value? Ukrainian obviously took on the distinctive feature of sharpening (phonemic palatalization), using it somewhat differently than Russian or Byelorussian; Czech apparently developed sharpening, only to lose it about the 15th century; but there is no convincing evidence that the Serbs ever developed it. It is meaningless when Kovaliv contends that the 'hardening' of specific consonants (or consonants in certain positions) is the same in Ukrainian, Czech, and Serbian. The important thing about 'PSI. *č*' is that Ukrainian developed the opposition of plain vs. sharpened in the sibilant affricates, unlike either Russian or Byelorussian. But further specific discussion of the supposedly scientific details noted by Kovaliv is pointless here; the review would have to be far longer than the pamphlet. Suffice it to say that he omits pertinent facts large and small, he confuses different types of evidence, he seems ignorant of the standard literature and of the facts of Slavic languages other than Ukrainian, and he appears totally oblivious of the entire field of dialect geography.

It is a pity that this nicely printed booklet adds nothing at all to the body of knowledge about Ukrainian or its position among the Slavic languages. In forty pages it would have been possible to cite and discuss the concrete linguistic features (16 phonetic and 19 morphological) given by Nikolaj Durnovo as characteristic of all dialects of Ukrainian,² and at least to mention the 28 features that he lists as common to all three East Slavic languages (100-6). A concise critical examination of these points, with special reference to their value as isoglosses from various periods of the long history of the East Slavs, would be an excellent introduction to the history of Ukrainian. The value would be vastly increased if the discussion were couched in phonemic terms.

Nationalist persecutions have in the past hampered and often interrupted the work of Ukrainian linguists. It is precisely because of this and because of the inaccessibility and obsolescence of much that did get published that Slavists need

¹ Kovaliv's ridiculous oversimplification of the problem is easily demonstrated by reference to a serious (though by no means unassailable) attempt to classify these languages on a quantitative basis. J. Czekanowski, using some forty features (nine of them morphological) subdivided to make a total of 89 separate items, operates with complex mathematical formulas and several elaborate charts in his essay, *Różnicowanie się dialektów prasłowiańskich w świetle kryterjum ilościowego*, *Sborník prací I. sjezdu slovanských filologů v Praze 1929* 2.485-504. He comes up with the traditional E-W-S division, but with various shadings, particularly in West Slavic. And even Czekanowski can be accused of oversimplification, for he did not include enough features to show the differences within South Slavic.

² N. Durnovo, *Vvedenie v istoriju russkogo jazyka* 161-4 (Brno, 1927). Durnovo specifically states in a footnote (100) that he numbers the items for convenience only, and expresses doubt as to the validity of any sort of numerical scale of linguistic relationships. He is also careful to point out that the features he lists are of different orders and values.

to be furnished with reliable information about Ukrainian. It is to be hoped that the Canadian branch of the Free Ukrainian Academy of Sciences will waste no more time on unproductive polemics, but will present to the interested scholarly world the facts of the Ukrainian language, with its dialects and history, in the form of documented, objective studies worthy of the dignity of such an organization and of its responsibilities to Ukrainian culture.

The Norwegian language in America: A study in bilingual behavior. By EINAR HAUGEN. (Publications of the American Institute, University of Oslo, in coöperation with the Department of American Civilization, University of Pennsylvania.) Vol. 1. The bilingual community, pp. xiv, 317; Vol. 2. The American dialects of Norwegian, pp. vii, [377]. Philadelphia: University of Pennsylvania Press, 1953.

Reviewed by MARTIN JOOS, *University of Wisconsin*

This is a magnificent and epoch-making book. Its only weakness, for us, is in the title: some linguists may neglect it, supposing it to be of interest principally to Norwegians and their descendants and intimate friends. It is true that that audience has been admirably served here: we are told where the Norwegian immigrants came from, and when they came here (principally in the latter half of the 19th century), what problems the new world confronted them with, and what they have done about those problems, generation by generation, down to the present. Every significant feature of immigrant history and adjustment has been dealt with in this book; and it is shown how every conflict and every adjustment, from the most superficial externals of terrain and climate inward to the intellectual and emotional functioning of communities and individuals, is duly formulated in the patterns of their speech. Indeed, for those readers who are innocent of any special interest in language, it will be a revelation to see how the linguistic viewpoint, better than any other, brings order and clarity into the natural history of cultural adjustment.

The subtitle indicates a limited interest to linguists. The hint is far too modest to fit the facts. Here we find both documentation and discussion of what happens when two linguistic systems come into competition, probably fuller and more illuminating than anything else in print. This area of conflict between competing languages is an area which at least every historical linguist needs to be thoroughly familiar with. In the past, common sense has saved the wiser historians of linguistic change from serious blunders like talking about 'mixed languages'; in the future, it seems likely that any statement about a linguistic substratum, for instance, or about loanwords, will have to be kept in harmony with the results of Haugen's researches in order to gain a sympathetic hearing.

Not only the historical linguist, but everybody who needs insight into the nature of language can profitably come to this book for it. If he knows no Scandinavian, he may skip about a quarter of the matter in the first volume and in the first half of the second; the latter half of the second volume consists of seventy pages of specimen texts, a fifty-page selected annotated vocabulary of English loans, and the Appendixes and Index. But even without knowing Norwegian

the discussion is easy to follow, and it is never made necessary to take the author's interpretation on faith.

Previous studies of bilingualism and of immigrant speech have been combed for insights and illustrations, for comparisons and for help in establishing the general rules developed in this book. The bibliographic references seem to leave nothing unnamed that we might want to read for more facts and theories in this field. But the bulk of the book is an original collection of material and an independent analysis thereof, an analysis that is linguistically complete but never neglects extralinguistic factors. Here are a few of the chapter-headings and scraps of the contents:

1. The bilingual's dilemma: 'Only by observing closely the behavior of bilinguals, and giving them the same kind of detailed and objective study that other speakers have received can we draw valid conclusions about the theories that have been advanced to account for the many strange phenomena of interlingual imitation' (11).

4. The confusion of tongues: 'The contemporary poet Herman Wildenvey, who spent some time among his countrymen in Brooklyn, refers to their speech as "a linguistic crazy quilt." He specifically stated that "usually there are no laws of language among the city dwellers here." The laws he appears to be thinking of are the laws of rhetoric and style which are applicable to well-established standard languages in the centers of modern civilization. They are certainly not linguistic laws, for as we shall see, there are underlying regularities in the behavior of bilingual speakers which determine their linguistic expression, and which certainly deserve the name of laws if any statements about language do' (57-8).

5. The great vocabulary shift: '... of 32 words connected with the harvest in Norway, only 17 were carried on into America. Two of these were shifted in meaning to agree with English analogues, while two others were already so close to the English that they could hardly have been changed. Thirteen new English words were adopted, making a total of 30 words, or nearly the same as in Norway. Of these we may say that 15, or exactly 50 %, are importations from English ... By this change the distance from Norwegian to English was shortened by one half. But the cultural shift was actually complete, for the English and the American Norwegian vocabulary structure in this area was now identical, with a one-to-one correspondence between them' (95). Exact semantic matching is meant.

14. Bilingualism and borrowing: 'Terms for streams of all kinds abound in Norwegian, but none of them fit exactly the distinction of *creek* from *river* which is characteristic of midwestern conditions' (377). Both words are now the normal American Norwegian words for those kinds of streams, and Norwegian *bekk* and *elv* are all but forgotten.

Chapters 15-18 give a technical analysis of the affected vocabulary too full to be sampled here, under the titles The process of borrowing, The phonology of loanwords, The grammar of loanwords, and Native forms for foreign: Loanblends and loanshifts. Chapters 15 and 18 present the most thoroughly thought-out classification and elucidation of imitative vocabulary development that we

are likely to see, and the technical terms and their definitions will probably become standard, at least among American linguists.

But the ultimate value of this book lies not so much in the data, the analysis, and the systematic terminology as in the chance it gives us all to deal thoughtfully for a while with a large and much-neglected area of normal language behavior and thus to round out our education in the nature of human speech.

Isländisches etymologisches Wörterbuch. By ALEXANDER JÓHANNESSON. Fasc. 2-5, *uel-* to *syep-*, pp. 161-800. Bern: A. Francke AG. Verlag, 1952-54.

Reviewed by KEMP MALONE, *Johns Hopkins University*

The first fascicle of this book was reviewed by me in *Lg.* 28.527-33; there the reader will find a brief general description of the work. The present review is limited to details of one kind or another. The number set at the head of a given comment or group of comments stands for the page of Jóhannesson's book to which the comment applies.

161: The abbreviation *Vols.* presumably stands for *Volsapáttir*, but only those familiar with the story will understand the reference; the others will take it as short for *Volsungasaga*, and look there in vain. ON *Volsi* (weak) answers to OE *Wæls* (strong), a name which the author fails to mention here; see Björkman, *Eigennamen im Beowulf* 113 ff. 162: English *weld* (earlier *well*), OE *wellan*, *wiellan*, the causative of *weallan* 'boil', is a native word; cognate with Icel. *vella*. The OE cognate of Icel. *vella* is *wiellan*, Anglian *weltan* (not *wealtan*); it has weak inflection (not strong). The author seems to have confused it with OE *wealcan* (7th class strong). 163: OE *wealt* should have been marked with an asterisk, or its modern continuation *walt* should have been cited instead. The negative *unwealt* 'steady' does occur in OE texts.

164: The postulated 'germ. wz. *welep-* in me. *wlappen*, *wrappen*' does not exist. These forms are blends of *lappen* and *wrien* (OE *wrēon*). Under *uel-* the Swedish dialectal *öl* 'knochenspröde' (Holthausen) might have been mentioned along with OE and OS *wōl*; note also OE *bewēled* 'polluted.' On the etymology of *Váli* see *Lg.* 28.532 and my *Lit. hist. of Hamlet* 169 ff. 167: For *wastling* (line 3) read *wæstling*. Under *ues-* might have been listed ME *wase*, which glosses Latin *stūpa*. 168: One is startled to find the *Weder-* of OE *Wedergeatas* (in *Beowulf*) equated with Icel. *Viðurr*. This connection will hardly win much favor. 171: Under *yor-* the author mentions OE *wōri[g]an* but not the element *wōr-* 'wandering, wild' found in the names of the male and female pheasant: *wōrhana* and *wōrhenn*. Here 'wild' is opposed to 'tame, domesticated.' A south Gmc. form answering to Icel. *ylgr* 'she-wolf' is the element *vulg-* in the *Vulgares* of Paulus Diaconus; see my ed. of *Widsith* 199 f. and *MLQ* 1.37 ff.

174: For *stō-ðar* (line 4) read *siððan*. One wonders how the author got the idea that 'engl. *whore* ist nord. lehnwort.' 175: Under *kaito-* the *Chaideinoi* of Ptolemy's *Geography* ought to have been mentioned; see A. Noreen, *Altisl. Gram.*⁴ 216 and my *Widsith* 154. Under *qailo-* we are told that 'engl. *hale* "gesund" ist lehnwort aus dem nord.' In fact, of course, it is the north-country continuation of OE *hāl*. 178: Under *qaul-* the author gives OE *holh* but not

its modern continuation *hollow*. The form *holc* that he gives as a derivative of OE *hol* 'hole' should be *holoc*; see M. T. Löfvenberg, *ME surnames* 105. An English cognate of the Icel. adj. *hagr* 'fink, behende, geschickt' is the *-haga* of OE *salthaga* 'robin' (lit. 'one who is good at hopping'). 179: Under *kak-* the OE plural *hagan* is given as a gloss to Latin *genitalia*. This plural in fact glosses *quisquilia* and *gignalia* but not *genitalia*, so far as I can discover. Note Toller's comment in the BT *Suppl.* under *haga* 'a berry'.

181: Here and often elsewhere the author takes up words that seem to be unaffected, or only partially affected, by the sound-shifts that usually apply. Thus, under *kakka-* are listed not only Greek *kakkāō*, Latin *cacāre*, etc., but also Icel. *kúka* 'defecate' (with its noun *kúkr*), Danish *kakke*, German *kacken*, etc. If the Germanic words really go back genetically to IE *kakka-*, they managed somehow to resist the Germanic consonant shift. The author does not concern himself with the problem beyond saying that a word of this kind is a 'kinderlallwort' (an assertion rather than the statement of a fact). But under *qha qha* (IE for 'laugh') he tells us that Icel. '*há* interj., dän. schw. dt. engl. frz. *ha*, entspricht lat. *ha*.' Here the Germanic sound-shift worked with a vengeance, affecting not only the Germanic tongues but Latin and French besides! Under this same root are listed Indic *kakhati*, Greek *kacházō*, Latin *cachinnō*, and OE *ceahhettan*. Here Grassmann's law worked, and the Gmc. sound-shift worked in English for the second but not for the first stop! Elsewhere (402, 449) the author speaks of 'stockende lautverschiebung', and under *blē* (594) he explains: 'im germ. unterblieb die lautverschiebung wegen steter neunachahmung'. But 'Idg. *pātēr-* scheint vom lallwort *pa(pa)* ausgegangen zu sein' (533) and yet the Gmc. sound-shift worked beautifully here. It is all very puzzling.

183: Under *qat-* are listed not only words like *haðna* and *heðinn* but also *køttr* 'cat' and its cognates, which the author explains as seemingly 'aus dem kelt. ... übernommen'. Since we have no reason to think that this group of words is of IE origin, it would seem doubly improper to list them under an IE root. 184: Under *kad-* are listed Icelandic *hjaðna* 'sink' and Shetlandic *hjada* 'carcass, carrion' without a word on the phonological difficulties of the connection. This is no way to etymologize. 188: The author here fails to note that English *give* was taken from Scand. 197: Under *kei-* the author takes up *hý* 'fluff' but not *ský* 'cloud', although here if anywhere the prefixal or movable *s* would seem in place. 199: Under *keid-* we find Icel. *hitta* and English *hit* presented as cognates. It seems more likely that the English word was taken from Scand. 200: Under *keu-* the cognates of Icel. *húnn* 'knob, door-handle, top of a masthead' and its homonym *húnn* 'bear cub' are given, the two words being treated as one. The English cognate *hūn-* is presented as a simplex but it in fact occurs only in composition. English *hound*, the nautical term, was taken from Scand. The *-hound* of *horehound* (OE *hūne*) has been connected with OE *hūn-* 'stick, pole, mast' but the etymology is a doubtful one. Some connect with Icel. *húnn* 'bear cub' the element *Hūn-* found in many Germanic personal names, but see MLN 57.132.

201: English *sheen* is rightly marked 'veralt.' as an adj. but it should have been noted that the word is still current as a noun. 202: For *hight* read *height*.

205: Gothic *hugs* should have been marked with an asterisk; only the gen.sg. *hugis* occurs. The author follows the etymology of Johansson. 206: To the etymologists listed under *Hoenir* should be added Schröder (*PBB* 43.219 ff.). Under *geud-* the author has left out Icel. *skúta* 'taunt, scold, chide', though he mentions its English cognate *shout*. 208: The English cognate of Icel. *hengja* 'hang', if it existed, would take the form **hengan* in OE, but no such verb is recorded and ME *hengen* is commonly explained as taken from Scand. A noun *hengen* 'hanging' does occur in OE. 209-10: Under 3. *kem-* the OE proper name *Hemning* (Beowulf 1944) should have been listed; see my paper in *Anglia* 63.104 f. By inadvertence Jóhannesson has left out Icel. *hóm* 'haunch' in this entry, though he mentions it (238) in another connection. 211: Along with LG *hamme* 'umzäuntes feld' should have been listed OE *hamm* 'enclosure'.

211-3: Under 1. *qen-* modern English *neck* is not given, though OE *hnecca* duly appears. OE *hnoll* means 'scheitel', not 'schädel'. OE *hnocc* glosses Latin *mutinus* 'wether'. The medieval archery term *nock* answers to Icel. *hnokki* 'hook' rather than to OE *hnocc*. Note however OE *gehnyncned* 'wrinkled', which goes well with *nock* in its later sense 'notch'. 213-24: Under 2. *qen-* are entered a large number of words, not always in orderly fashion; thus OE *fnora* 'sneezing' appears twice within a few lines (214-5). The English cognate of Icel. *hnefi* is commonly spelt *nieve*; the word is now archaic. To OE *hnappian* 'slumber' answers the modern *nap*. The author wrongly traces Icel. *hnoss* 'treasure' back to Gmc. **hnudpi-* (219); instead of *-dp-* he should have given pre-Gmc. *-tt-*, whence Gmc. *-ss-*. We do not know just how the *tt* became *ss* in Gmc. but we may safely presume that the change did not come about by way of *dp*. Twice (219 and 220) the author records a non-existent OE verb *āhnēopan* 'pluck (off)'. The proper form is **āhnēapan*; the preterit is on record as *āhnēop* (Guthlac 847), and the verb therefore belongs to the 7th class strong, though Clark Hall by mistake marks it 2d class in his *Dictionary*. It will not do to say that 'engl. *snub-nosed* ist nord. lehnwort' (220), though the verb *snub* was certainly taken from Scand. The verb formerly might mean 'shorten, curtail', whence the compound adj. *snub-nosed*, a strictly English formation. Note that *nib* in the sense 'pen-point' is still in widespread use (222).

224: OE *hunta* 'jäger' survives in the surname *Hunt*. 225: For *earwunga* read *earwunga*. 228: For *raindeer* read *reindeer*. 232: English *screech*, earlier *scritch*, is short for OE *scriccettan* 'creak, grate' (not Scand. in origin). The author here missed the verb he was presumably seeking: *scream*, clearly Icel. *skrækja* in English dress. 233: For *scræf* (line 6) read *scræb*; other OE names for the cormorant that might have been mentioned here are *scealfor* f. and *scealfra* m. 234: A number of words given here had already been given a little earlier (232); the repetition serves no reasonable purpose. Icel. *hraumi* and *skraumi* answer to OE *hrēam* and modern English *scream* respectively but the modern word is not mentioned. 234-5: A number of Germanic words beginning with *kr* are listed here, apparently by mistake; certainly they do not go back to the IE root *kreu-* under which they are set. 237: Icel. *hirðir* 'herdsman' has modern English *herd* for cognate; this word occurs both as a simplex and in such compounds as

shepherd (which Jóhannesson mentions), *goatherd*, and *cowherd*; the earlier *neat-herd* is obsolete.

238: Under *ker(s)*- the author puts Icel. *herstr* 'bitter, rau, barsch' etc. but these words can hardly be kept apart from Lith. *keřštas* 'wrath' and the IE *k* was therefore not palatal. For *fischsuppe* read *fischschuppe*. 240 (line 25): For *hléowe* read *hléow*. 245: Icel. *hildingr* 'fighter' answers to English *hilding* 'vicious horse' (later 'good-for-nothing beast or person'); it is hard to say whether the English word is native or of Scand. origin. 247 (bottom): The author seems to think that OE *hlymman* and *hlimman* are merely variants of the same verb; in fact, of course, *hlimman* is a strong verb whereas *hlymman* is the cognate of Icel. *hlymjá*. 248: To *glam* etc. add ME *glam* 'talk, noise' and the tribal name *Glomman*, on which see my *Widsith* 150. 249 (line 28): For *hélian* read *hólian*. 250: OE *heord* 'hair' occurs only as the second element of a compound. 257: Under IE *quaq-* are listed a number of Gmc. words beginning with *k*, with the following explanation: 'Diese wörter sind nachahmungen des froschlautes und entengeschnatters.' If so, Latin *coaxāre* etc. are mere parallels and no IE root ought to be set up. 258: For *hweoperian* and *hweoperung* read *hwoperian* and *hwoþ(e)runɡ* respectively; the *eo* forms were mistakenly booked by Bosworth but corrected by Toller. 259 top: The *k* of *kvisa* etc. presumably represents an earlier *h*, as often in modern Icelandic; otherwise these words would be out of place under IE *kuei-*.

261: English *whimsy* is a noun, not a verb; for its origin see NED under *whim-wham*. 262: Modern English *wharf* so far as its phonetic form is concerned may go back equally well to OE *hwerf* or OE *hwearf*, and both these forms in fact occur. 263: Under *kues-* the author records an OE 7th-class strong verb *hwæsan* 'zwischen' which does not exist, and identifies it with the modern *wheeze*. He may have had in mind OE *hwōsan* 'cough', which is a 7th-class strong verb but which has no connection with *wheeze*, a word of Scand. origin; the author himself had earlier (259) connected it with dial. Swedish *hvisa* 'flüstern'. 268 (bottom): OE *hryþig* (not *hryþig*) is a variant of *hriþig*, a derivative of *hriþ* 'snowstorm' and unconnected with Icel. *hroði* 'offal'. 269: Under *greu-* add Icel. *hrymjast* 'grow old and feeble', and under *greut-* add Icel. *hreysta* 'hearten'. 270: English *roe* (of a fish) and *rown* should be kept apart. The latter is of Scand. origin (Icel. *hrogn*) but *roe* is a native word. Jóhannesson states that 'engl. *roe* ist aus *rown* entsteht (*n* wurde als pluralzeichen aufgefasst) aus me. *rowne*', but this will not do. In the dialectal area where *rown* was current, *n* did not serve as a plural sign, and in fact *rown* has *rowns* for plural to this day.

271: Under *qreg-* (*qerg-*?) the author gives an English *raggy*. He leaves the word unglossed but since he takes up *raggy* 'ragged' elsewhere (707) one must presume that here he had another word in mind. I have not found this other word and suspect his *raggy* of being a mistake for Shetlandic *rāgi*, found in the phrase *a rāgi shooer* 'a slight, passing shower'. The adj. *rāgi* is derived from Shetlandic *rag* 'thick; damp mist, very slight shower', a word connected with Icel. *raki* 'dampness, moisture'. If the author really had Shetlandic *rāgi* in mind, his *raggy* should be deleted, since it does not go back to a word beginning with *hr*.

Icel. *hregg* etc. are out of place under IE *qreg-* for another reason: their *gg*. To list these words without dealing in any way with so serious a difficulty is hardly a proper procedure. Shetlandic *rag* 'bully, ill-treat' (with a *g* going back to earlier *k*) might well have been entered under this root, however; certainly it is connected with Icel. *hrekja* 'plagen, quälen', as Jakobsen duly pointed out in his *Dict. of Norn*. Dialectal English *rag* 'annoy, torment' may conceivably have got into English from Shetlandic.

272: Under *qret-* against *hrotti* there should be a cross-reference to 231 top, where a very different etymology of this word is presented. There should also be a reference to my paper on *runt* in *Lg.* 20.87 f. 273: Under *qrom-* 'gestell aus latten, hölzerne umzäunung' the author puts Icel. *hrefni* 'unterste planke überm schiffsboden' and compares OE *hremman* 'hinder, cumber'. But this word cannot well be kept apart from Icel. *hremma* 'seize with the claws, clut., grab' etc. Compare also OE *hramma* 'cramp, spasm' and Icel. *hrammr* 'paw (of a bear)'. 274: It is hard to see the semantic link between *hlið* 'seite' and *hliðr* 'ochse, hirsch'. The author suggests, with a question mark, 'eig. der von der seite auf j-n losgeht', but castrated or timid animals would hardly take their name from any mode of attack. The author would have done better here to leave *hliðr* without an etymology; but his system of presentation forced him to put under some root every word taken up at all. 277: Under *qlæg-* the author puts words that belong to IE *klak-*; thus, OBulg. *klokotati*, Gothic *hlahjan*, and OE *hliehhan*. It would be better to keep the *g-* and *k-* words apart, even though the roots be taken to have the same meaning. 278: Under *qleno-* is listed OE *hlyn*; the form that occurs is *hlin*; if this is to be normalized, it had better be normalized in full, to *hlynn*. 279: Under *qlon-* is listed OE *hland* but not its modern continuation *lant*. 280-1: In his discussion of the Gmc. words for 'four' the author says that 'den germ. formen liegt *petvôr*, *petur* zugrunde (mit *p* statt *q* nach *penqe* 'fünf')'. Against this see my discussion in *MLN* 64.533 f.

285-286: The author, after listing *geit* 'goat' and related words, continues: 'Hierzu gehört kaum *kið* ... "junge ziege" ... Sicherl. onomatopoet. bildung, als lockwort.' In this way he manages to take up under *ghaido-* a group of words that do not belong there! Be it added that Shetlandic *kidi* means 'lamb', not 'kid'. The author here makes no mention of English *kid* and *kidling*. If we go by ME *kide* the English word is closer to Shetlandic *kidi*, in form at least, than to Icelandic *kið*. Under *ghaisos* should have been mentioned OE *ætqæru* 'spear' (see *Lund stud. in English* 7.148). 288: Under *gāb-* we find OE *cēpan* listed but not its modern continuation *keep*. 290: Under *ghan-* should have been listed OE *ganian*, modern *gane*. 291: Under *gang-* should have been listed OE *cincung* 'boisterous laughter'. 292: Under 1. *gal-* (*gol-*?) is listed Latin *calvus* 'bald' without a word on the phonetic difficulties involved. OE *sceallan* 'testiculi' may well belong here, though the author does not list it. 293: Under *gal-* 'rufen, schreien' the author puts OE *clacu*, glossing it 'kampflärm, streit', though it actually means 'injury' and goes with OE *clæclēas*, Icel. *klakklaust* 'uninjured' and with Icel. *klekkja* 'do harm (to someone)'. This group of words is of obscure origin; it would go better, I think, with *klakkr* etc. (366). In any case, the English cognate of Icel. *klaka* 'cackle' is not OE *clacu* but ME *clake* 'gabble'. The

corresponding nouns are Icel. *klak* 'cluck' and English *clack*. Note also OE *cloccian* 'cluck'. 294: Icel. *klappa* is rightly connected with English *clap* but the OE form of this word is *clæppan*, though a frequentative *clæppettan* (which the author cites) also occurs.

295: Among the words akin to Icel. *gaddr* 'spike' the author lists OE *gierd* but not its modern continuation *yard*. He should also have noted that English *gad* is of Scand. origin. 296-7: Under *ġēi:ġī* the author mentions OE *cīnan* 'gape, yawn, crack' but not its modern continuations *chine* (now obsolete) and *cheen* (current in Cornwall). He glosses *cīnan* 'aufspringen, keimen.' The first gloss may be allowed, but two dialectal words, Cornish *cheen* and Pembrokeshire *chime*, both of which mean 'sprout', furnish the only evidence I know of that OE *cīnan* might have this meaning. The corresponding noun, OE *cinu*, ME *chine*, early modern *chin*, died out in the 16th century. The verb took its place locally, the derivative *chink* generally. The author puts OE *cīan* 'gills' here (in the spellings *cēon*, *cīun*) but later gives the word a very different connection (390) without cross reference. Under *germ. kik-* he lists the English compound *chincough* (i.e. *chink-cough*) but not the simplex *chink*.

297-9: Under *gei-* the author puts *keipr* 'rowlock' and compares OE *cippian*, but this verb does not occur in OE as a simplex, though *forcippian* 'cut off' is on record. As a cognate of Icel. *kippa* 'pull, jerk' the author might have mentioned English *chip* 'trip up.' 300-5: Under *ġhēi:ġhī* appear Gothic *gailjan* 'erfreuen' and a group of related words (e.g. OE *gāl* and OS *gēl*); these are repeated (387) under *ghoilo-s*, with no indication that they had earlier been given a different etymology. English *gibe* 'spotten' (303) has affricate *g*, as the variant *jibe* shows, and it is therefore to be connected not with LG *gibelen* 'spottend lachen' but with OF *giber* 'handle roughly (in sport)'. The English words *gipe* 'glutton' (dial.) and *gasp* (304) were taken from Scand. English *gape* however (305) is a native word; it occurs in OE as the second element of *ofergapien* 'neglect' (i.e. yawn over one's work); the simplex first occurs in ME times. The group of words headed by *gabba* 'scherz treiben' (305) should go under the 'erweiterung *ghap-*' (304); here the author lists a non-existent OE verb *gabbian* (Bosworth enters *gabban* but Toller in his Supplement cancels the entry). It is hard to see how *gaddr* 'hard snow' etc. can plausibly be connected with 'germ. *gas-* "gähnen" ...' (305). Under *geid-* the English adj. *kittle* 'ticklish' should have been entered; it occurs only in the gradative phrase *kittle cattle* 'difficult people'. Like OE *citelung* 'tickling' it is probably of Scand. origin. The OE verb *citelian* which the author cites seems to be a ghost word.

306: Holthausen plausibly connects *kāð* 'unsittliches leben' with Latin *gaudeō*, Greek *gēthēō* etc. (Wb. 364). 307: The masc. *ia*-stem *kyllir* 'sack' is usually (and rightly) explained as taken from Latin by way of English; it answers with precision to Latin *culleus* and OE *cylle*. 308: If the author is right in taking the *kql-* of *kqlski* 'devil' for an alteration of earlier *koll-*, one may compare OE **colla* 'killer', found (in acc. pl.) as the second element of the poetical compound **morgencolla* 'morning killer'; note also its derivative the verb *kill* (OE **cyllan*). The author cites ME *colle* but this seems to be a proper name, a variant of *Colin*, in Chaucer at least. On Icel. *kárr* etc. see my paper in *English studies* 28.44 f.

310: With *koka* 'gulp down' and related words the author compares English *choke* but later (390) he gives *choke* another connection. Along with Danish *kok* 'heuhaufen' he should have mentioned English *haycock*. 311-2: English *kyte* 'belly' is misplaced; it goes with Icel. *kýta* 'maw of a wolf-fish' a dozen lines below. OE *cot* n. and *cote* f. 'cottage' should go together; the author separates them and adds a non-existent *cyte*. Another grade, with umlaut of *ēa* (Gmc. *au*) is shown in WSax. *cýte* (< **cīete*) and Anglian *cēte*, ME *chēte*; here the author gives *cýte* but not *cēte*. 313: The author gives OE *cýf* 'tub' but not its modern continuations *keeve* and *kive*. OE *copped* (not *coppod*) means 'polled' (not 'mit einem kamm versehen'). The sense 'crested' arose much later; it is still current in dialectal speech. 314: English *cob* has many meanings but the sense 'junges von tieren' is not given in NED or EDD. Related to Icel. *kúpa* 'butter-bowl' are OE *cýpa* m. and *cýpe* f. 'vessel, basket'. The author gives only the modern dialectal *kipe*.

316: OE *gyte* means 'flood', not 'wasserstrasse'. On the tribal name *Gautar* see *Namn och bygd* 22.34 ff. 318-20: With Gothic *gawi* goes OE *-gē* 'district', found e.g. in *Ēlgē* 'Ely' (lit. 'eel district'). The OE cognate of *geyja* 'bark' is *gēgan* 'cry out'. OE *earngeap* is misplaced; it goes under 'labialerweiterung *gheub-*' (320). Under *gheugh-* add dial. Swed. *gog* and Dutch *guig* 'fool'. 321: For *cloxae* read *coxae*. 323: Add OE *cost* to the cognates of *kostr*. English *cag* 'stump' must go back to an earlier **cagge* and thus differs in form from Swed. *kage*. 324: It is hard to see how German *gucken* etc. can go back to Gmc. *kag-*, *kug-*, even with a question mark. 325: For *geagl* (line 14) read *géagl*. 326: Under *geg-* add Icel. *káka* (*við*) 'tamper (with)'. 328: Add English *gat* 'opening, passage', a word taken from Scand. 329: Pace Sundén one cannot accept ME *chymble* as a Scand. word. 331: Add English *uncouth*. 334 top: Add English *knurl* 'knot, knob'. 338: For *ghædh* read *ghnædh*. It is not easy to see how Icel. *snati* 'dog' can be put under this 'erweiterung'. 339: OE *cnīdan* means 'beat', not 'rub', and is out of place under IE *ghnei-*. 344: English *get* was taken from Scand. and replaced OE *-gietan* in *beget* and *forget*. 346: Icel. *kjarka* 'schwatzen' has for cognate English *chirk* (ME *cherken*), the stem of which appears in OE *ceorcing* 'complaining'. Another grade is represented by English *chark* (OE *cearcian* 'creak'). 347: For *cracettan* (lines 11 and 29) read *crācettan*.

348: OE *gecrōcod* presupposes an etymon **crōc*, answering to Icel. *krókr* 'crook'. 349: *krá* 'ecke, winkel' has for cognate OE *crōh*, found in place names. The Vernerian variant, Gmc. **krang-*, plus *k*-suffix, is found in English *crank* 'bent or crooked object', recorded in OE as the first element of *crancstæf* 'weaving implement'. 353: The author here inserts a group of words going back (as he explains) to IE *ghrup-*. This group is out of place in the midst of words going back to IE *greu-*, and should have been put elsewhere. 354: English *crouch* 'bend low' is not a native word. 356: Add modern English *kernel*. 357: Under *gher-* might have been listed Latin *horīrī*. 360: Add OE *gyrl-* 'virgin' (in *gyrlgyden* 'virgin goddess'). Icel. *karpa* etc. (to which add OFris. *kerp* 'streit' and English *chirp*) are out of place under *gher-*; they should have been put under *ger-* (346). 366: Add ME *clak* 'tarry mark (on a fleece), spot, stain' and ME *cleken* 'cleek'

(variant of ME *clechen* 'cleach'). 367: Add modern English *chilver* 'ewe-lamb'. 369 bottom: After OE *clympre* add modern *clumper* (now dial.). 370 (line 9): For *climman* read *climban*. To OE *clamm* add modern *clam* 'kind of bivalve shell-fish'. 373: OE *colc* is a ghost word; the actual form is *-cole*, found only in compounds.

378: Here under 'wzf. *ghlei-*' and again under *ghleu-* (403) the author enters Icel. *glý* 'freude' and cognates without cross reference or other indication that he is in doubt. In both places OE *glēo* is listed but not modern *glee*. 382: The author is wrong in thinking that 'engl. *gloss* "glänzend machen" scheint aus dem nord. zu stammen.' The verb in this sense does not antedate the 18th century, and we have here merely a verb use of the noun *gloss* 'surface luster'. 385: For *gjelm* read *gielm*. 386: The term 'wurzel' seems strained as applied to *ghelunā* 'kiefer', a form which gives every indication of being a full-fledged word. 387: Add English *guild* 'zunft' (taken from Scand.). 390: It is customary to put *tyggja*, *tyggva* 'chew' under *gieu-* on the theory that it goes back, by dissimilation, to **kyggva*, but *toggla* 'champ, gnaw' may be explained plausibly enough as it stands, without recourse to dissimilation, contamination, or the like, and such an explanation ought to be given the preference. This verb goes back to Gmc. **lagulōn* (Holthausen) and belongs with *tagl* etc. (476). The author writes *toggla* in agreement with his etymology but this is not the usual spelling.

393: Add modern English *grit* and *groats*. OE *grēot*, ME *greet*, becomes *grit* by shortening of the vowel before final [t] in a monosyllable; compare *sprit* (OE *sprēot* 'pole'), *hot*, *foot*, etc. Modern *groats* continues OE *grotan* pl. 'coarse meal'. 395: Under *ghredh-* the author enters Gothic *griþs* etc., but adds that certain forms point 'unzweideutig' to Gmc. *grī-*, and later (401) he takes up this root, which, as he says, 'scheint idg. nicht bekannt zu sein.' Instead of setting it up for itself, however, he appends it to IE *ghren-*. 396: Under 1. *ghrebh-* add OE *gegræppian* 'seize', and ME *grapsen* (modern *grasp*) if *ps* is for earlier *fs* (by dissimilation). Under 2. *ghrebh-* add OE *grōf* 'ditch, sewer'. The etymological distinction which the author makes between Norw. *grov* 'natürliche aushöhlung in der erde' (355) and Norw. *grov* 'bach, flussbett' (396) can hardly be upheld. 397: Under *grem-* the author puts *korgr* 'dregs' and cognates, but he explains parenthetically that the group goes back to an 'erweiterte form von *ger-*, *gor-*' and an entry *ger-*, *gor-* should have been set up to take care of these words. 402: For *crás* read *créas*. 404: Add OE *clofe* 'buckle'. 408: For *cwīnan* (line 3) read *-cwīnan*. Note that OE *cwīsan* 'zerquetschen' cannot go back to pre-Gmc. *gweis-*, whether the *ȳ* represents earlier *īe*, as Holthausen thinks, or the mutation of Gmc. *ū*. Presumably the author took the *ȳ* for a variant spelling of *ī*.

410: English *quoth* means 'sagte', not 'sagen'. 415: Add OE *-gȳþ* 'war', used only in women's names. Words with initial *k* (*kvantr* 'leid' etc.) are out of place in this entry. 418: Add Gothic *warmjan*. 420: In connection with *kveld* 'evening' add OE *cwīldseten* 'first hours of night'. 422 top: Add OE *cwēad* 'dung, filth'. 422 bottom: Add OE *padde* (in *paddanīeg* 'frog island'). As for English *pad* 'bundle to lie on', this is a late (16th century) word, perhaps a blend of *pack* and *bed*. 424 f.: Under *tata-*, *tēta* the author brings together a number of words supposed to be onomatopoeic and therefore not subject to sound-shifts. Here it

is interesting to note that German *zitze* 'teat' underwent the regular High German sound-shift as if it were an ordinary word. OE *titt* survives to this day in many parts of the Anglo-Saxon world, though the literary word is *teat* (from French). With Icel. *tota* 'spitze' go OE *tōtian* 'stick out' (modern *toot*, now dial.) and modern *tootsy* 'protruding part, foot'. 427: For *byssa* read *-byssa*. For *mægenbysse* read *mægenþise*. 428: OE *geþyll* 'breeze' is entered here as a cognate of (*fimbul*)-*pul*; later (450) it appears under another root, as a cognate of *pula* 'rede, aufzählung', etc. 428 bottom: *tūli* etc. ought not to be put under an IE root beginning with *t*.

431: Under *teuā-* the author puts *þjótr* 'holzast oder stange, auf der etwas getrocknet wird', but this word in my opinion is identical with *þjótr* 'wind' (430) and *teuā-* may be dispensed with. The drying apparatus is the passive, the wind the active agent in drying clothes, nets, and the like, and a word applicable to one agent might readily be shifted to the other. Compare OE *þēote*, which may mean not only 'waterfall' (i.e. noisy stream of water) but also 'conduit, pipe' (i.e. apparatus for channeling a stream of water). Note that Icel. *þjóta* and OE *þēotan*, its cognate, both mean 'roar, howl', but *þjóta* applies especially to the howling of the wind whereas *þēotan* applies especially to the roaring of water. 432: For *twist* read *twish*. 435: The author describes IE *tet(e)r-* as a 'redupl. schallwort' but its descendant Icel. *þiðurr* 'auerhahn' shows by its phonetic form that the regular Germanic sound-shift took place this time. 439 bottom: Add English *tangle* 'kind of sea-weed' (from Norw.). 441: OE *þrūh* 'trough, coffin' has *þrīh* not only for gen. sg. but also for dat. sg. and nom. acc. pl. 442: The author ought to have explained how Icel. *þroka* and *þrok* can go back to IE *treuq-*. Under germ. *þresk-* add English *thresh*.

442-3: Under 4. *ter-* the author lists a non-existent OE *þrym* 'end-stück' (442). A number of words listed at the beginning of the entry are needlessly repeated at the end. 448: English *though* was taken from Scand. 449: Add *methinks* (line 18). Among the 'schallwörter' under *tu tutu* I do not find English *toot* and *tootle*. 450: On Icel. *pulr* and OE *pyle* see MLN 44.129 f. 452: Add OE **þwinglian* 'fasten' (the pp. *geþwinglod* occurs). 453: For *þváere* read *þwære*, *þwére*; the word means not 'ölpresse' but 'pestle'. 454: Modern Norw. *twirla* goes back to medieval **þvirla* and therefore cannot be the source of English *twirl*, which is probably a blend of *twist* and *whirl*. 456: The etymology proposed for *þræll* makes serious phonological difficulties and cannot be accepted. 457: Add OE *þrefe*, modern *threave* (taken from Scand.). ME *thrift* did not mean 'sparsamkeit'. 459: Add modern English *threat* (line 12) and *thrust* (line 21). OE *þrysmān* is a derivative of *þrosm* 'smoke'. 460 top: for *indryhti* read *indryhto*. 462: OHG *Thraso* has a Terentian flavor that I find it hard to ignore. 463: Add OE *þrotu*, modern *throat* (line 4); modern *tease* (line 23). It is not clear why the words in *-s* here are separated from those in *-s* later (467) in the entry. 465: English *tib* 'girl, strumpet' has nothing to do with OE *tife* 'bitch' but is the pet name *Tib* 'Isabel' used as a common noun. 466: English *ted* goes back to OE **teddan*, a native word cognate with Icel. *teðja*, as the consonantism reveals. 467: For *tættec* (line 8) read *tætteca*. English *toze* goes with Norw. dial. *tosa* 'zerfasern,

zupfen' and the NED is probably wrong in postulating an OE **tāsan* 'tease'. After *tótusen*, add modern *touse*, whence *tussle*.

471: Add OE *tānen* 'made of twigs' (line 11). On *tin* etc. see H. M. Flasdieck, *Zinn und Zink* (1952). Icel. *tífa* 'frau mit schnellen bewegungen' has OE *tife* 'bitch' for cognate. 475: Add (line 3) OE *-togen*, ME *-town* in *wantown*, modern *wanton*; and (line 18) ME *tuggen*, modern *tug*, the intensive of *tow*. 476: Under *dæk* add Icel. *toğla* 'gnaw' and Gothic *tahjan* 'reissen.' 478: Add OE *hund* '100' (line 32). 483: English *titter* 'zittern' and *titter* 'kichern' are probably the same word in origin, cognate with Icel. *titra*. 484: English *trade* 'handel' (line 5) was taken from MLG (see line 13), not from Scand. 485: English *trull* 'dirne' was taken from German, but not by way of French. 486: With OE *trinda* or *trinde* 'ball' go OE *trendan* and *ātrendlian* 'roll' (modern *trend*, *trendle*) and *trandende* 'steep'. 488: *tryggð* has OE *trēowð* for cognate. For *trēowian* (line 30) read *trēow(i)an*, modern *trow*. 490 bottom: OE *getæl* gives modern *tall*. 491: Under *del-* add *tol* n. 'slow trot', *tolari* 'trotter'. 494: Under *dō-* there should be a cross reference to Wood's etymology (464). 495: Add (line 27) OE *getwinn*. 496: Under *dhāg-* might be added OE *dēcan* 'smear', but this word may be given another connection: see Holthausen, *Ae.et. wb.* 497: Words like *dapi* and *depill* are out of place under IE *dhabh-*. 501: OE *dāge* means 'breadmaker', not 'dairymaid'. The latter meaning arose in ME times, out of an earlier general sense 'female servant'. Add OE *-dige* in *hlāfdige* 'lady'. 502: Under *dheu-* add OE *driegan* 'die'. 504 (line 5): add OE *-docca* 'muscle' (in *fingerdocca*), modern *dock* 'muscular or fleshy part of an animal's tail; part of tail left after cutting or clipping'. Some of the words listed on this page would go equally well under 4. *dhor-* (518). For *dodd* (line 30) read *dott*. 506: Add OE *dwola*, *dwolian*, *dwe-lian*, and *dwelsian*.

507: Cancel English *dusk* 'feucht' (line 19). Add OE *dūst* (line 38) and OE *dox* 'dark-haired' (line 41), whence by metathesis modern *dusk*. 508: Icel. *duga* has for cognates OE *dugan* and OHG *tugan*; the author seems to think that these infinitives do not occur. 509: For *dtefan* (line 13) read *ādtefan* and note that modern *dive* goes back, not to this verb but to OE *dýfan*. 510: For *Swæfdæg* read *Swæbdæg*. Add OE *dagung* (line 23). 513: For schott. read engl. (line 24). With *dandalast* etc. compare Faroese *danda* and English *dandle*. 515: For *berme* read *bärme*. OE *drabbe* (line 6) is a ghost word; Bosworth has it but Toller deletes it. Add (line 19 ff.) OE *drestian* 'slaver, drivel' and ME *dravelen* 'talk foolishly'. 516: After *dréahnian* add modern *drain*. 517: Add English *drone* 'sound of bagpipe'. 518: Under *dher-* add Icel. *drit* 'excrement' and cancel OE *drit*; modern *dirt* is of Scand. origin. 519 bottom: After *dræge* f. add modern *dray*. 521: OE *gedyrst* means 'trouble', not 'kühnheit'. The author may have had the adj. *dyrstig* 'bold' in mind. 522 bottom: ME *dalien* and its Anglo-French counterpart *dalier* 'dally' may belong here but the formation has not been explained. 523: Add English *dilly-dally*. 524: Under *dhelbh-* add OE *delfan* 'dig' (modern *delve*), OFris. *delwa*, OHG *telban* etc.; also OE *gedelf* 'excavation', *delfere* 'digger' etc. Under *dhō-* add Icel. *Dóri* (dwarf name). 525: OE *duru* 'door' so far as its form goes could perfectly well be a sing. in origin; Feist

takes it for an acc. pl. 527: Icel. *dreif* f. has for cognate OE *drāf*, *drāf* 'expulsion'. With Icel. *dreyri* 'blood' compare OE *drēor*. 528: Icel. *dropi* (line 24) has for cognate OE *dropa* but not modern *drop*, which goes back to OE *droppian*. 529 top: Add English *drill* 'rivulet' and *drill* 'trickle, flow in drops'. As cognate of *draugr* 'gespenst' the author gives OE *drēag* 'larva mortui', but I have not found this word in the dictionaries.

530: Under 2. *dhrebbh-* add *dramba* 'strut' and its cognate MLG *drammen* 'make a rumbling noise'. Another grade appears in English *drumble* 'make a droning noise'. 531: OE *feorinunga* 'spreu' is not in the dictionaries. 532: Add OE *gefæge* 'acceptable'. 535: For **pi(n)c-* read **pi(n)k-*. Add OE *fægan* 'paint'. 536: OE *ficol* appears both here, under *peig-*, and later on (541), under another root. Danish *famle* etc. can hardly go under *peim-* for obvious phonological reasons. 537: Under *peis-* add Amer. English *feist*, *fice*, short for *feisting dog*. 538: *Fathom* does not mean 'umarmung'. 539: Add English *fettle* v. 'get ready, put in order' (now dial.), whence *fettle* n. 'condition'. 540: *Fipla* etc. need a separate entry. 541: It is hard to see how *fika* etc. can go under *ped-*. OE *fetan* is a ghost word; it is in Bosworth but Toller cancels it. 542: Add OE *feterian*, modern *fetter*. 544: For *frock* (line 20) read *frosk*. 547: Add English *fro* (in *to and fro*), taken from Scand. 548 top: Add OE *forma* 'first'. 549: Add OE *gefara* 'traveling companion'. English *ferry* is a native word, the continuation of OE *ferian*, now used as both noun and verb. 552: OE *fyrð* 'menschenhaufen' does not exist; the author seems to have in mind *fyrhð*, a variant of *ferhð* 'person'. 554: OE *furh(wudu)* 'pine' and OE *fyrh* (ME *firre*) 'fir' are so distinguished in Clark Hall's dictionary but the distinction will hardly hold. We have no reason to think that modern *fir* is from Danish. Under 1. *perd-* for *feortan* read *feorting*. Under *pers-* add northern English *force* and *foss*. 555: Under 4. *pel-* for *eal-felo* read *ealfēlo*, *ælfæle*, and add OE *fēlan*, modern *feel*.

558: After *fealh* (line 26) add modern *felly* and *felloe*. 562: Under *pō(i)-* take *feimni* 'bashfulness', *feima* 'bashful girl', etc. (wrongly put under *poi-* 563). 563: OE *fāmne* means 'jungfrau' but not 'junge frau'. See *English studies* 17.225 ff. and *MLN* 53.32 ff. 564: Icel. *fylja* has English *filly* for cognate. 565: under *pū-* add ME *fulmard*, modern *foumart* 'polecat'. 566: The *jōl-* of *jōlfuðr* should have been taken up under 1. *el-* (72); for further examples see *Englische studien* 67.321 ff. 567: Under *pneu-* the author gives OE *fnēosan*; but this verb does not occur, though its noun *fnēosung* 'sneezing' is recorded, as is its ME continuation *fnēsen*, whence modern *sneeze* by blending with *snite*, *snivel*, *snore*, *snort*, or the like. The author might have cited OE *fnesan* 'pant, gasp' (5th class strong) but did not. He does give OE *fnæsettan* 'schnarchen, schnaufen' but this seems to be a mistake for *fnæstian*. Under *prēi-* he gives OE *frēogan* (*frēon*), along with an unrecorded *frīgan* but his gloss 'freien, werben' holds only for OS *friohian*. 568: For OE *frēo* f.sg. and *frīge* f.pl. (line 17) see *MP* 40.11 ff. The author does not mention *frīge*, and omits besides, oddly enough, the element *frīge-* in OE *Frīgedæg*, modern *Friday*.

570: Under *preg-* add OE *freca* 'warrior'. The IE root *prīs-* set up for the Frisian name will hardly do, in view of OE *Frēsan*, OFris. *Frēsa*, OHG *Frieso*, MLG *Vrēse*, with Gmc. *ē* < IE *ēi*. 571 bottom: Add English *flake* 'kind of

frame' (from Scand.). 573: OE *flōcan* means 'strike', not 'beifall klatschen' (see Liebermann *Festschrift* 155). 575: With *fleip* 'loses gerede' compare English *flippant* etc. Note also ME *flīpen* 'pull off', modern *flīpe* 'peel, flay'. Under *pleu-* might have been noted ME *flēm* 'current' (cf. OE *flēam* 'flight'), clearly cognate with Icel. *flaumr* 'strömung'. 577: Icel. *fleyta* has OE *flēte* 'boat' for cognate. 578 top: The cognate of OHG *flōz* is OE *flēat* 'ship'. Another grade appears in OE *flēot*, modern *fleet*. Icel. *flytja* answers to ME *flitten*, *flütten*, modern *flüt*. Under *germ. fleuh-* the author has confused two OE words: *flēah* m. 'flea' and *flēah* n. 'white speck in the eye'. 579: Compare English *fluster* 'bustle'. Under *pleuq-* the *-k-* words are hardly in place if truly Gmc., and, as the author himself points out, *flygsa* goes well with *fljúga*. In short, there is no need to set up this root for Gmc. at all. 580: English *flag* 'a turf, sod' is no 'lehnwort' though akin to Icel. *flag* and *flaga*; the gemination (ME *flagge*) is peculiar to English and presumably arose in English. The base *flag-* is a Vernerian variant of *flah-*; cf. ME *flaugh* and *flaught* 'a turf, piece of sod'. 582: After *flet* (line 9) add modern *flat* 'living-quarters on one floor'. 583 ff.: The roots entered here are 'schallwurzeln' that I will pass by. 586: Add OE *-platod* 'plated'. 588: The modern continuation of OE *byle* in standard speech is not *bile* but *boil*. 589: Add (line 10) English *pouter* 'kind of pigeon'. 591: OE *geposu* (ME *pose*) is the pl. of OE *gepos* 'cold in the head'. The author tells us, 'Mit germ. b (= idg. bh- oder unverschobenes b- oder neues b-) gehören hierher: ...' In this way he manages to list a great variety of words under one IE root, a method of etymologizing not marked by scientific rigor.

595: OE *gebann* (not *bann*) gives modern *banns*. 596: One ought to distinguish between *pipra* 'zittern, beben' and *pipra* 'ermuntern'. The latter is a derivative of *piparr* 'pepper' and properly means 'put pepper in or on (something)'. The sense 'enliven, cheer' is found in the phrase *pipra upp* 'pep up' and sounds remarkably like American slang. 597 top: Add OE *bytt* 'small piece of land'. 599: After OE *bærs* add modern *bass*. 600: Icel. *byrstr* adj. has for cognate OE *gebyrst*. 602: The author gives English *bait* vb. but not *bait* n., and he attaches the verb not to Icel. *beita* vb. (where it belongs) but to *beita* 'angelwurm'. 603: OE *gebætu* means 'trappings'. OE *bite* gives modern *bit* (not *bite*). 604: Add OE *beonet* (line 4). 605: Add OE *byldan* (line 12). OE *bēow* (line 30) means 'barley'. 606: After *baugr* add OE *bēag*. 607: After OE *bydel* add modern *beadle* and *Biddle*. 609: With *bakki* goes English *bank* (taken from Scand.); with *bekkr* goes English *bench* (cognate). After OE *bana* add modern *bane*. Under *bheng[h]-* add English *bangle* 'beat about in the air' (said of hawks). 610: Under *bheng[h]-* the form *bunki* has a *k* needing explanation. The author takes it for *g* made surd by 'kons.-schärfung', but the MLG cognate leads me to think rather of a *k*-suffix. 611: After the equality sign (line 2) put OE *batian*. Modern *batten* is from Scand. For *rob(b)in* read *roband*. 613: After ME *bāre* (line 2) add modern *bore*. OE *byre* means 'storm', not 'günstiger wind' (it glosses *aestus*). 614: Icel. *bráð* f. 'rohes fleisch' has OE *bræð* f. 'flesh' for cognate. OE *berian* 'strike, beat' is a ghost word. 616 (line 14): For *brémel* read *brémbel*, *brémel*. 618: Icel. *bergja* has OE *gebergan* etc. (not *bergan* etc.) for cognate. 619: *Bjarki* does not mean 'der kleine bär' but 'bear-person' (see *Arkiv* 61.284). 620: OE *brunepa*

and modern *brunt* are not connected. 621 top: After OE *broð* add modern *broth*. 623: OE *bearcian* 'bark' does not exist; the author may have had in mind the noun *bearce* 'barking' or the verb *borcian*. Modern *bark* continues OE *beorcan* (3d class strong). 625: The gloss 'flecken' for English *borough* will not do. Metropolitan London is made up chiefly of boroughs, and metropolitan New York consists of five boroughs. 626: OE *blæd* m. 'blast, breath, life, riches' (from *blāwan*) is here confused with OE *blēd* f. 'shoot, bloom, fruit, harvest' (from *blōwan*). Only the latter is in place in this entry. Unhappily the confusion is not peculiar to the author; it goes back to OE times! 627: English *ball* 'kugel' is of Scand. origin. OF *bale* (modern *balle*) or its Flemish original was taken into English in the form *bale*. 628: OE *bieldan* (line 6) means 'hearten'.

629: After OE *bylgan* add modern *bellow*. Icel. *bull* 'geschwätz' answers to English slang *bull* in much the same sense; compare ME *bul* 'falsehood' and OF *boul* 'deceit'. 631: OE has *bæc* (cognate with OHG *bah*) as well as *bece* (cognate with OS *beki*); the modern *beck* may be a blend of the two. 632: Icel. *beðja* 'hausfrau' has OE *gebedde* 'wife' for cognate. 634: Under *bhrag-* add OE *gebræc* 'noise' and *bræcian* 'make noise.' After ME *brallen* add modern *brawl*. 635: Icel. *breyta* 'ändern' has for cognate OE *brýtan* 'crush, pound' (< **brautjan*). Note also *brytsen* 'fragment' and *brytsnian* 'distribute,' as well as modern *brittle* 'easily broken'. 636: Add modern English *preen* 'pin' (noun and verb), now a north-country word. 638: Under *bhreg-* add *Breca* (legendary king). 639: Add dial. English *brimse* (taken from Scand.). 640: Icel. *brinki* 'kleine erhöhung im erdboden' is best explained as a derivative (*k-*suffix) of *bringa* 'brust; grasbewachsene wölbung'. 641: Icel. *brask* and *braska* may have for cognates 16th-century English *brash* 'assault' (noun and verb) and *brash* adj. 'given to the offensive, impetuous, rash'. After OE *brū* add modern *brow*. 642 top: Add English *bloat* (from Scand.). 644: After OE *blæge* add modern *blay* (fish name). It is wrong to say 'engl. *bleak* ist nord.' Icel. *bleikr* 'pale' appears in ME as *bleik*, not **blēk*. Modern *bleak* seems to be a blend of *bleach* (OE *blæc*) and *blake* (OE *blac*).

645: OE *blāw* is a ghost word. The Erfurt gloss actually reads *havi- blauum* (Sweet, *OET* 109); it glosses the Latin fem. *blat(t)a* 'purple'. Here *havi* answers to classical OE *hæwe* 'blue, purple, gray' and *blauum* (i.e. *blauinu* fem.?) presumably answers to classical *blāwen* 'blue'. ME *blew* may perfectly well be a continuation of OE *blāwen* (so also Kluge, *Et. wb.*). Under *bhleg-* add OE *blonca* 'horse' (orig. 'the white one'; compare *bay*). For *blæcern*, *blacern* read *blācern*, *blācern*; this word is out of place here and should have been entered under *bhleig-* (644). 647: Add OE *blysian* 'burn, blaze'. 648: It will hardly do to separate Gothic *gamaidans*, entered here under *mai-*, and Gothic *maidjan*, entered under 2. *meit(h)-* (658). English *mad* means 'verrückt', not 'töricht'. 650: OE *gemaca* gives ME *make* (now replaced by *mate*). 651: Icel. *úmagi* has OE *unmaga* for cognate. Under *maghu-* add English *may* (from Scand.). English *maid* is short for *maiden* (OE *mægden*) and must be kept apart from OE *mægeþ*. 652: Under *mad-* change *mettan* to *metian*. 655: To the cognates of Icel. *mæra* add OE *mæran*. 656: With Icel. *-mæri* and OE *gemære* 'boundary' goes the legendary proper name *Mæringas* (see *Acta phil. scand.* 9.84). 657: OE *minn*

'small' is probably a ghost word; see *Anglia Beiblatt* 16.229 (Holthausen). 658: For *migoð* (line 2) read *migoða*. 659: Add English *amiss* (line 5). Under 1. *meu-* add *Mauringa* etc. (see *MLN* 55.141 f.).

660: English *muck* is native, not 'nordisch'. Cf. OE *-moc* 'dung' and *-moce* 'scum, froth, slime'. 661: English *smug* 'smooth, sleek' may go here. 662: Under *megh-* add OE *māga*, *māge*, and *mago*. Under *meg(h)-* add OE *mycel*, whence ME *muchel*, modern *much*. Under *med-* the author lists an OE fem. *meoto*. The form occurs in *Beowulf* 489 but is better taken as a neuter plural. OE *metod* may mean 'schicksal' but more often it means 'creator, God'. 664: Icel. *miðlung(i)* adv. has English *middling* for cognate. 665 bottom: Add OE *men(n)en* 'female slave'. 666: Add OE *mennisc*. The Mannus of Tacitus is hardly well identified as the 'urahn der Deutschen'. After all, Tacitus called his treatise *Germania*. 667 top: Add *Metuonis* 'North Sea' (Pliny). 668: German *mond* goes formally with Icel. *mánaðr*, not with *máni* 'moon'. 669: Under *mer-* add English *morn* and note that OE *morgen* had *mornes* and *morne* for gen. and dat., whence the new nom. *morn* first found in ME. Add also OFris. *morn*. 671: Under *merio-* add ME *-mard* in *fulmard* 'polecat' (modern *foumart*). The heading *merq-* is decidedly confusing, since in the body of the entry we are told that the words there taken up go back to IE *merǵ-*. 672: For *mealn* read *mealm-*. 673: Icel. *mola* 'zermalmen' has for cognate English *mull* (now dial.). For *smiellan* read *smyllan*. 674: For *mysln* (line 5) read *molsn*. The word means 'decay', not 'dust'. Add OE *smyltan* 'appease, assuage'.

675: Icel. *molla* 'langsam kochen, schlaff sein' seems to be connected with English *mull* 'stupefy, make dull' (obsolete) and 'heat (wine or beer), adding spices etc.'. 677 top: For *melc* read *meolc*. Add Mod. English *milk* 'spat (of an oyster)'. 678: OE *mæsle* (line 2) seems to be a ghost word. English *fulmar* was taken from Scand. 685: Icel. *nauðigr* has for cognate OE *nēadig* (in *nēadignes* 'obligation'). 686: English *nuzzle* is not a dialectal word. 687: After *nullus* (line 5) add English *none*. OE *nicor* may mean 'river-horse' but not 'krokodil'. 688: Icel. *nú* 'now' and cognates are taken up here under *neuos* and again, as a separate entry, under *nu-* (697). 690: The author puts Icel. *náð* 'gnade' and Gothic **niþan* under *net-* rather than *neth-* for reasons that are not clear to me, though since the IE connections of these words are highly uncertain the form of the root does not matter much. Under *nē-tr-* add English *adder*. 691: Under *nepōt-* some mention of *niðr* 'man' etc. should have been made, if only a reference to 'wzf. *ni-*' (57). 692: Under *nem-* add modern English *nim* 'steal' and *numb*. Schütte's paper in *DSt.* came out in 1946-47 (not 1948). The correct critical reading of the tribal name in Tacitus is *Nuithones*; for an emendation more plausible than Schütte's *Nurthones* see *Namn och bygd* 22.48. 695: Icel. *níða* has for cognate OE *nīðan* 'envy, hate'. 697 top: The *m* (for *n*) of Avestan *magna-* 'nackt' is a case of dissimilation; compare Icel. *móna* 'mama' with *n* for *m*. 698 top: English *rally* 'make fun of' is a variant of *rail*, a word of French origin, unconnected with Icel. *relli*. For *rómig* (line 7) read *rómei* (i.e. *rómeg*).

699: After OE *dráfan* add modern *rove* 'draw out (wool or cotton fibers) and slightly twist (them)'. A sliver thus formed is called a rove. 700: OE *ripan* 'ernten' seems to be a ghost word. The verbs recorded are *repan* (modern *reap*)

and *rīpan*. OE *rīfter* 'reaping-hook' is no longer current. 703: OE *rād* (line 4) does not mean 'weg' (see *PMLA* 67.538 ff.). It gives modern *raid* as well as *road*. 704: The adj. *graith* 'fertig' (now obsolete) is not especially Scottish; the author seems to have it confused with *graith* 'equipment'. Add OE *gerāðian* 'arrange'. 706: For *roun* (line 8) read *round*. 707: For *roscian* (line 15) read *gerōscian*, *gerōstian* 'roast, dry'. Under *reu-* add English *rug* (from Scand.). 709: The author left out (rightly, no doubt) OE *gerēþre*, *gerȳþre* 'clearing' (if that is what the word means): the variation between *ē* and *ȳ* points to Gmc. *au* and the *þ* forbids connection with IE *reudh-*. But it will hardly do to bring in OE *rēodan* 'redde' here; its poetical meaning 'kill' grew out of bloodshed and has nothing to do with making land 'urbar' by clearing. Modern *rid* is a continuation of OE *geryddan* 'clear (land)', not of *āryddan* 'strip, plunder'. 710: Add OF *rober* (from German), English *rob*. 711: For *rieþan* (line 4) read *gerȳpan*. 712 bottom: Add OE *rūmian* 'become clear (of obstruction)'.

713: For OE *riec*, *riecan*, and *riecels* read *rēc*, *rēcan* or *rīcan*, and *rēcels* or *rīcels* respectively. Add OE *edreccan*, *edrocian*, *eodorcan* 'ruminate'. 714: Icel. *rauðr* has for cognate OE *rēad*, modern *red* (not *read*); the old form is kept in the surname *Read*. Add OE *rudduc* 'robin' (lit. 'the red one'), modern *ruddock*. 715: Add English *rug* 'pull, tear, tug' (now dial.). 720: Under *reth-* add MWelsh *rhawd* 'course' (whence Irish *rót* 'way'). 721: Under *rep-* add English *ravel* (from Dutch). 723: For *ramme* (line 7, 4th word) read *ramm*. 725: Under *lā-* add Icel. *læmingr* 'loom, guillemot' (der. of *lómr* 'geschrei') and OE *lā* (the ejaculation) < Gmc. **lai-*, IE **lāi-*; also Gothic *laian*, Latin *lāmentum*, etc. The English cognate of Icel. *lulla* is *lull*, not *loll*. English *loll* 'faulenzien' is not a dialectal word. 730: OE *slīc* 'listig' is a ghost word; Bosworth has it but Toller deleted it. OE *slīc* 'mallet, hammer (for polishing)' belongs here however, as does OE **slīcian* (not **slīcian*), the pp. of which makes part of the compound *nīgslycod* (not *nīw-slicod*) 'newly smoothed'. In all likelihood *slīc* 'tool for polishing' is a noun use of an unrecorded OE adj. **slīc* 'smooth', whence modern *sleek* and *slick*. The author holds that these are 'aus dem nord.', but this theory leaves the vocalism unexplained; Icel. *slīkr* 'glatt' would give modern English **slike*. 731: For *tóslūpan* (line 5) read *tóslūpan*. This word is out of place here, of course. Add OE *slīpig* 'slimy'.

733 bottom: After *hwelc* add modern *which*. 734 top: After *swylc* add modern *such*. 735 top: Add OE *lācan* 'spring up', *gelācan* 'emulate', and *gelācian* 'bestow'. Under 4. *leig-* we are told that English *leech* or *leach*, the nautical term, is 'lehnwort'. In fact its three early forms, *leche*, *lek*, and *lyche*, can be plausibly explained only on the theory that we have to do with a native word. ME *lēche* presumably continues OE **gelāce*, WGmc. **galaikja-*. ME *lek* seems to be a blend: alongside **gelāce* there presumably existed in OE an *a*-stem **gelāc*, in which, of course, the *c* had the value [k], and in the blend **gelāc* (later *lek*) the [k] came from **gelāc*, the *ā* from **gelāce*. These hypothetical forms are cognate with MHG *geleich* 'gelenk'. ME *lēche*, OE **gelīce*, shows another grade; it is cognate with Icel. *lūk*. 739: OE *lēopian* is a ghost word; Bosworth has it but Toller deletes it. 740: Add Dan. *lur* (taken from Norw.). After OE *forlēosan* (line 32) add modern *lose*. The English cognate of Icel. *lauss* is the suffix

-less (OE *-lēas*); modern *loose* was taken from Scand. English *release* is not a native formation (line 43); it has nothing to do with OE *liesan* 'loosen, release'.

741: For *lēske* (line 3) read *leske*; the *e* was once long but lost its length by Orm's time if not earlier. The author presents *leske* as a cognate of Icel. *ljóskei* 'groin' but the consonantism shows that the ME word was taken from Scand. A kindred word native to English, with nil grade of the IE vowel, is the adj. *lush*, which first meant 'soft, tender'. 744: It will not do to say that *warlock* is 'aus dem nord.' It is actually a Scottish form continuing OE *wærloga* 'devil'. 745: Under *leud-* add OE *lot* 'fraud, guile', ME *lotien* 'lurk', and ME *lotebi* 'paramour'. 746: The author puts Icel. *ljóðr* 'fehler' under IE *leud-* in spite of its *ð*. He seems to think it an alteration of the *-ljótr* found in the legal term *áljótr* 'mit vorbedacht beigebrachte wunde', but he does not explain how the alteration came about. It is easier to take another tack and suppose that the *r* of *ljóðr* made part of the stem but was mistaken for the nom. sg. ending, a mistake that gave rise to gen. sg. *ljóðs* etc. instead of the original **ljóðrs* etc. For a parallel see *Acta phil. scand.* 6.330 f. On this hypothesis we may plausibly connect *ljóðr* with OE *lypre* 'wicked' (modern *liher*) and German *lieder(lich)* 'debauched, dissolute'. Under *leudh-* add English *leed* 'kind of plant' (see *Namn och byggd* 20.224) and the *Liothida* of Jordanes. Most unhappily the author has overlooked the etymology of *Lóðurr* set forth by W. Krogmann (*Acta phil. scand.* 12.59-70).

747: The author wrongly glosses OE *lēoden* (i.e. *Latin*) with 'sprache'. For the various forms that the word *Latin* took in OE, see *Jespersen Misc.* 52 (1930). The variant *lēoden*, found only in the Leechdoms text, may have arisen by confusion with OE *lēode* 'people', but the two words are not etymologically connected. The variant *lēden* which the author gives does not occur, so far as I can discover. The OE word meant 'Latin' to start with, but later became applicable to any foreign language. 748: OE *lēap* may mean 'headless body'. Under *leubh-* add modern English *believe*. 749: Under 1. *leg-* add English *leak* (noun and verb). The author takes up here MLG *lak* 'mangel' etc. and takes the group up again under 2. *lengh-* (756) without cross reference. 750 bottom: After OE *leger* add modern *lair*. 751: For *lóg* (line 18) read *löh*. 753: Add English *lapel* and *earlap* (line 10). After OE *slæpan* (line 24) add modern *sleep*. 755: Icel. *lemja* has for cognate OE *lemman*. Under *lenq-* add Icel. *lengja* 'strip'. 758: Under *læs-* add OE *lysven* 'pus'. 763 bottom: For further material see *Namn och byggd* 22.38.

765: For *sieltan* (which does not occur) read *sæltan* (or *selltan* or *sylltan*). 766: For *sól* and *sólian* (line 11) read *sol* and *solian*. These words cannot properly be kept apart from Gothic *bisauljan* etc., words which the author puts under another root (774). 767: After OE *sibb* (line 11) add modern *sibling*. OE *swān* (line 20) does not mean 'mann, kriegler' in poetry (see *RES* 21.126 f.). Icel. *sjaldnar* has for cognate OE *seldnor*. 768: On *swá* etc. see E. E. Ericson, *Hesperia* Ergänzungsreihe 12 passim (1932). 769: On *sil* (line 13) and its congeners see *ZfdPh.* 4.214. 771: The author puts *sjóli* 'könig' under two roots, one here, the other earlier (763), without cross reference. 773: Under *seib-* add OE *sāp* 'amber'. Modern English *sipe* and *seep* go back to OE *sīpian* and *sipian* respec-

tively. 774 top: Add OE *segg* 'salum' (see *Stud. in honor of A. M. Sturtevant* 41). 775: For *séofian* read *seofian*. The *eo* here is not original (note the variant *sefian*) and the word is out of place in this entry. Icel. *syfla* has for cognate OE *gesyflan*. 776: English *suds*, if native, is better connected with OE *syde* 'water in which something has been boiled'. 780 bottom: On *siklingr* etc. see *Stud. germ. tillägnade E. A. Kock* 193. 781: Add OE *sætung*, *gesæte* 'ambush', *sæt(n)ere* 'robber', *sæte* 'house', *sæta* 'holding (of land)', *-sæta* 'inhabitant', and *seten* 'plantation'. 788 bottom: For *selþ* read *sælþ*. 789: English *solan* 'gannet' is wrongly equated with Icel. *súlan* (i.e. *súla* plus definite article). The variant *soland* indicates that the word is a compound: *súla* plus *ond*. 790: For *salu* (line 3) read *sala*. Under *sel-* add OE *sæl* 'happiness'. OE *sētra*, *sēlost* mean 'better, best' (not 'gut'). 791: Under *soito-s* add OE *-siden* in *ælsiden* 'nightmare'. 792: French *surelle* (whence English *sorrel*) is not a German formation, though the base *sur* is of German origin. 794: On Irish *sceang*, *sceng* (better spelt *scing*) see *Lg.* 28.532.

In trying to shorten this review I have left out more than I kept in.

Werkwoordsvorme in Afrikaans in die verlede tyd. By MEYER DE VILLIERS. (Annale van die Universiteit van Stellenbosch, Jaargang 26, Reeks B, No. 1.) Pp. 92. [Stellenbosch, South Africa], 1951.

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The title of the English abstract of this book furnishes a convenient summary of the contents: The relation between the Afrikaans verb form and time, referring particularly to the past. The relationship of verb forms to tense, aspect, and mode is outlined in Chapter 1; the Afrikaans forms are classified and the basic temporal, modal, and aspectual values of each form are delineated. Chapter 2 is given over to an exposition of the stylistic values of verb forms. The subject of Chapter 3 is the verbs in time, noun, and restrictive adjective clauses which are present in form but past in meaning; the basis for the study is a statistical tabulation of such forms taken from a number of literary sources. Chapter 4 contains a general discussion of the problem of modality, with particular reference to certain types of dependent clauses; the modal character of the future tense is also taken up briefly.

In Chapter 1, §4, the forms of the Afrikaans verb are enumerated and classified. The majority of verbs have only a stem form (*stamvorm*) and a past participle (*deelwoord*) as morphologically distinct forms. The participle is formed by prefixing *ge-* to the stem form, i.e. *gesien* from *sien*, *gelees* from *lees*. However, as in German and Dutch, stems with unstressed prefixes do not combine with the prefix *ge-* and the result is a verb with no morphological differentiation of forms, e.g. *beskou*, *vermeld*. The participle forms a phrase with the auxiliary verb *het* which is called the compound form (*samegestelde vorm*). In this phrase the participle often lacks the *ge-* prefix when a dependent infinitive is present, e.g. *Ons het hom goed leer ken* 'We became well acquainted with him'. This corresponds to the so-called 'double-infinitive' of German and Dutch.

The infinitive category is established, as is a part of the participle category,

on the basis of syntactic criteria. But at least two verbs have differentiated infinitive forms: *hê* (stem form *het*) 'to have' and *wees* (stem form *is*) 'to be'. For the sake of completeness it would have been desirable to identify, on the same syntactic basis, a compound infinitive consisting of participle + *het*, e.g. *Jy behoort dit te doen* 'You ought to do it' : *Jy behoort dit te gedoen het* 'You should have done it'. This would have been unavoidable if the infinitive form *hê* rather than the stem form *het* stood in this construction, but the syntactic criteria are clear. The author identifies this form with the compound form.

Another construction, or phrase, consisting of *word* or *is* + participle, is given the name passive (*passief*, *passiewe vorm*). None of the combinations of modal auxiliary verb and infinitive are given constructional names.

A paragraph titled Irregular verbs (*Onreëlmatige werkwoorde*) discusses the verbs which have simple past-tense forms, as well as some verbs which have no participle form. The latter type is represented by *behoort* 'ought', *hoef* 'to have (to)', and *durf* 'to dare (to)', which are invariable auxiliary verbs in that the tense of an expression containing one of these verbs is set by the use of a simple or compound infinitive, i.e. as in English *ought to do* vs. *ought to have done*. Verbs of the type which have more than the usual number of forms are *het* 'have', *weet* 'know', and *dink* 'think'. Each of these has a second form: *had*, *wis*, and *dag* (*dog*). These forms are labeled imperfects. The forms *gehad* and *gedag* (*gedog*) appear as past participles for *hê* and *dink*. The so-called imperfect forms *had*, *wis*, and *dag* (*dog*) are said to vary freely as relic forms with the corresponding compound forms *het gehad*, *het geweet*, and *het gedink* (*gedag*, *gedog*). The verb *wees* is treated separately from the others although it has corresponding forms: *is*, *was*, *gewees*. The reason for the different treatment is apparently that the form *was* appears much more frequently than the compound form in main clauses. Indeed, the form *het gewees* in a main clause 'klink onafrikaans' even though the author is able to quote a pair of examples from his sources. The form *was gewees* is likewise frowned upon, though it apparently occurs in speech. The distinction between the imperfect *was* and the compound form *gewees het* in subordinate clauses is passed over with the observation that in some constructions they are in contrast, particularly in conditional sentences.

Following the inventory of verb forms is a general classification of verbs which divides them into three groups: regular verbs (*gewone werkwoorde*), the modal auxiliaries *kan*, *moet*, *sal*, *wil*, and the copulative verb (*koppelwerkwoord*) *wees*. It is clear that this classification takes little account of form. Even though the writer asserts that the modal auxiliaries are characterized by their two forms (the primary forms *kan* etc. and the secondary forms *kon*, *moes*, *sou*, *wou*), the same is true of the verbs *wees*, *dink*, *hê*, *weet*. The same classification is used in the later discussion of the verb forms of dependent clauses (Chapter 3).

In the opening section of the book (§1) the author discusses the relationship of grammatical tense concepts to the concept of time. The tense of a verb form is said to be an expression of the speaker's point of view with respect to the events rather than a logical determination of the time of the events. Thus even the traditional distinction between present, past, and future is difficult to determine in that the present may be conceived as an indeterminate period of time

surrounding the moment of speaking and may include segments of the logical past or future. This is shown in the examples *Hy eet nou* 'He is eating now'; *Hy studeer nou al jare lank* 'He has been studying for years'; *Ons gaan nou na die plaas* 'We are going to the farm now'.

Further, the basic concepts past and future are open to interpretation in causal or consecutive terms. In the sentence *Nadat Columbus Amerika ontdek het, het hy teruggekeer na Spanje, waar hy allerlei beproewings sou verduur* 'After Columbus had discovered America, he returned to Spain, where he was to endure all sorts of trials', the three verb expressions are to be interpreted respectively as pre-past (voorverlede), past (verlede), and past future (verlede toekomst). The future tense form can be interpreted analogically as future perfect (voldoende toekomst), future (toekomst), and post-future (na-toekomst).

The author comments (9-10) that 'We can ... infer from the sentences that simple past, present, and future are "basic", in the sense that they are direct experiences, while the other tenses are secondary in that they are delineated or experienced only in terms of the basic tenses.' This statement leads up to the author's premise (§3.2) that every word or grammatical form has a nuclear value which is constant and defines the limits of the secondary values, or functions, which the words or forms may have. Thus (89), 'many of the values which we would be inclined to attribute to verb forms really lie implicit in the connection or situation.' The criteria by which the author distinguishes in practice between primary and secondary values are never made clear. In his exposition of the nuclear values of the stem form, the author quotes a passage from Overdiep's *Stilistiese grammatika* to the effect that the present-tense form, lacking any limitation deriving from the rest of the sentence, is neutral with respect to time and thus can represent generally valid truths or imagined events. This is followed by the comment (§5.1): 'In that the stem form is in large measure neutral with respect to time it can represent general truths or imagined events. But these are secondary functions.' The author defines the nuclear value of the stem form as 'direct presence or vividness'. It would appear then that the nuclear value is purely an abstraction on the basis of the secondary values or functions, and that the latter are the ones that can be demonstrated. The author states in §2 of Chapter 1 that the tense of an expression is not dependent solely on the form of the verb, but also 'indirectly' upon other factors such as adverbial modifiers, intonation, type of syntactic construction, etc.; but he continues: 'Although these factors may not be completely disregarded, I shall proceed from the verb forms [themselves] in the present investigation in order to determine their values for the indication of the past tense and related concepts.' The fundamental weakness of the presentation lies in the author's failure to use the various conditioning factors consistently as rubrics for classifying the divergent secondary values. Such a classification would perhaps have given a clearer picture of the nuclear values.

With the discussion of the values of the compound form (§5.2) it becomes increasingly difficult for the author to distinguish primary and secondary values. It is indicated that the compound form sometimes expresses primarily past tense, sometimes completed aspect. The differentiation of these two values bogs

down in a discussion of the various possible meanings of past participles derived from transitive and intransitive verbs. The aspectual problem, however, is discussed quite cogently in the section devoted to the passive voice. As we shall see, the key to the difference is the meaning of the individual verb involved, i.e. whether it describes a process with a definite terminus or not.

The nuclear values of the compound form are finally summarized as '*afstand-neming*, establishment of a factual situation, summation of events'. This summary is followed by a statement of a number of contrasts between the stem form and the compound form, such as present vs. past, past vs. past perfect, duration vs. completion, dramatic vividness vs. historical detachment, possibility vs. factuality, concreteness vs. abstraction. The contrast values 'are not the only ones and they are not necessarily primary or basic, for it is extremely difficult in this instance to separate the primary from the secondary functions.' Several of the contrasts, which are clearly secondary in the sense that they are conditioned by the linguistic environment, are discussed later in the book.

The verb *wees* 'to be' receives special attention (§5.3). As we would expect, the basic values of the forms *is*, *was*, *gewees het* are established as present, past, and perfect or past perfect respectively.

In connection with the verb *wees* the author discusses the construction *is* + past participle, which serves as the past tense of the passive form. There is possible ambiguity in that *is* + past participle can in many instances be construed as an equational sentence, with a past participle acting as a predicate adjective, as may happen when the participle is derived from a verb which has perfective meaning, i.e. when it describes the result or end of a process. Thus the sentence *Hy is gewond* can be interpreted either as 'He is wounded' or 'He was (got) wounded'. But the ambiguity is serious only if one is intent upon finding a basic meaning for the form *is*, without regard to the syntactic constructions in which it participates. The meanings of *is* are statable only in terms of the constructions in which it occurs.

By far the most thorough treatment of the values of forms is given in connection with the modal auxiliaries. These verbs are characterized by a primary and a secondary form, e.g. *sal—sou*, *moet—moes*, and it is the author's firm conviction (§5.4) 'that these forms are differentiated basically with regard to modality and that their functions with regard to tense are only secondary.' As in the preceding instances, the statement of the basic values of these forms poses a problem which can only be solved by giving examples of the many functions (i.e. secondary values) which the forms serve. The primary and secondary forms of each verb combine with both the infinitive and the compound infinitive (i.e. past participle + *het*), resulting in a total of four possible constructions for each auxiliary verb.

Although we cannot discuss each of the points in the treatment of the modal auxiliaries, it is possible to show with a few examples that the secondary forms of these verbs have two distinct functions. The first function is that of a past-tense form contrasting with the primary form or present tense: *Slegs langsaam-erhand sou die plattelandse Afrikaner ontwaak ..., en daarna eers sal die volk werklik die behoefte gaan voel* 'Only gradually would the Afrikaner of the flatlands

awaken ..., and only after that will the people begin to feel the needs'; *Dat is 'n vraag wat ek meer as een maal moes beantwoord* 'That's a question which I had to answer more than once'; *Ons kon nie langer wag nie* 'We couldn't wait any longer'. The contrast between present and past tense can be clearly identified only in the combinations with the simple infinitive. In sentences of the type which the author calls logical hypotheses, both the primary and secondary forms can be found in combination with the compound infinitive, with no clear distinction of meaning, e.g. *Dat ... moes vir 'n ieder duidelik gewees het* 'That ... must have been clear to everyone'; *Uit die voorafgaande bladsye moet dit duidelik geword het* 'It must have become clear from the preceeding pages'. The similarly constructed phrase used in conditions contrary to fact permits only the secondary form to be used. Many of the examples suggest that the use of the primary form in a logical hypothesis implies a relationship of the events to present time, but this is certainly not true in all cases.

The other function of the secondary form can be seen in phrases containing both simple and compound infinitives. This function is similar to that of the subjunctive in conditional sentences in German: *Dit sou hulle 'n fortuintjie inbring as hulle dit sou verkoop* 'It would bring them a small fortune if they should sell it'; *Sy wonder of sy ooit moes gekom het* 'She wonders if she ever should have come'. As was mentioned above, the only distinction in form between the logical hypothesis and the condition contrary to fact is that the latter is formed exclusively with the secondary form of the auxiliary while the former may have either the primary or the secondary form. The main distinguishing feature is most certainly the sentence type, with intonation perhaps serving as a distinguishing factor.

The entire second chapter is devoted to the stylistic values of the stem form vs. the compound form. In a narrative, the compound form is used primarily in descriptive or subsidiary passages. Where the attempt is made to describe events vividly the stem form is used.

For the statistical treatment of verbs in dependent clauses, the author has drawn examples from approximately 25 literary sources, including some books of the Bible. The choice of materials is unfortunate: it represents in large measure the work of men who were acutely conscious of the use of grammatical forms for stylistic purposes. The first part of the statistical treatment is broken down into clauses introduced by such words as *toe* 'when', *terwyl* 'while', *as* 'whenever', and *wanneer* 'when'. To this list is added *asof* 'as if', which, as the author admits, usually introduces a statement of appearance and not of reality. Two other types of clause, the noun clause (*voorwerpsin*) and the restrictive adjective clause (*byvoeglike bysin*) are similarly treated. The author acknowledges that the statistics are of less value in the case of the restrictive adjective clause, since it is impossible to separate the various types of adjectival clauses accurately in working with written materials.

It is impossible to comment on the details of the analysis; the work of sorting out the aberrant forms has been generally well done by the author. The tables show a distinct preference for stem forms of regular verbs in all types of clauses except the restrictive adjective clause. The clauses containing modal auxiliaries

exhibit predominantly the secondary forms. The same is true of clauses containing the verb *wees*, where the preference is for *was* rather than *is*. In both of these types there is a much more even choice between primary and secondary forms in noun clauses. The statistics on passive-voice forms are meager and hence inconclusive.

On the whole, the results of these tabulations could have been made more illuminating. It is clear that the category of restrictive adjective clause contains a number of separate clause types, many of which regularly have the compound form. Further, there is little attempt to delineate the types of clauses in which the compound form contrasts with the stem form to show completion versus continuation. The results with respect to the use of primary and secondary forms of modal auxiliaries and *wees* in noun clauses could perhaps have been made to seem less inconclusive by taking account of the two major functions of these forms, as past tense and as subjunctive.

The author's statistical treatment neglects the use of the secondary forms *wis*, *had*, and *dag* (*dog*), although the available examples show parallelism with the other secondary forms. They are arbitrarily included among regular verbs, where the opposition is between the stem form and the compound form. Conversely, there is no attempt to differentiate the use of the secondary form of the modal auxiliaries and the compound form (e.g. *het ... kon* + infinitive). A more careful breakdown would have given much more information than merely the fact that many stem forms can serve also as past-tense forms.

The final chapter of the book contains a number of observations on the concept of modality, which is defined (74) as 'the attitude of the speaker toward his utterances'. Many of the paragraphs, such as the treatment of noun clauses with 'subjunctive' or 'optative' values (§2) and the discussion of conditional sentences (§5), are addenda to Chapter 3. To these are added a short historical note on the origin of the Afrikaans form of the unreal statement and a brief description of the modal values of the future-tense forms.

The book contains much useful material on a subject that deserves our close attention. A more representative statistical sample and a closer regard for conditioning factors will some day produce a more reliable result.

Zur Inversion in deutschen Satzwörtern. By STEN HAGSTRÖM. (Uppsala Universitets Årsskrift 1952:8.) Pp. 90. Uppsala: A.-B. Lundequistska Bokhandeln; Wiesbaden: Otto Harrassowitz, 1952.

Reviewed by HUGO J. MUELLER, *Georgetown University*

Hagström is well known for his research in onomastics. The little book under review is a by-product of his more extensive work on German names.¹ He undertakes here to examine a special problem, on which earlier workers have held widely different views: that of *Satznamen*, names that consist of a phrase—usually a finite verb and an object, a predicate noun, or an adverbial. Since Hagström includes appellatives as well as names, he speaks of *Satzwörter* rather

¹ The first big volume of his *Kölner Beinamen des 12. und 13. Jahrhunderts* (Uppsala, 1949), was recently reviewed by Otto Springer in *Lg.* 30.167-71.

than *Satznamen*; but except for a few examples apparently brought in to back up his theory, his primary concern is with names only.

Hagström applies the label 'category A' to phrase names with normal word order, 'category B' to those with inverted order. In F. Becker's study *Die deutschen Satznamen* (Basel, 1872-3) he finds the only specific treatment of category B; later authors² have either ignored the phenomenon altogether or have mentioned it only in passing. Becker held that names of category B were relatively frequent in German, and considered them a specifically German formation; among his examples are *Deubelbeiss*, *Zubring*, *Dankhab(t)*, *Bärenhalt*, *Mundhenk*, *Lantöse*, and *Hasenschreck*. But even Becker admitted that it was often difficult to decide whether the second part of such a name is an imperative or a noun. This is the point which Hagström sets out to discuss.

He takes *Zeitvertreib* 'pastime' as an example to show how unwise it is to accept uncritically the idea that all Satz Wörter with a nonverbal first part must be explained as inverted syntactical constructions, the second part being always an imperative or a 1st person singular present. In his *Deutsche Grammatik* Jakob Grimm explained *Zeitvertreib* as an imperative, holding that there was no noun *trip* or *vertrip*; and although Lexer lists both of these in his *Mittelhochdeutsches Handwörterbuch* (1872-8), the *Deutsches Wörterbuch* (15.577 [1931]) still explains *Zeitvertreib* as originally imperative, with explicit reference to Grimm. Hagström agrees that most words in category A are probably Imperativnamen; but for category B he cautiously maintains that this is not the only possible interpretation. Clear cases of imperative names in category B are in the minority; examples are *Dankhab* (71), where the inversion is due to emphasis, and *Wolmichwart* 'Take good care of me' (72), where it is due to both emphasis and rhythm.

Nearly all the words which he investigates are interpreted here as nomina agentis—a tendency already apparent in his *Kölner Beinamen* (see fn. 1). Thus the second part of *Eisenbeiss* 'braggart' (25), *Teufelbeiss*, *Deubelbeiss*, *Orabpeis* 'Bite ear off', *Steinbeiss*, and the bird name *Kernbeiss* is explained as an agent noun from MHG *-bîz*, in contrast to Grimm, Götze, Gottschald, and others, who took these formations to be phrases. For *Früetrinck* (28) Hagström assumes a MHG agent noun **-trinke*; *Utdrank* (28) is explained as a regular compound with MHG *tranc* m.; the second part of *Vorbring* and *Zubring* (31) is said to be the apocopated form of an agent noun, and so are the second parts of *Leidvergess* (37), *Bärenhalt* (53), and *Treubleib* (57). The same explanation applies to *Mundhenk* 'One who lets his mouth hang down, Sourface' (46), *Lantöse* 'Land destroyer' (35) from MHG *æsen* 'destroy', and *Wolenber* 'One who knows well how to do without' (34).

Particularly interesting is *Hornblass* or *Hornblas* 'Hornblow(er)' (44), because the equivalent English name was explained by Ernest Weekley in his *Surnames* (1917) as a phrase name. Hagström is able to strengthen his counter-theory by pointing to the OHG agent noun *hornblāso* (fem. *hornblāsa*), MHG *hornblāse*.

² A. Bach, *Deutsche Namenkunde*, Vol. 1 (*Grundriss der germanischen Philologie*, Vol. 18; 1943); J. K. Brechenmacher, *Deutsches Namenbuch* (*Deutsche Sprachkunde*, Vol. 3; 1928); M. Gottschald, *Deutsche Namenkunde*² (1942); E. Schwarz, *Deutsche Namenforschung*, Vol. 1 (1949).

Altogether Hagström deals with about 400 words of this type, completely indexed. His carefully collected material and his cautious evaluation of it have enabled him to produce an important contribution to onomastics, which will be of the greatest help to future workers in this field.

Renaissance dictionaries, English-Latin and Latin-English. By DEWITT T. STARNES. Pp. xii, 427. Austin: University of Texas Press, 1954.

Reviewed by SHERMAN M. KUHN, *University of Michigan*

Dictionaries, like electric lights and written constitutions, must be ranked among the basic facts of modern civilized life. Without them, our Western culture would no doubt survive, but only at the price of a notable increase in frustration, confusion, and unhappiness. A growing awareness of this fact may help to account for the considerable number of books and articles dealing wholly or in part with the history of lexicography which have appeared in the last two decades. By no means the least of these was *The English dictionary from Cawdrey to Johnson*, completed by Starnes and G. E. Noyes in 1946. That book presented us with a systematic and well documented study of Samuel Johnson's predecessors in the strictly English field. It demonstrated what we already knew in a general sort of way; namely, that Johnson was not himself the fountainhead of lexicography in England, but an adapter and perfecter of techniques already well established. The picture was still incomplete, however, in the absence of any systematic study of the bilingual dictionaries and glossaries which preceded the English and prepared the way for them. The present volume, designed to remedy a part of the deficiency, achieves its aim with admirable thoroughness and insight.

The title, as Starnes himself points out, is a loose one, for his study extends well beyond the confines of the English Renaissance. It begins with late medieval lexicons like the *Promptorium parvulorum* and *Medulla grammaticæ*, extant in MSS of the first half of the 15th century, and concludes with Robert Ainsworth's *Thesaurus*, which was first published in 1736 and retained its popularity well into the 19th century. The kernel of the book, however, the central two-thirds, is concerned with dictionaries of the period from 1500 to about 1660. The works discussed are nearly all bilingual (Latin-English or English-Latin or both combined). A few are trilingual: Veron's *Dictionariolum* (Lat.-Eng.-Fr.) 1552, Baret's *Alvearie* (Eng.-Lat.-Fr.) 1573, Huloet and Higgins' *Abcdarium* (Eng.-Lat.-Fr.) 1572, and Hutton's *Verborum* (Lat.-Gk.-Eng.) 1583. Some of the specialized dictionaries and glossaries, such as Peter Levins' *Manipulus vocabulorum*, are treated in an appendix (353-9).

Starnes provides a wealth of information concerning men and books and editions. He includes not only the English writers and publications, but all the major Continental lexicographers and their works, especially where the latter exerted any important influence upon the English compilers. It would be hard to single out any portion of the book for special commendation, but the chapters dealing with the dictionaries of Sir Thomas Eliot and Thomas Cooper, Thomas Thomas, Rider and Holyoke, and Robert Ainsworth will probably be most

stimulating to the general linguistic reader. The interest of these portions lies in the important contributions of the men rather than in any special handling. Even the brief chapters on the minor figures are carefully done and should meet with the approval of the more specialized reader.

The author is liberal of quotation, and his quoted matter is judiciously chosen to illustrate the rise of methods and techniques (from the bodies of the various dictionaries) and the development of theoretical views (from prefaces and other sources). The abundance of facsimiles is noteworthy, specimen pages having been reproduced from many of the early lexicons. It is also noteworthy that, although Starnes has drawn upon several European and American libraries for his materials, an unusually large proportion was available to him in a single American library, that of the University of Texas.

The book is a storehouse of information, but it is much more than that. It is a closely knit history of the growth of bilingual dictionaries and of the development of the art and science of dictionary-making. The dependence of each dictionary upon its predecessors, English or Continental or both, is indicated by painstaking analysis. The individual contributions of the various works are isolated, wherever possible, and their effects upon succeeding lexicographers pointed out. The analysis of techniques is detailed and penetrating.

The importance of such a study as this—to linguists, to historians of literature and culture, to practical dictionary-makers—may be indicated briefly. As has already been suggested, most of the methods and techniques now in use were developed by the men with whom Starnes is dealing. Most of the mistakes of modern lexicographers are not new; there were men of the Renaissance who followed the same or very similar practices, and either mended their ways in subsequent issues or failed to sell their dictionaries. Moot points of the present day, such as whether proper names should be placed in a separate section or distributed throughout the body of a dictionary, were matters of controversy as early as the 16th century. A few of the methods and techniques which the moderns owe to the early bilinguals are these: the alphabetic arrangement (one of three in use during the Middle Ages and the Renaissance), the marking of pronunciation by means of diacritics, some of our devices for conveying morphological information, the use of illustrative quotations, and the listing of phrases, idioms, and synonyms. The idea of dividing meanings into numbered senses, arranged in some logical or historical order, occurred to the bilingual lexicographers about the end of the 17th century and was later adopted by Johnson's predecessors. Although their etymologies were few and often erroneous, the bilinguals were the first to see that a dictionary might give information on the origins of words. In a small way, some of them tried to indicate levels of usage (for the Latin, of course, not for the English); whether hierarchies of linguistic purity have any real value is a debatable question, but certainly the manner in which men like John Rider tried to distinguish archaic Latin, 'words that be old and out of use', was a valuable contribution. The inclusion of lengthy encyclopedic articles in the body of a dictionary may or may not be desirable, but this practice also was originated by some of the early bilinguals. Even the idea of a period dictionary may owe something to the Renaissance classicists and their

efforts to produce a dictionary confined to the Latin vocabulary of a single brief period, the 'Golden Age'. Starnes' book sheds new light on all these matters.

The book has some minor shortcomings. In the adages quoted from the 1616 edition of Withals' dictionary (181-2), there are several readings which vary from those found in some other copies of this edition. One must make allowances for the lack of uniformity in Renaissance printing; it is hard to speak positively without a general collation of the text, which the reviewer has not made. Nevertheless, there are three errors in the Latin and one in the English which suggest either that Starnes' transcription is faulty or that he is quoting from a badly printed copy. The adage is evidently *Mutum muli scabunt*, but he gives the reading *Multum*. He has *Dimidum* in the adage *Dimidium facti etc.*, and *sterquilino* in *Gallus in suo sterquilinio etc.*; these spellings seem rather improbable in a dictionary whose entire appeal was to 17th-century schoolmasters. The English rendering of *Lucri bonus odore etc.* is given as 'Somewhat hath some faouurs, so we get the chincke, we will beare with the stinke.' Here it would seem that *faouurs* must be a misprint for either *sauours* or *sauour* (my own copy has *sauour*). Other quoted matter used by Starnes, so far as I have verified it, exhibits a high standard of accuracy.

Although Starnes' historical interpretations of his data are generally excellent, he seems not to have sensed the real causes underlying the failure of the Renaissance classicists in the field of lexicography. In summarizing the 16th century, he writes (234-5):

In the Elyot-Cooper dictionaries, perhaps most nearly representative of the century, there appears to be definite progress toward a dictionary exemplifying classical principles. Nearest to this ideal is Cooper's *Thesaurus*, which rules out many Latin terms regarded as barbarous or medieval and aims, according to the compiler, to supply examples of meaning and usage chosen from standard classical authors. There remain, however, many terms later to be challenged. The times were hardly ripe for a purely classical dictionary.

Similar remarks appear elsewhere in the book (103, 110, etc.). It is true that the 16th century was not ready for a dictionary of pure Golden-Age Latin. In fact, if we may judge by criteria such as long-continued popularity and frequency of editions and re-issues, the most successful dictionaries of the period were those which departed furthest from the ideal of the classicists. The reason is to be sought, not in the barbarity of Tudor England, but in the fact that Latin was still a living language. It had not yet retreated to the classroom and the ivory tower. It was still the everyday tool of scholars generally and of many men of affairs, who could not limit their reading and writing to the pure classical vocabulary without crippling their own effectiveness. Lawyers, churchmen, diplomats, scientists—all had their peculiar terms. These may have been barbarous or medieval or what-have-you, but they were indispensable to the men who used them. The bilingual dictionaries of the age were essentially commercial ventures, and they could not hope for much success if they ignored the needs of the infallible customer.

The last fault to be castigated is the manner in which the 15th-century lexicons are treated in Part I. This part is sketchy and leans heavily at times on secondary

sources. Starnes is well aware of the weakness, remarking in one place (vi) that 'An exhaustive study of fifteenth-century dictionaries, printed and in manuscript, would require a separate volume.' It is to be hoped that Starnes, or some scholar with comparable qualifications, will soon undertake to remedy this defect with a first-class study of the dictionaries and glossaries of the later Middle Ages.

English topographic terms in Florida 1563-1874. By E. WALLACE McMULLEN JR. Pp. [ix], 227. Gainesville: University of Florida Press, 1953.

Reviewed by FREDERIC G. CASSIDY, *University of Wisconsin*

Before finding any faults with this book let me say that in general it is a fine addition to the growing literature in the field of topographic terminology. It is a lexicographic study, not one of word geography, but obviously would be valuable in a broad way to students of the latter field. It is limited to Florida as McJimsey's work is to Virginia, and is historical, coming from the beginning down to the time when post-bellum settlement of Florida began in earnest. It therefore presents only the earlier usages. Nor can these be called local in any strict sense, since the sources are for the most part accounts written by travelers and outsiders. Some were even composed in Spanish, French, or Latin and had to be read for this study in early English translations.

The date-range is well enough justified, but the limitation to nouns and noun phrases seems unfortunate (adjectives and verbs, listed on page 4, are excluded from the Glossary). Still other limitations were made: terms for artificial features are omitted, along with 134 others (listed 2-4) which are considered too vague, formal, technical, or literary. Naturally one would like to have these treated; a complete study would have included them despite the admitted problems of definition.

McMullen's analysis of the collected terms (Part VII of the Introduction) is detailed and excellent. It considers first the usage of the terms: which are British and which American, and their sources in Spanish, French, and American Indian languages; their labeling as archaic, poetic, dialectal, etc.; the extensions and limitations which the terms have undergone; the status of neologisms as Americanisms, Southernisms, or localisms; and the words for which quotations have been found antedating those of the *Oxford dictionary*, the *Dictionary of American English*, and the *Dictionary of Americanisms*.

Second, there is an analysis into 'land forms' and 'water forms' of various classes, with very careful discussion of the shades of distinction among terms. The Introduction concludes with a list of 'difficult terms' and with material preparatory to the Glossary. This Glossary constitutes almost three quarters of the volume; it is in the form of a historical dictionary, with the terms listed alphabetically and defined with supporting quotations. It ends with a bibliography of 134 items.

The evidence collected in this volume proves to be of considerable value. The *DAE* quotations for 30 terms have been antedated from two to 73 years; 30 others antedate the *DAE* and *DA* quotations (but not the *OD*) by as much as

79 years—which indicates that some, at least, began to be naturalized in America earlier than was thought before. Many of the examples collected help to sharpen definitions, to show the development of new meanings, and to clarify derivations. The bulk of the neologisms (listed and discussed 32–7), as might be expected, are new compounds, often of quite local application. The author is to be complimented on his use of accentual pattern as the criterion of what is and what is not a compound (35). Obviously, with written sources, this has involved some interpretative reading, but I have almost no quarrel with the conclusions.

The two chief criticisms concern matters of definition and the treatment of variant forms. A considerable number of definitions are unclear, overlap, or include too much or too little. *Alligator hole* has two distinct meanings; they should be separated. In view of the definition of *back water*, *back-water swamp* surely means more than a swamp 'adjoining' back water; is it not 'formed by', or does it not at least 'receive' back water? Under *bay-bush flat(s)* and *bay-gall*, are 'bay shrubbery' and 'sweet bay' the same? This could well be clarified. In defining *blowing spring* the verb *boil* is used without indication that it has not its standard sense. How is one to know that this is not a hot spring? The question is resolved when one arrives at the noun *boil*, but it would have been clearer to put the verb in quotation marks or to give a cross reference.

Chasm: the one referred to is called 'a natural well', but since a well usually contains water and this is dry, it would be better described as a 'well-like hole'. The definition of *dead water* is misleading; it could be corrected by inserting *at before the meeting-place* and *in before a lake*. *Dell* should surely be labeled 'poetic'; all three supporting quotations show the usage to be consciously so. *Field* certainly deserves listing, but I question *floating field* as a separate entry. It is supported by only one quotation, in which 'floating', according to this sense of *field*, is redundant or at best incidental. Appealing to the test of accentual pattern, I doubt whether, in the phrase, *floating* would take more stress than *field*. The definition given for *freestone spring* does not seem derivable from the sole supporting quotation, on the basis of which one might just as well derive 'a spring issuing from a freestone formation'.

Hard marsh: the definition merely repeats the words of two out of the three supporting quotations. Nothing is gained by this; a physiographic definition would be better, or simply 'see quotes.' as in the historical dictionaries. The definition of *Live-oak hammock* is inaccurate since a hammock does not 'consist of' trees; the definition of the equivalent *live-oak hummock* is much better. The word *narrow* does not belong in the definition of *portage*; it is mistakenly transferred from the quotation, where it has a limiting function. *Race*: not clearly stated; the feature cannot be defined both as a 'current' and as an 'island' (and 'key' would be more accurate than 'island' in any case).

There seems to be some inconsistency in the way in which entries are made. Sometimes the variant forms are listed separately though their meaning is the same (*kay*, *key*; *slew*, *slough*, *slue*); sometimes they are merely given together as variants (*marsh*, also *march*, *marish*; *shoal*, also *shoald*). In this last instance another spelling *sholde* is unlisted though it appears in a quotation (1609) on the same page. A number of other variant spellings are not listed: *goll* (for *gall*; quot.

1829 s.v. *palmetto flat*—important for pronunciation), *mash* (for *marsh*; quot. 1874), *meadow* (for *meadow*; first three quotations), *rapids* (as a singular; quot. 1823—very important as an American variant, cf. *woods* and *falls* as singulars), *vallie* (for *valley*; quot. 1784), *canebreak* (for *canebrake*; quot. 1791—potentially a different word). If *hardwood land* deserves entry, why not *hard-timber land*, which occurs in the first quotation under *timber land*? If *cypress swamp*, *bay swamp*, etc. deserve entry, why not *wahoo swamp* (see quot. 1837 s.v. *cove* 2)?

Despite the author's statement (19), the way in which place names are used is not always acceptable. They are not in every case equivalent to topographic terms: a place name is specific and makes an individual reference; the topographic term is generic. *Oat point*, for example, is listed as a topographic term on the basis of one reference to a spot called 'Oat Point'—but the existence of this name does not imply that such a feature as AN oat point was recognized. It justifies *point* as a topographic term, but not the compound. The same criticism holds for the first two quotations of *beech hammock* and perhaps for *bowl*.

The spelling of entries is sometimes unsupported by quotations—so *pine knoll* and *pine plain*; the quotations have *knoll* and *plane*, which are also listed. If these unsupported spellings have been inserted because they are standard, why is *tupelow* the only entry form when *tupelo* is the spelling of both the definition and the quotation?

A number of quotations are unnecessarily repeated. The last under *rush land* has been used under *branch land*, and a reference would be sufficient. One group of expressions, the 'collocations formed with coordinated attributives', are listed separately (30-2); it would have been helpful at least to refer to them in their alphabetic positions in the Glossary. Thus under *ash swamp* we might be referred to *ash and maple swamp*, the first of these collocations, especially as the latter is illustrated with a reference which postdates the references to the former. They need to be considered together.

Some miscellaneous notes: The explanation of *bite* as 'a projection in a river' (22) is not clear: what projects? On the same page, *dismal* may mean 'IN low spirits' but not 'low spirits'. 'Land touching water on two sides' hardly describes a *natural bridge* (40). Footnote 19 lacks page references. *Slide* (for alligators, 47) belongs better with *alligator hole* (48).

Listed as they are here, these objections may seem numerous. Proportionally they are not, nor are they all serious. We are in debt to McMullen for a painstaking collection of materials, which he has analyzed with great care and presented in a workmanlike and welcome study.

The place names of Franklin County, Missouri. By ROBERT L. RAMSAY. (University of Missouri studies, Vol. 26, No. 3.) Pp. 55, with portrait. Columbia, Mo.: University of Missouri Studies, 1954.

Reviewed by FREDERIC G. CASSIDY, *University of Wisconsin*

This book should perhaps not be reviewed here. It is addressed less to the onomatologist than to the general reader, and therefore suppresses or treats tangentially many details without which it is impossible to judge its scholarly

basis. It is informative and entertaining enough, but one has to take many statements on faith and leave many questions unanswered. Nevertheless it deserves attention as the last publication of its author (who did not live to see it through the press), a small bit out of the extensive collection of material on the place names of Missouri to which he devoted so much of his scholarly effort.

In his *Introduction to a survey of Missouri place names*, published in 1934 (*University of Missouri Studies*, Vol. 9, No. 1), with a bibliography by Allen Walker Read and a sample list of names by Esther Gladys Leech, Ramsay set out a model of procedure for American place name study which many have followed since. He set his students to collecting information on the names of the 114 counties of Missouri, overseeing the whole project himself. The present volume follows his usual plan of sorting the names by types: borrowed, historical, personal, topographical, cultural. Ramsey had looked forward to putting his collections at last into the form of a dictionary; see the plan set forth in *Our storehouse of Missouri place names* (*University of Missouri bulletin*, Vol. 53, No. 34; 1952).

To justify the narrative style of the present volume Ramsay writes (9): 'No one reads a dictionary, which is useful merely for reference. Any reader who desires merely to learn the source of some particular place name in the county will find it easy to do so by consulting the index.' Why the purpose of reference should be thought of as 'mere' need not be pursued; yet we may remark that many people READ ABOUT in dictionaries—it is one of the best kinds of reading—and that certainly for the scholar the dictionary's method of presentation—orderly, full, and precise—has its advantages. A lot has been sacrificed here to readability.

To give but one example of questions raised but not answered by this study, I quote (17):

The village of Elmont (sometimes witten Elmo) had its first post office in a store owned by John D. Miller and C. E. Gehlauf. Gehlauf, when interviewed, said he had picked the name out of a postal guide. His partner wanted to name the place Bruno, but he preferred Elmont because he had always taken special pride in making the letter E, and he had his way. Probably it was the Elmont in New York that caught Gehlauf's attention, though there is also one in Texas, and another in California.

This account leaves one feeling that not enough has been said. What, for example, were the dates of establishment of these post offices? Presumably this information is in the files. A rapid check of U. S. postal guides, however, turns up the following: The 'one in California' was never Elmont but El Monte, est. 1875, and therefore anterior to all the Elmonts (N. Y. 1883, Tex. 1885, Mo. 1888). Furthermore, there was an El Monte post office established in another Missouri county (Clinton) long before any of these: some time between 1851 and 1855, and discontinued some time between 1856 and 1859. If this information was in the author's possession, why does it not appear in the account? Is this simplification in the interest of 'popular' presentation? We may be certain, at least, that if Ramsay had been able to edit the materials for a dictionary, he would have insisted upon a greater degree of rigor and exactitude than this little book shows.

As George B. Pace tells us in his well balanced prefatory sketch, Ramsay knew, shortly after his retirement in 1951, that he was incurably ill and would not live to finish the dictionary of Missouri place names. He continued nevertheless to work at it to the end. The collections, virtually complete, remain at the University of Missouri in the form of files. His plan has been published. There is no doubt of the value of such a work to scholarship and to general knowledge; it is to be hoped that some devoted successor may be found who will carry the work to completion. It would be a worthy monument to the man and a matter of pride to his university and state.

Growth and structure of the Egyptian Arabic dialect. By HARRIS BIRKELAND. (Avhandlingar utgitt av Det Norske Videnskaps-Akademi i Oslo: II. Hist.-filos. Klasse, 1952, No. 2.) Pp. 57. Oslo: Jacob Dybwad, 1952.

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This slender monograph is of interest for two reasons: as a by-product of its main theme it offers the first explicitly phonemic analysis in print of the Egyptian Arabic dialect; and it represents the first serious attempt to apply modern structuralist techniques to diachronic analysis of the Arabic language and its various dialects. These two subjects will be dealt with separately below.

The author, who is among the very few scholars working with structural methods in the field of Semitic linguistics,¹ came to study Egyptian Arabic in following up his interest in the pausal forms of earlier Arabic, and this interest shows itself throughout the book. For this reason the title is somewhat misleading: the data studied are primarily features of the phonology connected with pausal forms and word-final phenomena. He also touches on some other features of the phonology, but makes no attempt to consider 'growth and structure' in morphology, syntax, or lexicon.

The Egyptian vowel system presented in Chapter 4 may be summarized as follows. There are five long vowels *ā ē ī ō ū* and three short vowels *a i u*. Since long vowels are always stressed and short vowels are usually unstressed, there is some question as to the phonemic relevance of BOTH quantity and stress. In stressed nonfinal open syllables, however, both long and short vowels may occur. Birkeland therefore suggests a transcription in which stress is always marked, length is never marked, and shortness is marked only in the critical position.

The analysis is clear and accurate, and the transcription is practical. The same analysis was reached independently at the Foreign Service Institute; but in our transcription we have marked length throughout, instead of both stress and (in one position) shortness. Birkeland recognizes this possibility (38-9), but chooses his system because he feels that stress is the primary factor and length secondary. This viewpoint has much to commend it; Birkeland is probably right in assuming that a system based on stress is in the process of replacing the

¹ His previous works include *Akzent und Vokalismus im Althebräischen* (Oslo, 1940); *Altarabische Pausalformen* (Oslo, 1940); *The Syriac phonemic vowel systems, Festschrift til Olaf Broch* (Oslo, 1947). He is now engaged in further study of the historical development of modern Arabic dialects.

earlier one based on quantity. In any case, it is certainly a pleasure to see the lack of contrast between *ē* and *ī* and between *ō* and *ū* in Egyptian Arabic at last noted in print along with the general shortening of unstressed vowels, a distinct improvement on the transcription used by Gairdner and Elder.²

In several instances Birkeland is overcautious. He finds few if any minimal pairs for certain contrasts, such as nonfinal stressed *a* : *ā* (39), *ē* : *ī*, *ō* : *ū* (48), and concludes that although they are in each case separate phonemes, the oppositions are very little utilized. While it is true that these oppositions are less fully utilized in Egyptian Arabic than in other dialects, there are still many minimal pairs. The following may serve as examples. (I mark length, not stress and shortness.)

<i>a</i> : <i>ā</i>	<i>sawa</i> 'together'	<i>sāwa</i> 'he arranged'
	<i>nada</i> 'dew'	<i>nāda</i> 'he called'
<i>ē</i> : <i>ī</i>	' <i>ēš</i> 'bread'	' <i>īš</i> 'live!'
	<i>ḡēr</i> 'birds'	<i>ḡīr</i> 'fly!'
<i>ō</i> : <i>ū</i>	<i>tōb</i> 'garment'	<i>tūb</i> 'repent!'
	' <i>ōl</i> 'talk (noun)'	' <i>ūl</i> 'say!'

The analysis of the 'phonematic diphthongs' (Chapter 5) is also clear and accurate. Earlier Arabic *ay* and *aw* have become *ē* and *ō* respectively, and all instances of the phonemically distinct *ay* and *aw* in modern Egyptian Arabic are either analogical new formations or loanwords from Classical Arabic or other languages.

The author does not give a full treatment of the consonant phonemes, offering as excuses the adequacy of Gairdner's phonetic description and his own inadequacy in the field of the latest 'functional, acoustic view-points'. It is unfortunate that he did not examine at least the problem of velarization. Since Gairdner shows the 'modified' allophones of *a* and *ā* next to certain instances of *r b m* and not next to others, his data would probably have been sufficient for Birkeland to discover the contrasts *r* : *r̥*, *b* : *b̥*, *m* : *m̥* of Egyptian Arabic; and a discussion of the history of these oppositions would have been very interesting.

In his analysis of the development of the Egyptian dialect the author sets forth two important assumptions, which are worth repeating here. The first (7) concerns the relationship of Classical Arabic and the dialects: (a) modern dialects are descended from Classical Arabic, the 'Arabīya, which was a koiné of the

² The phonetic transcription devised by W. H. T. Gairdner with the aid of Daniel Jones and other English linguists is employed in Gairdner's *The phonetics of Arabic* (London, 1925) and *Egyptian colloquial Arabic* (London, 1926), and in E. E. Elder's *Egyptian colloquial Arabic reader* (London, 1927). Though far superior to previous transcriptions of the dialect, it suffers from several glaring inaccuracies; one of these is the constant differentiation between short *e* and *i* and between short *o* and *u*, in spite of the fact that these distinctions are neither phonetically nor phonemically present in the dialect described. Thus, '*ešna* 'our bread' and '*išna* 'we lived', however they may differ morphologically, are identical in pronunciation.

Somewhat better is the transcription used in the revised preliminary edition of the manual prepared for Shell Company employees by T. F. Mitchell, *Egyptian colloquial Arabic* (mimeographed; Cairo, ?1954). Even this transcription, though it satisfactorily indicates vowel shortenings, helping vowels, and elisions, still differentiates short *e* *o* from *i* *u*, and fails to mark the velarized phonemes *r̥ b̥ m̥*, or even the *l̥* which is indicated in Gairdner's system.

Arabic tribes, somewhat artificial, but no more so than any literary language; (b) the development was not always direct, and there are cases where special dialects seem to be the source of particular features; and (c) possibly a later stage of the koiné constitutes the starting-point of the dialects. This is an excellent, conservative statement of the working hypothesis which must be used in this kind of study until convincing counter-evidence is offered.

Second (10), the forms of Egyptian Arabic are almost invariably derived from the PAUSAL forms of earlier Arabic, the only exceptions being the construct form of the feminine ending and a few relic forms. It is important to have this stated explicitly. It is clearly true, and certainly holds for many, if not all, Arabic dialects. Thus, instead of discussing the dropping of final short vowels as a phonetic change between earlier and modern Arabic, it is better to regard this phenomenon as a generalization of the pausal forms. The proof of this is that in cases where more or less than one short vowel is dropped in final position the resultant form invariably corresponds to the pausal form of Classical Arabic.

As a corollary to this the author points out that those final short vowels which have been preserved were 'protected' by a pausal *-h* in earlier Arabic. The reviewer agrees with this, and assumes that others interested in Arabic studies have also reached this conclusion; but it is good to see it stated unequivocally in print.

The basic theme of the study is that the development of Egyptian Arabic is characterized by a succession of at least five well-defined stages, each of which presents a coherent structure. The stages are discussed in every chapter and are summarized briefly in the Conclusion (56-7). Structural linguists will all agree with the aim of describing the history of a language in terms of stages and structural shifts, and the reasoning which the author uses in setting up his stages is generally sound. Since the whole series of reconstructed structures and shifts is of some general interest for linguists and Arabists, it will be recapitulated here.

Birkeland describes each stage in terms of permitted final phoneme sequences and the kind of stress that was present, and gives the structural shifts which characterize each stage. His scheme is summarized in the accompanying table.

STAGE	PERMITTED FINALS	STRESS	SHIFTS
Cl. Ar.	nonpausal -V, - \bar{V} , -C, -CC; pausal - \bar{V} , -C, -CC	none, phonetic or phonemic	
I	- \bar{V} , -C, -CC	same	pausal forms generalized
II	same	automatic word stress probable	$\bar{V}h$, \bar{V}' > \bar{V} (fem. <i>-h</i> only)
III	-V, -C, -CC	same	- \bar{V} , - Vh > -V
IV	same	strong stress	(1) appearance of strong stress (2) $\bar{V}CiCV$ -, $\bar{V}CuCV$ - > $\bar{V}CCV$ - (3) $CiCV$ -, $CuCV$ - > $C(\bar{a})CV$ - (4) unstressed \bar{V} - > -V- (5) VCC - > VCC -
V	-V, - \bar{V} , -C, -CC	stress becoming dominant over quantity	$\bar{V}h$ > \bar{V}

The order of events outlined by the author seems correct, and the stages listed are probably also accurate; but two points which are probably well understood by the author are not made clear. In the first place, the various stages are not of the same importance structurally. From the point of view of phonological structure, Stages I through III are essentially the same. As soon as there was no longer any length contrast in final position it became meaningless to label final vowels in Arabic long or short; the shortening of final vowels was not a phonemic change. From Stage I through Stage IV the dialect had two sets of vowels medially ($\bar{a} \bar{i} \bar{u}$ and $\bar{a} \bar{i} \bar{u}$) and only one set finally ($a i u$). The latter were reflexes of earlier Arabic long $\bar{a} \bar{i} \bar{u}$. It was only with the dropping of final $-h$ after long vowels in Stage V that they became identified with short vowels. Phonetically, however, as Birkeland points out, they were probably shortened before the dropping of $-h$ after short vowels; otherwise the new short vowels might have contrasted with them instead of merging with them. The basic phonological structure, however, did not change during Birkeland's first three stages.

On the other hand, Stage IV could be broken down into substages. All modern Arabic dialects known to me shared the changes listed for Birkeland's first three stages PLUS the changes I have listed as (1), (2) and (3) under Stage IV. Change (4) has taken place in some other dialects, but nowhere in more complete fashion than in Egyptian. Change (5) must form a separate stage at least from the first three, since it presumably took place independently in Egyptian Arabic. The location in time of the appearance of the whole series of regular morphophonemic alternations so characteristic of Egyptian Arabic would be of considerable importance. The dropping of final $-h$ after long vowels (Stage V) has happened in many dialects, but apparently as a parallel development, not as a change shared in common.

In the second place, investigation of permitted finals, stress, and several internal phonetic changes is not enough to characterize fully the phonological structure of the language. The development of other features should also be studied and the results correlated with these in order to have a more complete picture. Investigation of the relative chronology of features like the following could throw considerable light on the development of Arabic dialects in general and Egyptian Arabic in particular (some are treated by the author but none are placed chronologically): disappearance of a final glottal stop after short vowels (as in *qara* 'he read' > *qara*); disappearance of 'V in the initial sequences 'iCC and 'uCC; appearance of stress on the second vowel in the sequence CVCCVCV; the change $t \ d \ \dot{d} > t \ d \ \dot{d}$; and the change $\dot{g} > g$.

But following up all these details would have compelled the author to expand his study very considerably, and would undoubtedly have postponed the appearance of this monograph. Instead of asking for more, we should certainly express our gratitude for what there is. Birkeland sees the problems, understands the methods of attacking them, and gives a very useful presentation of results.

Having expressed fundamental agreement with the author's aims, methods, and results, however, I must draw attention to various details of his analysis

which are questionable, usually because a given phenomenon is assumed to be purely Egyptian when it is in fact widespread in Arabic dialects.

To attribute the coalescence of *ṭ* and *ḏ* with *t* and *d* to the Coptic substratum (54) is clearly not justified. This change occurred also in areas where Coptic was not spoken, and even in areas where the language previously spoken was Aramaic, which probably had the spirants as separate phonemes. This change is in fact characteristic of sedentary Arabic in many parts of the Arab world. Likewise, the assumption that the distribution of *ǧ* and *g* in Egyptian Arabic is based on the Sa'idi and Bohairic dialects of Coptic, although more plausible, certainly requires documentation.

The author's conclusion (26) that the *-š* of Egyptian negation was introduced in the 14th century, which is then a terminus post quem for his Stage III, is surely not as certain as he claims; many points would have to be demonstrated before it could be accepted. First one would have to show that the modern dialects of Egypt and the Sudan are descended from a relatively homogeneous pre-14th-century prototype isolated from other dialects. The author assumes this to be the case, but fails to examine the mass of evidence against it. Second, one would have to take into account the date of the appearance of *-š* in other dialects. At least two possibilities would have to be explored: (1) that the *-š* was present before the 14th century (cf. its presence in Moroccan Arabic) and was lost in the Sudan, or (2) that the *-š* was introduced more recently and spread to various areas including Lower Egypt (cf. the spreading of *-š* in South Lebanon). The appearance of the preceding long vowel could be based on analogy with the treatment of other suffixes or could be borrowed with *-š* from another dialect. Birkeland's date may be correct, but it is by no means proved, and cannot be used as a device for determining the absolute chronology of his Stages.

The coalescence of prepositions with the preceding verb, which is listed as a feature of Stage III (25), is actually limited to one morpheme, *l-* 'to, for', which functions as a verbal suffix in many dialects. The suffixal use of *l-* should probably be attributed to an earlier stage in the development of Arabic, possibly to the late koiné which Birkeland suggests as the starting-point for the development of the modern dialects, although Egyptian Arabic differs from other dialects in having the *-l-* also AFTER pronoun endings (e.g. *'i'milhāli* 'do it for me').

The long vowels, or vowel plus semivowel, of *rāgil*, *huwwa*, *hiyya*, *yīgi* are attributed to a process of lengthening under stress assumed in the development of the Egyptian dialect. This seems unlikely in view of the fact that all of these words occur in similar form in dialects which have had quite different developments in length and stress. The pronouns *huwwa* and *hiyya* should probably be reconstructed in this form for the late koiné; *rāgil* is one of the new formations which have almost everywhere replaced the unusual Classical pattern *raḡul*; *yīgi* may possibly³ come from a dialect when the root was either *'gy* or *gyy*.

The lack in Egyptian Arabic of the final *-a* which might have been expected to develop from the Classical accusative indefinite pause form in *-ā* is explained by saying that it 'has its root in a linguistic system where *-an* in pause had the same fate as *-un* and *-in*' (13); but this fails to place the point in its larger frame.

³ For this possibility see J. Cantineau, *Les parlers arabes du Hōrān* 245-6 (Paris, 1946).

As in the dropping of the final vowels, this is not a question of phonetic change but of analogical extension.

Whenever the pausal forms of a noun in Classical Arabic differentiated more than one case, or whenever a noun had separate pausal forms for definite and indefinite, the form which represented the largest number of categories has invariably been generalized. Typical examples are summarized in the accompanying table; the middle column is the koiné assumed to be the basis of most modern dialects.

CL. AR. PAUSAL FORMS			KOINÉ	MODERN EGYPTIAN
N G indef.	<i>kalb</i>	}	* <i>kalb</i>	<i>kalb</i>
A indef.	<i>kalbā</i>			
N G indef.	<i>ġāl</i>	}	* <i>ġāli</i>	<i>ġāli</i> ⁴
A indef.	<i>ġāliyā</i>			
N G A defin.	<i>ġālī</i>	}		
N	<i>kātībūn</i>	}	* <i>kātibīn</i>	<i>katbīn</i>
G A	<i>kātibīn</i>			
N	<i>kalbān</i>	}	* <i>kalbayn</i>	<i>kalbēn</i>
G A	<i>kalbayn</i>			

This process is sufficient to account for the lack of *-a* in the accusative; there is no need to have recourse to a special dialectal feature in early Arabic. As a matter of fact, the dialectal feature alluded to may have been a reflection of the very process explained here.

Further evidence to support this hypothesis is found in the few words where the accusative *-ā* has survived as *-a*. These are noun forms which have persisted as polite formulas or adverbs, where the accusative form became separated semantically from the other forms of the noun. One such example, *ḥaqqā*, is discussed by Birkeland in a somewhat perplexed way (15); further examples in various dialects are *marḥaba* 'hello' and *'ahla wsahla* 'welcome' (Lebanese), and *dīma* 'always' (Moroccan). The forms with *-an* are not relic forms as Birkeland suggests, but are clearly loanwords from the Classical languages: the relics are the words in *-a*.

The account offered of the origin of the second- and third-person pronominal suffixes (12, 19) is not completely clear. Classical Arabic *-ka*, *-ki* 'you, your', masculine and feminine respectively, are represented in Egyptian Arabic, as in many other dialects, by *-ak*, *-ik*. The author naturally rejects the explanation of simple metathesis which is sometimes given, but his statement that the masculine comes from the Classical accusative and the feminine from the genitive is misleading. The present vowel of the suffix occupies the position of the case ending in earlier Arabic, but in back of this development, as well as that of the masculine third-person singular ending *-u*, is probably the assimilation of the

⁴ Two exceptions are known to the reviewer, where the N G indefinite form of a noun of this type has been the basis of the modern development: Eastern Arabic *'āl* 'fine, excellent' (but cf. *'ālī* 'high') and Western Arabic *wād* 'river' (but cf. Eastern Arabic *wādī*). The former is mentioned by Birkeland (16). A third possible exception is the uninflected form *māš* 'going to' used in Tunisian Arabic, but this may be a modern shortening of the full forms which are also in use in the dialect.

preceding vowel before the final vowel was dropped. In other words, pausal forms with the present vowels must have developed, then the pausal forms were generalized, and finally *-h* was dropped, giving the modern forms. This process is hinted at by the author (19), but could have been more clearly explained. It is summarized in the following table, where parentheses indicate final short vowels dropped with the generalization of pausal forms:

CLASSICAL	INTERMEDIATE STAGE	MODERN EGYPTIAN
N <i>baytuka</i> , G <i>baytika</i> , A <i>baytaka</i>	NGA * <i>baytak(a)</i>	<i>bētak</i>
N <i>baytuki</i> , G <i>baytiki</i> , A <i>baytaki</i>	NGA * <i>baytik(i)</i>	<i>bētik</i>
N <i>baytuhu</i> , G <i>baytihi</i> , A <i>baytahu</i>	NGA * <i>baytuh(u)</i>	<i>bētu</i>

The dynamics of this development are then similar to those described above for the amalgamation of case endings.

The origin of special alternants *-k*, *-ki*, *-(h)* of these pronominal suffixes after stems ending in a vowel is also not made completely clear by the author. The *-k* is a direct reflex of Classical *-k(a)*. The modern *-ka* of which Birkeland speaks is not used in Egyptian Arabic proper, but is found in some varieties of Sudan Arabic and is irrelevant to the discussion. The *-ki* is probably either from **kī*, representing a feature of Arabic dialects differing from Classical,⁵ or an analogical formation based on the masculine, feminine, plural suffixes (zero, *-i*, *-u*) found in the imperative, the imperfect, and as a later development in the perfect (Egyptian Arabic *-t*, *-ti*, *-tu*). In this connection it must be noted that the second-person pronominal suffixes after vowels in Egyptian Arabic are now usually *-k*, *-ki*, *-ku* (cf. Cl. Ar. *-ka*, *-ki*, *-kum*).

Birkeland is to be congratulated on applying structural linguistic methods to the analysis of Arabic dialects; his further work in this field should prove extremely valuable. In the reviewer's mind there is no question that the path toward fuller understanding of Old Arabic dialects and the development of the Arabic language is to be found by an intensive attack on the modern dialects and the rigorous application of current linguistic techniques. The study under review demonstrates unusual knowledge of facts and methods; but a more intimate knowledge of a wider range of dialects would have provided a corrective to some of the main hypotheses, and investigation of additional aspects of the structure of the language would have rounded out the work. It is to be hoped that Birkeland will continue his work in this field and provide us with more and larger studies as stimulating as this one.

Manuel élémentaire d'arabe orientale (parler de Damas). By JEAN CANTINEAU and YOUSSEF HELBAOUI, Pp. 124. Paris: Librairie C. Klincksieck, 1953.

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The spoken Arabic of Damascus has now been treated by two outstanding scholars of Arabic dialectology, G. Bergsträsser¹ and J. Cantineau. Neither

⁵ For an extended discussion of the **kī* possibility see J. Cantineau, *Une alternance quantitative dans les pronoms suffixes sémitiques*, *BSL* 38.148-64 (1937).

¹ G. Bergsträsser, *Zum arabischen Dialekt von Damaskus* (Hannover, 1924).

treatment is an exhaustive study: Bergsträsser's is a description of the phonology with some fifty pages of text material, and Cantineau's is an elementary textbook. But because of the high quality of these two brief studies, the linguist interested in Arabic dialects has more accurate and reliable information about the speech of Damascus than about that of almost any other spot in the Syrian dialect area, in spite of the large number of studies devoted to the dialects of this region.²

The manual under review consists of twenty-five lessons intended to occupy the first year of study. The first lesson is on the sounds of the language. After this each lesson consists of a vocabulary of about fifty words, a brief section on grammar, a short text (*version*) containing words of the vocabulary, and an exercise (*thème*) for translation from French to Arabic. The lessons from 19 on have, in addition, a second short *version*, and there are two longer texts at the end of the book. Many of the texts are adapted from other works on Syrian Arabic, chiefly Bauer's *Das palästinische Arabisch*. The author uses transcription, not Arabic script, and the transcription is almost completely phonemic. The description of the sounds (13-5) is excellent without being too technical, and for the purposes of a student's manual leaves little to be desired. The grammatical sections are also very good; they offer straightforward, accurate explanations without elaborate grammatical terminology and without oversimplification. The texts for reading are all very natural and well chosen.

The lesson vocabularies seem somewhat carelessly prepared. The same word is frequently listed as a new word in two different lessons; one word is even listed twice in the same vocabulary (*q'if* 'weak, sick' 49); another is listed in three lessons ('*aw* 'or' 33, 62, 81). One word is listed twice but with different vowels (*lëra* 'pound' 45, *lira* 77): the usual Damascus pronunciation is *lëra* or *nëra*; *lira* is very widespread elsewhere in Syria and Lebanon, but rare in Damascus.⁴

An introduction setting forth the language situation in the Syrian area and a complete glossary of Arabic words used in the text would have added greatly to the value of the book. It is somewhat surprising that the author did not include these features, since they are found in Brunot's comparable manual for Western Arabic,⁵ which according to Cantineau himself (8) acted as a stimulus to the completion of this volume. The lack of an introduction is especially to be regretted, since in all probability Cantineau is by far the best qualified scholar in the world to write such an introduction.

As an elementary textbook for use in a university course, this book can be heartily recommended; it has few flaws in the actual language material, and these chiefly in minor points. Most of them are covered in the detailed criticism

² One unpretentious study deserves mention because of its accurate data and in spite of its unreliable interpretation: Msgr. David, *Etude sur le dialecte arabe de Damas*, *JA* 8:10.115-99.

³ The transcription in this review is that of the book, except that *x*, *h*, and ' are used for the voiceless velar spirant, the voiceless pharyngeal spirant, and the voiced pharyngeal spirant respectively.

⁴ The data presented and the views advanced in this review are based on a five-year acquaintance with the dialect and on work with six informants. I hope to publish a full description of Damascus phonology soon.

⁵ L. Brunot, *Introduction à l'arabe marocain* (Paris, 1950).

below. The body of this review is devoted to a treatment of some features of Damascus phonology and morphology which are either ignored or misunderstood by Cantineau in the careful phonemic and grammatical analysis which obviously underlies the pedagogical presentation in the manual; it is intended to be a constructive linguistic discussion, not a critique of the textbook as such.

In the phonology several points call for comment: the 'foreign' phonemes, the treatment of velarized consonant phonemes, the short-vowel pattern, and stress and intonation.

The phoneme *v* (13) occurs in many common words besides *krāve* 'tie', such as *brāvo* 'bravo', *narvaz* 'he upset (someone)', *verānda* 'balcony'. The phoneme *p* is less frequent, but occurs in words regularly used by educated speakers, e.g. *'awruppa* 'Europe'. Furthermore, at least two additional vowels occur in the ordinary speech of educated Damascenes *ō* and *ō̄* (French *on* and *eu*). These appear usually in the final syllable of nouns, and are often replaced by *ōn* and *ō̄* respectively when a suffix is added; morphologically they function as *ōn* and *ō̄*. Examples: *garṣō* 'waiter' (pl. *garaṣīn*, f. *garṣōne*); *daktōr* 'doctor' (du. *daktōrēn*, pl. *dakātra*).⁶

The phonological function of velarization in Arabic dialects is very difficult to describe, and is invariably one of the major topics discussed in the presentation of the phonology of a given dialect.⁷ Cantineau has frequently dealt with this problem in his writings, and has made some valuable contributions to the analysis of it. In this manual he apparently recognizes six velarized phonemes *q l r s t z* for the dialect; *l* is not mentioned in the list of emphatics (14), but appears in the text (e.g. *'an šā ḷāh* 45, *waḷḷāh* 85), and *z* is included in the list but sometimes missed in the text (e.g. *zarīf* 77).

In addition to the velarized consonants listed, *b* and *m* must be recognized as separate phonemes in Damascus, as they are in most of the Syrian dialect area. On the other hand, Damascus Arabic is unlike much of Syrian Arabic in having an independent *n* phoneme, and in apparently not having *r* as a separate phoneme. With some contriving, minimal pairs can be elicited for *b* and *ḅ*, *m* and *ṡ*, and *n* and *ṣ*; it is apparently impossible to find such a pair for *r* and *ṣ* in this dialect. The best examples I can cite are *bāba* 'her door' : *ḅāḅa* 'daddy, Pope'; *mayy* a girl's name : *mayy* 'water'; *nāyek* 'having sexual intercourse' : *ṣāyek* 'your (f.) flute'. Negatively, pairs like *žāri* 'flowing' : *žāri* 'my neighbor', common throughout the Syrian area, seem not to occur in Damascus Arabic.

The phonemic status of *b m n* is quite clear, however, even without these minimal pairs. Most occurrences of these sounds are in words which contain other velarized phonemes (e.g. *ḅaṣṣ* 'he looked', *ḅuuz* 'ice', *bēd* 'eggs'; *maṣṣ* 'he sucked', *mādi* 'past', *maṣṣa* 'factory'); but there are words with *b m n* without other velarized consonants (e.g. *ḅank* 'bank', *ḅāy* 'Bey (of Tunis)'; *maama* 'mommy', *mayy* 'water'; *lokānda* 'restaurant', *nāy* 'flute'), and there are words containing nonvelarized *b m n* with velarization elsewhere in the word (e.g.

⁶ Further investigation may reveal still other phonemes. The phonemic analysis of incompletely assimilated loanwords has still to be solved.

⁷ For an interesting description of velarization in a Syrian dialect see Haim Blanc, *Studies in North Palestinian Arabic* 52-73 (Jerusalem, 1953).

ḥarnāʿa 'hat', *'abyaḍ* 'white'). It is sufficient to establish a phoneme that a sound occur in surroundings in which it cannot reasonably be identified with any already established phoneme in the language—a principle which the author seems to accept elsewhere when he uses the symbol *v* in *krāve*.

The contrasts and distribution of short vowels in Damascus Arabic are complex and difficult to determine. Cantineau's analysis is good and his transcription better, but he has still failed to understand the system completely. He posits five long vowels /ā ē ī ō ū/, five 'medium' vowels in final position written with the symbols for the long vowels minus the macron, and three short vowels /a e o/, of which *e* and *o* are often neutralized and represented by *ə*.

The first clue to the actual vowel system is the occurrence of *a e i o u* both short (his 'medium') and long in final position. Cantineau sidesteps this by analyzing the final long vowels as long vowel plus *h*, and saying (38) that the final *-h* of the third person pronoun ending is very weak. This is simply not so: there is normally no final *-h* at all for the pronoun ending. There are dialects of Arabic where the final *-h* of the pronominal suffix is preserved after long vowels (e.g. Moroccan Arabic in general); there are dialects in an intermediate stage, where utterance-final *-h* has disappeared phonemically but all word-final long vowels regardless of origin have as variants long vowels plus *-h* (e.g. Cairo Arabic); and there are many dialects where final *-h* has disappeared completely and has then been restored analogically in words where it is part of the root, thus making possible contrast between final long vowel and final long vowel plus *-h* (e.g. Baghdad Arabic). Damascus Arabic, like all sedentary Syrian Arabic dialects known to me, belongs in this last category. Because of the rarity of actual final *h*'s it is difficult to find a minimal pair; perhaps the best that can be done is something like *nabīh* 'alert' (also a man's name) : *nabī* 'his prophet', a fairly unlikely word.

The phonemic status of short *i* and *u* would be more difficult to ascertain without recognition of their occurrence in absolute final position. Apart from *i* < **iy* and *i* < **yi*, all the instances of medial *-i-* and *-u-* are in loanwords, chiefly from Classical Arabic, and in any case these phonemes are relatively rare. Examples of unstressed medial *i* and *u*: *birūh* 'he goes' (< **iy*), *nasito* 'she forgot him' (< **yi*), *bināye* 'building' (Classical loan); *mudīr* 'director, manager' (Classical loan). Cantineau writes unstressed medial *i* as *y* or *ī* and apparently rules out medial *u* as a Classicism.

Stressed short *i* and *u* are even rarer, occurring chiefly in Classical loanwords; some of these loanwords are very common, however, and the contrast should have been observed. Cantineau writes *ə* for both. Examples of nearly minimal contrast in common words in the dialect are *'iza* 'if' : *'əʒa* 'he came'; *luḡa* 'language' : *šəḡəl* 'work'. The error in the analysis of *i* and *u* is not serious pedagogically, because in the position of greatest frequency (finally) they are correctly shown in the transcription, and elsewhere they are relatively rare.

Somewhat more serious is Cantineau's failure to recognize *ə* as an independent phoneme. Many occurrences of *ə* he regards as automatic, hence subphonemic: 'sans valeur grammaticale ni lexicale et son omission n'est pas une faute' (14). This may be true of the helping vowel *ə*, although even this offers some problems

in establishing the conditions of its occurrence, but it is not true of the *a* in a final syllable before a single consonant. In this position *a e o a* occur: *xabaz* 'he baked', *byaxboz* 'he bakes', *xaabez* 'one who has baked', *xabaz*, *xabz* 'bread'. In most words with *a* in this position a free variant without *a* also occurs, but contrast is still possible. The simplest proof of the contrastive value of this vowel lies in the cases where it is or is not permitted in a final geminate cluster. For example, the word for 'I passed, entered' is freely either *fatat* or *fatt*, but the imperative 'crumble' is *fatt*, never **fatat*.

A fairly serious slip is the writing of *e* and *o* for stressed *a* in many words. It is not enough to note in the first lesson that other than in the final syllable '*e et o ont tendance à se confondre en a*' (15). The fact is that every instance is clearly one or another of these vowel phonemes; there is not a vague 'tendance' to be noted, but rather an important general phenomenon of the language. Unstressed *e* and *o* are replaced everywhere and without exception by *a* when by a change in the form of the word, usually by the addition of a suffix beginning with a consonant, they become stressed. Forms like *hmali* and *ktabi* are given correctly (22) but *byahmalni* and *byaktab(h)on* are written with *e* and *o* in the same sentence with the correct *xadamna* (42), and there are many examples of this. Also, the stressed combining form of the feminine ending is *at*, not *et* (43); and *ilet* and *tmen* should read *ilat* and *tman* (62).

In Damascus Arabic the position of fewest vowel contrasts is that of the medial stressed vowel; here only *a* and *a* occur, except in loanwords. The positions of greatest number of contrasts are absolute final unstressed (*a e i o u* all frequent) and in pre-tonic open syllables (*a i o u a*, of which *a* is by far the most frequent). This phenomenon of reduction of the number of contrasts under stress is of considerable theoretical interest, since linguists often tend to assume that if there is phonemic stress in a language the unstressed vowels tend to be weakened. In many dialects in the Syrian area the reverse is true. In much of Lebanese Arabic, where there are only three short vowel phonemes /*a i u*/, the *i-u* contrast disappears under stress, *u* being replaced by *i*. In some dialects in South Lebanon, where the three-way contrast exists under stress, the phonetic distance between the phonemes when stressed is less than when unstressed, so that it may be harder for an American to tell *i* from *u* when stressed than when unstressed. This 'muddying' effect of stress and the conditions under which it occurs deserve further study in Arabic dialects and elsewhere in the languages of the world.

In general, Cantineau has not handled the question of stress satisfactorily. First, he dismisses it as not a significant feature of the phonology: '*L'accent de mot est faiblement marqué et ne sert à différencier des significations ni par sa place ni par sa nature*' (15). Second, he misses several phenomena associated with stress. With regard to the significance of stress in the sound system, it may be noted that the position of WORD STRESS is almost completely automatic, and it is difficult to find even near-minimal contrasts; but the position of PHRASE STRESS is highly variable, has differentiative function, and should be examined in connection with syntactic constructions.

The position and phonetic quality of word stress were ably described by Bergsträsser (op.cit. 33), and could have been usefully summarized in several

sentences; Bergsträsser also described some of the more important features of phrase stress (44-5), such as the existence of the phonemic phrase ('Akzenteinheit') and the heavier stress on *ġēr*, *kall*, and *mā* in certain types of phrases. The inclusion of these points, with perhaps the addition of several others (such as the heavy stress on a cardinal number preceding its noun and the reduction of stress in questions), could have been mentioned by Cantineau easily and with obvious profit to the student.

With regard to phenomena associated with stress, two striking features may be noted, both of which are ignored by Cantineau: (1) the replacement of *é* and *ó* by *a* and the general muddying effect of stress discussed above, and (2) the shortening of certain unstressed long vowels. The latter has been observed in many dialects, is widespread in Damascus Arabic, and was noted by Bergsträsser (op.cit. 29-30); Cantineau marks such vowels long and does not mention the phenomenon.

As a final criticism on phonological matters, I must draw attention to the complete absence of any reference to intonation. This is, of course, not unusual even in good language textbooks, and could be justified on the grounds that the phonological analysis of intonation is not as generally agreed upon as that of the 'segmental' phenomena. The intonation of Damascus Arabic is striking, however, and is often imitated by outsiders in mocking Damascus speech; for this reason if no other, at least a few comments are in order. Again Bergsträsser has provided a good preliminary discussion³ which could have served as a point of departure for Cantineau.

In morphological matters there are a few inaccuracies, chiefly connected with verbs and numbers, or else relatively minor errors in the transcription of miscellaneous forms.

The long vowel in the masculine imperative of sound primary verbs when standing alone (21, 22) is a normal long vowel, indistinguishable from long vowels elsewhere in the language; there is no reason for Cantineau's statement, 'la voyelle ... est souvent allongée. On ne tiendra pas compte de cet allongement qui n'est pas grammaticale ...' Compare *lġēt* 'turn' and *lġēt* 'the house', *ftāh* 'open' and *maftāh* 'key', and hundreds of similar pairs. The same stricture applies to the imperatives *xōd* 'take' and *kōl* 'eat' (98, 102). Occasionally, especially in the *versions*, the correct long vowel is indicated (e.g. *xōd* 92). All other imperatives are described accurately, except for those of the verb '*aṭa* 'give' (22), which should read '*aṭi*, '*aṭu*, not '*aṭi*, '*aṭu*. The only other irregular imperative in Damascus Arabic could have been given too, since it is a frequently used word: '*ōa* 'watch out, beware!' from *wāḡi*.

Secondly, the shortenings before the *-l-* suffix 'to, for' are not accurately explained (46, 54); they are more extensive than the author states: CVC before *-l-* is always shortened to CVC (*ā* > *a*; *ī*, *ū* > *ə*). This is true not only of the forms listed by Cantineau but also of forms like *ḡab^ol^a*, where he denies it (46), and of many others (e.g. passive participles) which are not mentioned at all. It is a very general phenomenon. The alternants *-all-* and *-all-* of the suffix itself are not described at all, though *-all* is frequent in the language and the occurrence

³ Op.cit. 43-50 (Die Satzmodulation).

of both is very precisely determined by the forms of the verb to which they are suffixed. I noted only one instance in the text: *maddallo* in a *version* on 108; this particular form is usually *maddallo* in Damascus.

Verbs of *wCC* roots (29, 98) also have an imperfect of the type *yəCaC* which is quite common in Damascus though rare in sedentary Syrian Arabic as a whole. Finally, *ntala* (listed in the vocabulary, 65) is another example of the form which, it is said (83), 'n'est attesté que pour *lla'a*'.

The complicated forms of the cardinal numbers from 3 to 10 are on the whole well presented under the rubrics of first, second, and third forms (62, 63). There is, however, a fourth set of forms used before *miyye* 'hundred', and by some speakers also before *wā*, the plural of *w'iyye* '200 grams'. One example of this set appears unexplained in the text (*tlāt miyye*, 75). Also, '*ida's* 'ele. 3n' is more common in Damascus than the form *hda's*, which is the only one given (67). The ordinals are listed correctly (87) except for 'first'. Here the form '*awwal* is listed as both masculine and feminine, but this is wrong. All ordinals, in addition to participating in the construction Cantineau explains, may also PRECEDE the noun, in which case the masculine form of the ordinal is used regardless of the gender of the noun. When the word for 'first' comes AFTER a feminine noun, it is either the Classical '*ūla*, or a new formation like '*awwalāniyye*.

The minor corrections listed here exclude mere misprints and careless slips. All items listed are of frequent occurrence; the page references give only the first or chief occurrence. For *yəlli* (22) read *yalli* (relative); for *naḥna* (34) read *nəḥna* 'we'; for *rāḥ* (91) read *raḥ* (verbal prefix, 'going to'). The corrected forms are widely current throughout the area, not peculiar to Damascus. Beside *raḥ*, however, the variants *raḥa* and *laḥ(a)* are common in Damascus itself.

For '*ən šā lḥāh* (45) read *nšālla* 'God willing', and for *l-ḥamd əllāh* (ibid.) read *lḥamdəlla* 'praise be to God'. These expressions are now single words in Syrian Arabic, with stress on the penultimate syllable. Damascus differs from most of the area in having long *ā* in the first word, which is usually *nšālla* elsewhere. Occasionally longer forms of these and similar expressions occur as Classicisms.

For '*ənd* (17) read '*and* 'chez'; for '*alayyi* (45) read '*aliyyi* 'on me'; for *ḥādōlīk* (59) read *ḥəndənk*, *hadənk* 'those'. Cantineau's forms are more typical of the area as a whole, and may be heard occasionally in Damascus; but the corrections are more typical of the city.

One inaccuracy in syntax should be mentioned. The two phrases *kān byəktob* and *kān yəktob* (90) are not equivalent in meaning. The former, with *b-*, means 'he would have written', and usually appears after an if-clause, less often independently; the latter means 'he used to write' or the like.

Einführung in die altaische Sprachwissenschaft: II. Formenlehre. By G. J. RAMSTEDT, ed. by PENTTI AALTO. (Mémoires de la Société finno-ougrienne, No. 104.2) Pp. 262. Helsinki: Suomalais-Ugrilainen Seura, 1952.

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As a result of the great progress which has been made in Altaic linguistics during the past few decades, it has now become possible to compile an outline

of the comparative grammar of the Altaic languages. This progress is due primarily to the work of Ramstedt, who, unlike his predecessors in the field, emphasized the necessity of comparing only those words and grammatical forms which exhibit the working of regular phonetic laws.

The language family traditionally referred to as Altaic consists of Turkic, Mongolian, and Manchu-Tungus. Korean has also been shown by Ramstedt to be a member of this family.¹ Whereas a few decades ago there was still room for doubt about the relationship of various Altaic languages,² a sufficient number of regular phonetic correspondences have now been established³ so that today only the most skeptical will doubt the relationship of the languages which we call Altaic.⁴

Although Ramstedt's book is devoted exclusively to morphology, and contains no discussion of comparative phonology,⁵ I would like to present the reader with a number of etymologies which demonstrate that there are regular sound correspondences between Turkic, Mongolian, Manchu-Tungus, and Korean. I shall confine myself to two particular cases, initial **p* and initial **s*.

In one of his articles,⁶ Ramstedt showed that initial *f* in Manchu and *p* in Nanai (Goldi) corresponds to *h* in some Tungus languages, and to zero in both Mongolian (as known at that time) and Turkic. We now know that in Monguor, a Mongolian language spoken in Kansu province of China, *f* and *χ* correspond (in different positions) to Manchu *f*.⁷ Middle Mongolian (13th to 15th centuries) shows *h*-corresponding to these sounds.⁸ Ramstedt gave forty-two examples of this correspondence, of which twenty are irreproachable. At present, I can add twenty more examples which seem beyond doubt.

Ma.⁹ *fača*- 'separate, go asunder',¹⁰ Mo.¹¹ *ača* 'fork; ramification', Urd.¹² *ača*,

¹ Ramstedt, *Studies in Korean etymology* (Helsinki, 1949); *A Korean grammar* (ib., 1939); *The nominal postpositions in Korean* (ib., 1933); *Remarks on the Korean language* (ib., 1928); *Über die Stellung des Koreanischen* (ib., 1951); Koreanisch *kəs* 'Ding, Stück', *Journal de la Société finno-ougrienne* 48.4 (1937).

² J. Németh, *Die türkisch-mongolische Hypothese*, ZDMG 71.549 ff. (1912).

³ For instance, Turk. *z* = Mo. and Tung. *r*, Kor. *r/l*; Turk *š* = Mo. and Tung. *l*, Kor. *r/l*. Cf. Martti Räsänen, *Materialien zur Lautgeschichte der türkischen Sprachen* 25 (Helsinki, 1949); Władysław Kotwicz, *Studia nad językami altajskimi* 1-318 (Kraków, 1953).

⁴ Johannes Benzing, *Einführung in das Studium der altaischen Philologie und der Turkologie* 9 et passim (Wiesbaden, 1953).

⁵ Part 1 of *MSFOu.*, Vol. 104, which will appear in the next year or two, is to be devoted exclusively to the comparative phonology of Altaic.

⁶ Ramstedt, *Ein anlautender stimmloser Labial in der mongolisch-türkischen Ursprache*, *JSFOu.* 32:2.8 (1916).

⁷ A. de Smedt and A. Mostaert, *Le dialecte monguor parlé par les mongols du Kansou occidental*: 1. Phonétique, *Anthropos* 24/25.805; Mostaert, *The Mongols of Kansu and their language*, *Bull. Cath. Univ. Peking* 8.85 (1931).

⁸ P. Pelliot, *Les mots à h- initiale, aujourd'hui amuie, dans le mongol des XIIIe et XIVe siècles*, *JA* 1925.193 ff.

⁹ Manchu after the dictionary of Zakharov.

¹⁰ Because of its *-č-*, this cannot be connected with Ma. *faču* 'ramification'.

¹¹ Written Mongolian after the dictionary of Kowalewski.

¹² Urdus (Ordos) after Antoine Mostaert, *Dictionnaire ordos* (Peking, 1941-4).

Kh.¹⁸ *aca*, Bur.¹⁴ *asa*, Kalm.¹⁵ *acv*.¹⁶ — Ma. *fajan* 'excrement' (< **fargan* < **fargal*), MMo.¹⁷ *haryal* 'dry dung serving as fuel', Mo. *aryal*, Mong.¹⁸ *χargar*, Urd., Kh. *argal*, Alar-Bur. *argādahan*, Kalm. *aryosn* 'dry dung' (< **argalsun*).¹⁹ — Ma. *faqča* 'burst, crack', Lam.²⁰ *hěskandāi* 'crack', MMo. (Secret History) *hay-* 'become dry, coagulate', Mo. *aysi-* 'shrink', Urd., Kh. *agši-* 'dry up, become dry; be contracted', Bur. *agša-*, Kalm. *akši-/akči-* 'shrink, be contracted'. — Ma. *faqsa* 'sudden, unexpected, vehement', Mo. *aysum* 'vehement, tempestuous', Urd. *agsum*, Kh. *agsam*, Kalm. *aksn*. — Ma. *fasi-* 'stick to, adhere', Mo. *asa-* 'stick to', Kh. *asa-*, Bur. *aha-* 'adhere, stick to', Kalm. *as-* 'stick to', Turk.²¹ *as-* 'hang, suspend'. — Kor.²² *padak* 'ground, floor; palm of the hand, sole of the foot', Ma. *fatan* 'bottom, sole' (< **patan* < **patak*), Mo. *adaγ* 'end', Kh. *adag*, Turk. *adaq*, *azaq*, *ayaq* 'foot'.²³ — Ma. *fatxa* 'paw of an animal, claws of a predatory bird',²⁴ MMo. (Secret History) *hadqu-* 'hold in the hand', Urd. *adxu* 'handful; fist', Kh. *adxa*, Kalm. *atxv*, Turk. (Tuva and Karagas) *adiš* 'handful' (< **atič*). — Ma. *faχala* 'dregs, sediment; thick wine' (< **payara*), Mo. *ayarča* 'sour cheese, dregs, mash' (< **pawar-ča*), Kh. *ārca*, Bur. *ārca*, Kalm. *ārco* ~ *ārmog*; cf. Mo. *ayarul* 'sour cheese', Kh. *ārūl* (< **ha'aru'ul* < **pawar-u-yul*), Turk. (Shor etc.) *abirtqi* 'a kind of barley beer'.²⁵ — Ma. *fele-* 'act in an insolent manner, be impudent', Mo. *eleg* 'mockery, derision', Urd., Kh. *eleg* 'derision; target of someone's mockery', Kalm. *elag*; cf. Mo. *elemeg* ~ *elmeg* 'scoundrel, brute', Mo. *elmer*, Kalm. *elmr* 'swindler'.²⁶ — Ma. *foqfixiyan* 'given to anger, flying into a violent passion', Mo. *oyčim* 'given to anger', Kh. *ogcom* 'brusque, rough', Kalm. *okčn* 'steep, precipitous'. — Ma. *fituxan* 'lute', Mo. *yatuyan* ~ *yatayan* (< **itayan* < **hituyan* < **pituyan*), Urd. *yatuga*, Kalm. *yatvγv* 'harp, lute'.²⁷ — Ma. *foxolon* 'short', Mo. *oqor* (< **oqar* < **hoqar* < **poqar*), MMo.

¹⁸ Khalkha after A. R. Rinčine, *Kratkij mongol'sko-russkij slovar'* (Moskva, 1947).

¹⁴ Buriat after K. M. Čeremisov, *Bur'at-mongol'sko-russkij slovar'* (Moskva, 1951).

¹⁵ Kalmuck after G. J. Ramstedt, *Kalmückisches Wörterbuch* (Helsinki, 1935).

¹⁶ Ramstedt connects Mo. *ača* with Turk. *ač-* 'open', Tung. *ačo-yini* 'take off', and Kor. *ačham* 'morning', probably thinking of the morning as the time when the day 'opens' (see *Studies in Korean etymology* 3-4), but I cannot accept this.

¹⁷ Middle Mongolian.

¹⁸ Monguor after de Smedt and Mostaert, *Dictionnaire monguor-français* (Peiping, 1933).

¹⁹ Cf. Ramstedt, op.cit. 185. Ma. *-j-* originates from **-rg-* in numerous other words, e.g. Ma. *nujan* 'clenched fist' = Tungus *nurga* < **nudrga* = Mo. *nidur-γa*, Kh. *nudarga*, Turk. *yudruq* 'fist'; Ma. *bujan* 'forest' = Mo. *burγasun* 'willow-plot, brushwood', etc. Similarly **-rk-* > Ma. *č*, e.g. Ma. *uče* = Tung. *urkə* 'door'.

²⁰ Lamut (Even) after V. I. Cincius and L. D. Rišes, *Russko-evenskij slovar'* (Moskva, 1952).

²¹ Turkic.

²² Korean; see Ramstedt, op.cit. 180.

²³ Cf. my review of Ramstedt's work in *Harvard journal of Asiatic studies* 13.577 (1950).

²⁴ Ramstedt compares *fatxa* with Ma. *fatan* 'sole', but I separate these two words.

²⁵ Cf. Ramstedt, *Kalmückisches Wörterbuch* 21; cf. B. Ya. Vladimircov, *Sravnitel'naya grammatika mongol'skogo pis'mennogo jazyka i zalsaskogo narečiya* 209 (Leningrad, 1929).

²⁶ Mo. *eleg*, *elemeg*, and *elmer* are derived from the verbal stem *ele-* < **pele-*. Cf. Ramstedt, *Über die Konjugation des Khalkha-Mongolischen* 103, 115, 95 (Helsinki, 1903).

²⁷ Cf. Ramstedt, *Kalmückisches Wörterbuch* 217.

(Leiden MS.)²⁸ *hoqar*, Mong. *xuquor*,²⁹ Kh. *oxor*, Urd. *ogjom*, Bur. *oxotor* 'having a short tail', Kalm. *oxr* 'short', *oxtr* 'short-tailed'; cf. also Kh. *ōdoγ* 'short, squat, having a short tail, tailless' (< **oyadun*), Mo. *ogotuna* ~ *oyotuna* 'a rodent like a rat with no tail', Kh. *oxotono* > Ma. *oxotono*. — Ma. *folo-* 'carve, scabble' (< **fōlō-* < **fōgele-*), Mo. *ōgele-* 'chip, make even or smooth', Urd. *ōlō-*, Kh. *ōl-* 'cut off', Kalm. *ōl-* 'chip, make smooth'. — Ma. *fulaqčan* 'a small bag for the tinder-box', Tung. *hula* 'punk', Mo. *ula*, MMo. (Muqaddimat al-Adab) *hula* 'tinder, punk', Mong. *fula*, Urd. *ula*, Kh. *ulla*, Kalm. *ul*. — Ma. *fumere-* 'put in disorder, cause an upheaval' (< **fūimere-*), Mo. *ūimege-* 'cause disorder', *ūimegen* 'revolt, disorder', Urd. *ūime-* 'be in disorder, run to and fro', Kh. *ūime-*, Kalm. *ūma-*. — Ma. *furdexe* 'furs, merchandise consisting of furs', MMo. (Secret History) *hūrtesün* 'rags of silk', Mo. *ūrtesün* 'rag, patch', Kh. *ūrtes*, Bur. *ūrteheγ*. — Ma. *furgi-* 'boil, stream, gush, well', Lam. *huyūdāi* 'boil', Tung. *huyud'e-* 'boil', Barguzin Tung. *huyu-*, Solon *ūyi-*, Mo. *orgil-* 'boil, bubble (e.g. of a spring)', Mong. *šd'zieli-* 'boil', Kh. *orgil-*, Bur. *oryol-* 'boil, well' (< **orgil-*). — Kor. *phulda* 'rub, grind down', Ma. *furu-* 'cut into small pieces', *furuku* 'rasp, coarse file', Mo. *ūrū-* 'rub, file', MMo. (Secret History) *hūrū-* 'sharpen (an arrow)', Urd. *ūrū-* 'sharpen', Kh. *ūr-*, Bur. *ūre-*, Kalm. *ūr-*, *ūrteγ* 'sawdust' (< **ūrūdesün*), Turk. *üz-* 'tear, cut'.³⁰ — Kor. *putta* 'pour', *ppūmda* 'sprinkle water', **pis-*, Ma. *fusu-* 'sprinkle, strew upon someone', Goldi *pisa-yni*, *pisiuri*, Olcha *pisūri* 'squirt; water plants', Lam. *hustāi* 'sprinkle', Mo. *ōsūr-* 'sprinkle', Mong. *fudzuru-* 'pour',³¹ Kh. *ōsōr-*, Kalm. *ōsr-* 'sprinkle, jump upward'.³²

The following examples will illustrate the developments of initial *s. — Ma. *sa-* 'know', Tung. *sā-*, Lam. *hā-*, Solon *sāgildi-* 'be worried' from *sā-* 'know', Mo. *sana-* 'think', Kh. *sana-* 'long for'; cf. Mo. *saki-* 'protect, be watchful' (< **saqi-*), Turk. (Uighur) *sa-* 'count', *san* 'number', *sana-* 'count', *saqin-* 'be sad, mourn'.³³ — Ma. *sele* 'iron', Solon *sālē*, Lam. *hāl*, Solon *sāldi* 'armor', *sālēmāi* 'sabre, sword', Mo. *selme* 'sword', Kh. *sellem*, Bur. *heleme*. — Tung. *sāl-* 'awaken', Mo. *sere-*, Kh. *sere-*, Bur. *here-*, Turk. *sāz-* 'feel'. Cf. Tung. *səri-* 'awaken' < Mongolian. The primary root is *se-*; cf. Ma. *se-* 'speak, say', Mo. *se-d-ki-* 'think', *sefig* 'doubt' (< **se-d-i-g*), *sedūb* 'plan' (< **se-d-ū-b*), etc. — Solon *sərbī* 'sharp', Mo. *serege* 'fork, gavel', *serbege* 'sharp fins of fish'.³⁴ — Tung *sigi* 'dense forest', Lam. *higi* 'forest', Mo. *siγui* 'grove, forest', Kh. *šugui*, Bur. *šugi* 'forest'. — Tung. *sirugi* 'sand', Solon *širuktāā*, Mo. *siroi* 'earth, dust' (< **siruyai*), MMo. *širu'ai*, Kh. *šoroi* 'dust, earth'. — Ma. *sike* 'urine', Solon *šixē*, Mo. *sigesün* 'urine', *sige-* 'urinate', Kh. *šē-*, Turk. *sig-*, *siy-*, etc. — Tung. *sila-* 'fry in a pan', Solon *šila-*

²⁸ Poppe, Das mongolische Sprachmaterial einer Leidener Handschrift, *Bulletin de l'Académie des Sciences de l'URSS* 1928.76.

²⁹ De Smedt and Mostaert, *Dictionnaire monguor-français* 179.

³⁰ Ramstedt, *Studies in Korean etymology* 216.

³¹ Ramstedt, op.cit. 212.

³² Ramstedt, *Kalmückisches Wörterbuch* 301.

³³ Ramstedt, *Studies in Korean etymology* 223. Ramstedt does not compare *sana-* with *saki-*, *saqin-*, etc.

³⁴ Ramstedt compares Mo. *serege* 'trident, three-pronged fork' with Ma. *sertei* 'triple' and Kor. *se* 'three'; cf. op.cit. 225. But this etymology is doubtful.

'fry', Mo. *sira-*, Kh., Bur., etc. *šara-*. — Tung. *silgin-* 'shake, tremble, shiver', Mo. *silgüd-* 'shake', Turk. *silk-*. — Ma. *solori* 'polecat', Solon *sōligi*, Mo. *sologya*, Bur. *holongo*.³⁵

From these examples we may set up the phonetic law that *s- > Manchu s, Tungus s (in some dialects š), Lamut h, Mongolian (except Buriat) s, but š before i, Turkic s (Bashkir h, Yakut zero, etc.).³⁶

In the *Einführung*, Ramstedt has established many correspondences in the morphology of the Altaic languages. These are agglutinative, showing considerable resemblance in structure to the Uralic languages. Changes in the forms of words are brought about by suffixation only. The suffixes are sometimes added directly to the stem, but sometimes a stem-final consonant is dropped or assimilated before a given suffix, e.g. Yakut *at* 'horse' + *-qa* results in *aqqa* 'to the horse', Mo. *jaɣal* 'horse with dark mane and tail' + *-γčīn* (a feminine suffix) results in *jaɣaγčīn*. It is known, however, that in Middle Mongolian many more suffixes were added mechanically to the stem than at the present time, e.g. *qonin* 'sheep' + *-d* plural + accusative *-i* results in *qonindi*, whereas written Mongolian of a later date has only *qonidi*.

Ramstedt is certainly right in regarding some of these suffixes as having originated from independent words, e.g. Tung. *-tki* in *jūtki* 'toward the house' < **jū* 'house' + *tiki* 'direction' (= Mo. *čike* < **tiki* 'straight; direction', Turk. *tik* 'straight, direct, erect', 51), or the reflexive-possessive suffix in Mo. *-ban/-ben* < a pronoun = Tung. *mān* 'self' (73), but I disagree with Ramstedt when he connects the locative suffix **-da* with Korean *taɪ* (*dai*) 'place' (37), or the lative suffix **-rū* with Chinese *lu* 'road, way' (49). It is very doubtful that foreign words such as these should have become grammatical endings in other languages. In my opinion Ramstedt's theory of the origin of suffixes from independent words, as it is applied in the *Einführung*, raises many doubts and constitutes the weakest part of his book.

Of the common Altaic suffixes which Ramstedt treats, I should like to discuss first of all those of the nominal declension. I shall use names such as 'genitive' and 'accusative', although the functions of the forms in question are not the same as those of the Latin cases.

The genitive (or possessive) has the suffix **-n*, which becomes Ancient Turkic *-ŋ*, Mo. *-n* (in *-u-n*, *-y-i-n*), and Tung. *-ŋi* < **-n-ki*. (This element *-ki* is an adjectival ending which we find also in Turkic pronouns such as *māniŋki* 'mine', in Mo. *ende-ki* 'being in this place' < *ende* 'here', etc.)

The accusative had two different endings, one in the pronominal declension, another in the nominal: **-i* > Turk *-i/-i* (pronominal), Mo. *-i*, Kor. *-i/-i*; **-g* > Turk. *-γ/-g* (nominal), Mo. (colloquial) *-(i)g*. The Tungus languages do not have these suffixes, but instead have an accusative in *-wa/-wə* which is equivalent to Ma. *-be*. Ramstedt derives them from the Korean converb *poa/boa* ~ *ba* of the verb *poda* 'see', with the semantic change from 'someone performs an action seeing something' to 'someone performs an action in relation to something' (30).

³⁵ Ramstedt, *Kalmückisches Wörterbuch* 330.

³⁶ Räsänen, *op.cit.* 174.

This explanation is very unconvincing, and the fact remains that the Tungus suffix has no counterpart outside of Manchu-Tungus.

The dative suffixes are **-a* and **-ga*. The former is found in Turkic (e.g. *at-ım-a* 'to my horse') and in Mongolian (e.g. *γaγar-a* 'to the country'). The latter is found in Turkic as the most common dative ending. In Mongolian it is found in adverbs of the type *qami-γa* 'where', *γada-γa* 'outside'. A dative in *-ē* and *-gē* is found in Korean (35), but Ramstedt does not find it in Tungus, and states (33) that the latter lost it long ago. In my opinion, the Tungus partitive in *-a* is a further development of the ancient dative in **-a*. Ramstedt believes that this partitive suffix has resulted from the accusative suffix **-i* (30), but it seems impossible to explain the development of **i* into *a*. Furthermore, the partitive case is not a variant of the accusative, since it is not always a direct object; e.g. Tung. *biγečē umūn bəyā ahiya ubai* 'a man lived with no wife' (*ahiya ubai* literally 'wife-less', where 'wife' is a partitive). A dative or a locative (these two functions are closely related in the Altaic languages) can easily become a partitive. An example of this change of function is the Yakut partitive suffix *-ta*, which is historically the same as the *-da* locative in other Turkic languages and the ablative *-da* in Ancient Turkic; e.g. Yak. *ūla ayal* 'bring (some) water' (*ūla* < **surda* > Turk. *su-da* 'in the water').

The locative suffix is **-da/-du*. Reflexes of **-da* appear in Turkic *-da* (locative), in Mongolian *-da* (adverbial suffix in Written Mongolian; dative-locative in the colloquial languages), and in Manchu *-de*. The **-du* suffix is found in Mongolian, where it occurs also as a derivational suffix (Mo. *ami-du* 'living' from *ami* 'life', *γadaya-du* 'outer' from *γadaya* 'outside'), and in Tungus (dative *-du*). Korean does not have this suffix, but Ramstedt believes that the word *taḡi* (*dai*) 'place' may have been the source of the dative-locative suffixes in question (37). This seems unlikely, as the word has little in common with the locative suffix **-da*.

The suffix of the lative (or prolativ) case is **-ru*. It occurs in Ancient Turkic *ābim-rū* 'towards my house', and in numerous adverbs in modern Turkic languages. Together with the ancient dative suffix it forms a compound suffix *-γaru/-gārū* occurring in e.g. *ič-kārū* > *ičāri* 'into'. This same suffix occurs also in Mongolian *inaru* 'hither', *činaru* 'thither', etc. Ramstedt believes that the Tungus locative suffix *-la* might be a further development of **-ru*, but this seems unlikely, since it is difficult to explain the phonetic and semantic changes involved. The Korean lative suffix **-ru/-rū* > South Korean *-ro*, North Korean *-ru* (*-rī*), *-illī* (*-ilī*) is also to be compared with the above.

The Tungus lative suffix *-ski* is the same as Mongolian *-gsi*, e.g. Tung. *čā-ski* 'to a place further on' (cf. *čā-du* 'further on', answering the question 'in what place'), Mo. *dege-gsi* 'up' (cf. *dege-re* 'on, above'). This suffix does not occur in Turkic; it may be a borrowing from Tungus into Mongolian, or vice versa. Its origin is obscure.

The Tungus prosecutive suffix *-li* (in *hoktolī* 'along the road'), also found in numerous adverbs (e.g. *ālī* 'hither', *talī* 'thither'), is historically the same as that in the Mongolian pronouns *keli* 'when?', *teli* 'then' (42). It is possible that this suffix is the same as the Turkic ending *-li* in *aqalī inilī* 'the elder and the younger

brothers', which also appears in the suffix *-lɿγ* of Ancient Turkic *at-lɿγ* 'having a horse'. This latter is identical with the Mongolian comitative-sociative suffix *-luya* which becomes Kalm. *-lā* in *axpla* 'with the elder brother'. It should be noted that the instrumental case in Mongolian has sometimes the meaning of a prosecutive or comitative, e.g. Bur. *χargayār* (instr.) *yabana* 'he goes down the road', and *manai morin tanai morinōr* (instr.) *belšeže baina* 'our horse is grazing together with your horse'.

The instrumental (or instructivus as Ramstedt calls it) has the suffix **-n*, which, according to Ramstedt, may be of the same origin as the genitive suffix (43). It occurs as *-(i)n* in Turkic (chiefly in adverbs in the modern dialects), and in Mongolian is found in the suffix of the converbum praeparativum in *-run*. Manchu shows the cognate suffix *-i*, which coincides with that of the genitive.

Another instrumental suffix is **-ti* or **-di*, which occurs in forms such as Ancient Turkic *ädgü-ti* 'well' (from *ädgü* 'good'), Goldi *paloa-di* 'with the ax' (from *paloa* 'ax'), *hurmi-di* 'briefly', and is the same as the Tungus instrumental suffix *-ji* < **-di* ~ **-i*. This suffix does not exist in Mongolian, unless it is present in forms such as *ürgülji* 'always, continually'.

The equative is a case denoting size or measure. Its suffix, **-č* and **-ča*, is found in Turkic (e.g. *anča* 'so much') and in Mongolian (e.g. Bur. *abχuisa* 'as much as one can take'). It is not quite clear whether the Mongolian ablative suffix *-ča* and Manchu *-či* are related to this suffix; Ramstedt believes they are not (47), but more research is needed.

Ramstedt also discusses numerous case forms occurring only in Tungus or Mongolian. His explanations of their origins are frequently unconvincing; it cannot be proved, for example, that the Mongolian instrumental suffix *-bar* originated from Mo. *mör* 'way, road' (49), or Chuvash *-lla* from *äl* 'hand' (52). These suffixes remain obscure.

In summary, it may be said that the Altaic languages have the following declensional suffixes in common:

genitive and instrumental <i>*-n</i>	lative <i>*-ru</i>
accusative <i>*-i</i> and <i>*-g</i>	prosecutive <i>*-li</i>
dative <i>*-a</i> and <i>*-ga</i>	instrumental <i>*-ti</i> or <i>*-di</i>
locative <i>*-da</i> or <i>-du</i>	equative <i>*-č</i> or <i>*-ča</i>

I remarked above that the terms genitive, dative, etc. are used here for lack of more appropriate names for the forms in question. I should add that a characteristic feature of the Altaic languages is the dual nature of the forms constituting the declensional system. It is true that they serve in the same manner as the Latin or Greek or Slavic case forms to express syntactic relationships of words; for instance, what is called a genitive expresses the same relationships as the genitive case in the Indo-European languages—or at least its functions largely coincide with those of the latter. On the other hand, the Altaic case suffixes form new words, i.e. they serve also as derivational suffixes. This feature of Altaic has not been discussed in Ramstedt's book; it may be worth while to dwell on it longer here.

We have found the locative suffix **-du* in Mongolian and Tungus. Its func-

tions are, first of all, the same as those of the locative or dative case in other language families; e.g. Mo. *aga-du ög-be* 'he gave to the older brother', *usun-du ükü-be* 'he was drowned' (literally 'he died in the water'), Tung. *atirkän-du gun-čš* 'he spoke to the old woman'. But we also find numerous words derived with *-du* which are not locative forms at all, e.g. Mo. *ami-du* 'living, alive' (from *ami(n)* 'life'), *γadaya-du* 'outer, external' (from *γadaya* 'outside'), *emüne-dü* 'front, southern' (from *emüne* 'before'), *dege-dü* 'upper' (cf. *dege-gsi* 'up'), *doora-du* 'lower' (from *doora* 'low, below'), *doruna-du* 'eastern, oriental' (from *doruna* 'east'), *dumda-du* 'middle, central' (from *dumda* 'the middle, center'). These forms can serve as attributes, e.g. *dumda-du oron* 'China' (literally 'the Middle Country', translation of Chin. *chung kuo*), *dege-dü sur-γa-γuli* 'high school' (literally 'upper school'), *emüne-dü γobi* 'Southern Gobi'. A shade of the locative meaning is to be found in such forms: 'southern' means 'in the south', 'central' means 'in the center', etc.; but they cannot be regarded as locative.

A characteristic feature of these and similar forms is that they can be 'declined'. It is known that in the Tungus languages and, outside the Altaic group, in the Finno-Ugric languages, there are numerous case forms with compound endings; for example, Tung. *jū* 'house', *jū-du* 'in the house', *jū-du-k* 'from the house'; *biral* 'rivers', *biral-du* 'to the rivers', *biral-du-lī* 'down the rivers'; *halga* 'hand', *halga-lā* 'in the hand', *halga-lā-k* 'from the hand'. Cf. Finnish *talo* 'house', *talo-lla* 'at the house' < **talo-l-na*, *talo-l-ta* 'from the house', *talo-ssa* 'in the house' < **talo-s-na*, *talo-s-ta* 'out of the house'. The difference between Tungus and Finnish is that in Finnish the elements constituting compound suffixes are no longer active, but are found only as elements in petrified case endings, while in Tungus at least some of them are living suffixes occurring alone. In the Altaic languages a case form may become a stem and take another case ending; e.g. the genitive may become a stem for further 'declension' in Tungus: *hunāt* 'girl', *hunāt-γī* 'of the girl', *hunāt-γī-wə*—an accusative formed from the genitive. The same type of doubled case forms is found in Mongolian, e.g. Kalm. *axo* 'the elder brother', *ax-in* 'of the elder brother' (gen.), *ax-in-ā* (gen. from gen.), *ax-in-du* (loc. from gen.), *ax-in-ās* (abl. from gen.).

Another characteristic of the Altaic languages is what may be called group declension. The essence of this phenomenon is that the case suffix is taken only by the last of a group of nouns serving as coordinate members of a sentence. Thus in a sentence translated as 'He spoke to the men, women, and children', only the last dative object takes the case ending, while the other members of the group remain in their stem form.

Although the volume under review does not deal with syntax, I have mentioned a few features of syntax common to the Altaic languages only because they remove the last doubt about their relationship.

Another important chapter of Ramstedt's book is that dealing with the conjugation of verbs (82-153). The most characteristic feature of the Altaic verb is that the only original verbal form is the stem; this serves also as an imperative, at least in some of the Altaic languages, e.g. Turk. *kāl* 'come', Mo. *ire* 'come'. All the remaining forms are in origin verbal nouns. If there were ever genuine indicative forms, they were lost very early; more probably, no such forms ever existed

in Altaic. The indicative forms which occur in present-day Altaic languages are historically verbal nouns, e.g. Tung. *tagənni* < **tagənsi* 'you sit' (originally 'you sitting'), cf. the suffix *-n* of verbal nouns in Mo. *singgen* 'fluid, liquid' (from *singge-* 'be absorbed'); pre-classical Mo. *yabum*, present tense of *yabu-* 'go, walk', cf. the deverbial noun in *-m* in Mo. *naɣadum* 'game, play' (from *naɣad-* 'play'), Turk. *ölüm* 'death' (from *öl-* 'die'); Turk. *kälür* < *käl-ü-r* 'he will come' (from *käl-* 'come'), cf. the suffix *-r* of verbal nouns in Mo. *amur* 'rest' (from *amu-* 'rest'), Tung. *tagəra* 'sitting down' (from *tagə-* 'sit down').

The nominal origin of the indicative forms is also evident from the fact that the personal endings of verbs are either nominative or genitive forms (i.e. subject or attributive forms) of personal pronouns. Thus Turk. *aldım* < *al-d-ı-m* 'I took' has the same possessive ending as *atım* < *at-ı-m* 'my horse'. In Buriat Mongolian, *jereɣemni* 'I must come' has the same ending *-mni* as *esegemni* 'my father'; in Tungus, *tagəčəw* 'I sat down' has the same possessive ending as *jūw* 'my house'. The verbal forms could be translated as 'my taking in the past', my future coming', and 'my having sat down'. Examples of verbal forms occurring with the nominative of a personal pronoun as personal ending are Bur. *jereɣeb* 'I shall come' (cf. *bagšab* 'I am a teacher', lit. 'I teacher'), Turk. *kälürsän* 'you will come' (cf. *saɣ sän* 'you are all right', lit. 'healthy you').

Altaic has a large number of so-called converbs, i.e. verbal forms denoting the manner or the circumstance in which the action of the main verb is performed. These converbs are in origin petrified forms of oblique cases of verbal nouns, e.g. Mo. *yabuɣsaryar* 'while he was going', historically an instrumental case form of the nomen perfecti in *-ɣsan* (e.g. in *ab-uɣsan* 'one who has taken', *ükü-gsen* 'dead', *törü-gsen* 'born'). Such converbs are characteristic of Tungus,³⁷ Mongolian,³⁸ Turkic,³⁹ and Korean.⁴⁰

Ramstedt does not dwell on these details. What he gives us in the *Einführung* is a brief outline of the comparative morphology of Altaic. He discusses only the general morphological features of Altaic, without following the forms discussed into all the Altaic languages. It should be remarked that his book is a posthumous work, prepared for publication by his pupil Pentti Aalto. The latter received an unfinished manuscript, and had to write some portions of the book on the basis of Ramstedt's notes. This explains why some chapters of the book are more detailed and better substantiated than others.

It became possible to write a complete comparative grammar of the Indo-European languages only after the work of several generations of scholars. Ramstedt's book is not a comparative grammar; but it is a very solid introduction to Altaic linguistics. I do not doubt that it will soon be possible, with Ramstedt's work as a foundation, to write a satisfactory comparative grammar of the Altaic languages.⁴¹

³⁷ Poppe, *Materialy po solonskomu jazyku* 136 ff. (Leningrad, 1931).

³⁸ Ramstedt, *Über die Konjugation des Khalkha-Mongolischen* 44 ff.

³⁹ A. von Gabain, *Özbekische Grammatik* 97 ff. (Leipzig-Wien, 1945).

⁴⁰ Ramstedt, *A Korean grammar* 87 ff.

⁴¹ The reviewer is indebted to John C. Street, a graduate student at Yale University, for improving both the organization and the English style of this review.

Vvedenie v žykoznanie. By A. S. ČIKOBAVA. Pp. 243. Moskva: Učpedgiz, 1952.

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It will be recalled that in the summer of 1950 Joseph Stalin published a number of articles on linguistics which formed the climax of a major public controversy over the so-called New Doctrine of Language.¹ This doctrine, elaborated by the eccentric Soviet scholar Marr in the 1920's and early 30's, had stood, up to the moment of Stalin's intervention, under the protection of the Soviet government as the reflection of Marxism in linguistics. In 1950 Stalin reversed the official position, 'unmasked' Marr's theories as an anti-Marxist hoax, ordered them purged, and reinstated the traditional neogrammarian dogma supplemented by Marxist overtones. Presently a new team of scholars, consisting of those who for many difficult years had managed to withstand the pressures of the Marrist monopoly, assumed the leading academic positions in the country.

Čikobava belongs to this group. Having played a prominent role in the controversy, perhaps having even acted as technical consultant to Stalin, he was given the assignment of writing the first of three volumes of a new *Introduction to linguistics* which constitutes the basic text of the post-Marr regime in Soviet linguistics.² It is this set of circumstances rather than the intrinsic merits of the book which lend Čikobava's volume special significance. Apart from being a textbook, it is the first systematic formulation of theoretical views currently acceptable in Soviet linguistics, and thus a new version of what represents Marxism in linguistics.

The book consists of a Foreword and five parts: 1. 'Introduction', 2. 'Language, its social nature, development, and origin', 3. 'Phonetics', 4. 'Language classification', and 5. 'The place of linguistics in the system of science'. Since the author perforce evaluates Stalin's linguistic contributions as the foundation of linguistic theory in the Soviet Union (7, 22), one is not surprised that much of what follows is an exegesis of Stalin's 1950 statements on language.³

According to Čikobava, what are the objectives of general linguistics? They are (1) to define the nature, origin, and development of language; (2) to elaborate methods for analyzing the forms and functions of language, as well as for investigating the history of one language in relation to that of others; (3) to

¹ Cf. Herbert Rubenstein, The recent conflict in Soviet linguistics, *Lg.* 27.281-7 (1951); J. V. Murra, R. M. Hankin, and F. Holling (translators), *The Soviet linguistic controversy* (New York, 1951); J. Stalin, *Marxism and linguistics* (New York, 1951).

² The second volume, by L. A. Bulaxovski¹, appeared in 1953; the third is to be written by V. V. Vinogradov.

³ As so often in Soviet scientific literature, the politically expedient introductory references to the works of the Marxist classics do not preclude the later expression of independent and even sarcastic views. An example of this is Čikobava's statement bearing directly on the significance of Stalin's role in the remaking of Soviet linguistics: 'It was easy to see that ... Marr's New Doctrine of Language failed to correspond to the facts ... yet ... the root of his mistakes was uncovered only by I. V. Stalin's works ...'.

specify the branches and respective functions of linguistics; and (4) to establish the place of linguistics among the sciences. The organization of the volume under review, indeed the general plan for the three-volume work, reflects these objectives. Čikobava's Chapter 2, entitled 'Language, its social nature, development, and origin', matches the first objective directly; his Chapter 5 matches the fourth objective. Chapters 3 and 4 at least contribute to the remaining objectives, but leave the detailed elaboration of grammatical, lexical, semantic, and stylistic problems to the other two volumes.

The insistence of Marxist theoreticians on a historical approach to the analysis of social phenomena has its roots in the same 19th-century outlook which directed comparative linguistics into its own historical channels. The constant Marxist call for 'historism' bears a close resemblance to the neogrammarians' belief that the study of language history was not merely the primary but the only legitimate object of linguistics. It is not surprising, therefore, to witness the reinstatement, on grounds of Marxism, of the traditional comparative-historical doctrine as the officially accepted framework of theoretical pursuits in the Soviet Union (16-7, 20, 198), though it is noteworthy that Marr himself, in his heyday, had been equally preoccupied with the past, preferably the pre-historic past. In his 1950 articles, however, Stalin has stressed the importance of historical language study in connection with the known history of the speakers of the given language. Neither Marr nor Stalin had raised the problem of descriptive linguistics as such, and energetic discussion of descriptive problems in the journals and at conferences has begun only since Stalin's death.⁴

For his principal definitions of language Čikobava has to refer to two often-quoted statements by Lenin and Stalin, to the effect that language is the most important means of human communication and that it lives and dies with the society that speaks it (23). No reference is made—to name only the most striking lacunae in these formulations—to the vocal nature of language, or to its recurrent patterns, or to its symbolic nature. The student who is thus introduced to the field is left to his own devices; the difference, for instance, between speech and writing is not made explicit until the chapter on phonetics (Ch. 3). The problem of defining language is taken up again later in the book, in an oblique fashion, as the author takes issue with de Saussure's *langue-parole* dichotomy (174) on the grounds that a clear division between (descriptive) phonemics and (historical) phonology is impossible. But the implications of de Saussure's system are not brought out, nor is there any further trace of the fundamental issues involved in choosing between what has been called 'God's-truth' and 'hocus-pocus' linguistics. The student is hardly ever referred to the sources. The concept of language remains ill defined, for although the author strongly argues that language is a system rather than an organism (59-60), he fails to name the constituents of that system or their relationships.

⁴ This does not mean that nonhistorical grammars of the conventional type were not written in the Soviet Union. It does mean that until 1953 considerable stigma was attached to the synchronic approach, a fact which may be held at least partially accountable for the failure of Soviet linguistics to complete the description of all minority languages spoken in Soviet territory. Needless to say, this stigma has also served to keep a theory of description from being developed domestically.

To illuminate the complex relationships between language and thought, Čikobava cites Marx's formula 'language is the immediate reality of thought', as well as Stalin's well-worn variant 'the reality of thought appears in language' (29). In the absence of further clarification these aphorisms are not very meaningful; nor is the problem advanced by Stalin's dubious generalization that nonlinguistic thought is impossible (29-30)—which, if true, would rule out the kind of thought that is involved in chess playing or musical composition. Čikobava follows Stalin's article in this as well as in regard to the conservatism inherent in the grammatical system and basic lexical fund of a language.⁵ As to the basic lexical fund, Čikobava's treatment sheds very little additional light on this curious category; according to recent Soviet doctrine, it includes not merely old root words but obvious recent borrowings. The basis on which Russian *kommunist* and *partiä* are numbered among the basic lexical fund (86), while *tramva*¹ and *trolle*² bus are excluded (33), is not revealed, but it cannot be linguistic; nor is it clear why French should be regarded as a 'new language' with a 'new base' in relation to Vulgar Latin, while modern Georgian and Armenian are 'variants' of Old Georgian and Old Armenian (34-5, 91).

About fifty pages are devoted to Stalin's interpretation of the position which language occupies in the Marxist scheme of economic base and cultural superstructure, and to the function of language in a society stratified by classes. These are not devoid of a number of contradictions.⁶ Whereas Marr had gone to one extreme in acknowledging hybridization as the exclusive form of linguistic development, Stalin and Čikobava now go to the other extreme of discounting this phenomenon altogether. Čikobava supports his rejection with an argument (99) which will not be lost on those aware of current Soviet language policies: 'bilingualism does not indicate the presence of language hybridization under all circumstances; only the compulsory character of bilingualism signals the battle of languages.' There can be little doubt that Čikobava could have refrained from perpetuating one of Stalin's more obvious technical errors, the designation of the Kursk-Orel dialect as the basis of the Russian literary language.

As so often in Soviet treatises on this subject, phonemics is discussed 'materialistically' under the heading of phonetics, and the functional separation of the two fields is obliterated. In the description of speech sounds the articulatory principle is deemed to take precedence over the acoustic (143), but this is not adhered to in the classification of Russian phonemes, which are distributed in the traditional manner in such acoustic groups as 'noisy' or 'sonorous' (150-8).⁷ The author's lack of appreciation for descriptive problems manifests itself

⁵ There is no mention of the fact that Meillet had made similar observations at least thirty years earlier; indeed, no credit is given to any prior sources—except the Marxist classics.

⁶ For instance, it is said on the one hand that language is indifferent to the fate of the classes and the development of the economy (38, 81), while elsewhere (78-9) it is claimed that both the class struggle and class cooperation manifest themselves in language. There is a similar contradiction in the insistence that language evolves by immanent laws, but must always be examined in connection with the history of the society in which it is spoken.

⁷ Cf. V. Porzezinski¹, *Vvedenie v žykovedenie*² 105 ff. (Moscow, 1916); A. A. Reformat'ski¹, *Vvedenie v žykovedenie* 61-7 (Moscow, 1947).

clearly in the slipshod manner in which he cites his examples. Further, his unwillingness to establish a firm system of phonetic or phonemic transcription results in a great deal of inconsistency, arbitrariness, and error. The Spanish name *Jose* is transliterated into Cyrillic as *Xose* (without indication of accent), English *Jack* becomes *Dzek*, and French *chanter* becomes *šante* (with an accent, but with no indication of the nasalized vowel; 170). This not only sets a bad example for students, but makes this chapter, when combined with the many errors of fact which it also contains,⁸ one of the worst in the book.

Čikobava perpetuates one of the most annoying features of recent scientific prose by refusing to print Western proper names in the Roman alphabet alongside the Cyrillic transcriptions. It is surely of no assistance to the first-year student in linguistics interested in consulting the library index to be acquainted with *Viliām Dzons*, *Zilieron*, or the grammar of *Por-Roāli*. This practice is particularly vexing in a work purporting to prepare linguists. Another important deficiency in a work of this nature is the absence of both an index and a complete bibliography.

In view of the Marr episode one can appreciate the author's warning that until more is known of the history of the world's languages, genetic classification must remain largely tentative (199). Yet Čikobava considers the Basque-Caucasian link an established fact (219), and includes Hittite in his Caucasian group (200, 225). Some of the figures in Čikobava's linguistic geography are astounding: speakers of English are reduced to 150 million (122), evidently by the same method which raises the number of Polish speakers in the United States to 3 million; and Macedonian is ignored as a South Slavic language (205-6).

The concluding chapter emphasizes again the fact that in the Soviet Union all science must be regarded as historical, and establishes linguistics as a branch of 'social historiography' (241). One cannot quarrel with the author's statement that linguistics is important to general education, to the creation of alphabets, to the correction of speech defects, and to other practical pursuits, nor with his view that historians, psychologists, philosophers, and literary critics must all become familiar with the fundamentals of linguistics (242). One wishes only that these fundamentals had been presented more adequately, in keeping with the excellent traditions established in Russia earlier in this century. As it stands, Čikobava's volume compares unfavorably with practically all works of a similar nature which have preceded it. Whether this regrettable weakness is due to the fact that the book had to be written hurriedly, or whether political circumstances made a different presentation impossible, is hard to determine. The general topics which were assigned to Čikobava for treatment under the overall plan are precisely those on which Stalin dwelt most fully, and it is here that the pressure to keep his formulae must have been strongest. But the book is weak also in areas which Stalin did not touch upon

⁸ As an example of a triphthong Čikobava cites English *twenty-storeyed* (sic); *goose* and *geese* are transliterated into Cyrillic *guz* and *giz* respectively, and the English spelling is incorrect; Chinese is transcribed without mention of tones (183-5); the plural of German *Bach* is said to be *Bächer* (169). This is merely a sampling of errors.

at all. Perhaps the worst feature of the book is a false notion underlying its function: should an introduction present cut-and-dried 'truths' without discussion, without reference to divergent views, without reference to the most basic sources? It would seem that at least the semblance of breadth in treating fundamental issues could have been achieved within the framework of Stalin's precepts; for had not Stalin himself made the statement that 'no science can develop and flourish without a struggle of opinions, without free criticism'?⁹ It is to be hoped that Volumes 2 and 3 will surpass the level of the first, particularly since they can be based more openly on appropriate sources.

Power of words. By STUART CHASE, in collaboration with MARIAN TYLER CHASE. Pp. xii, 308. New York: Harcourt, Brace and Co., 1953, 1954.

Reviewed by HARRY HOLJER, *University of California, Los Angeles*

The field of communication has been intensively cultivated in the last few years. Its problems are many and complex, and belong not only to linguistics, but to anthropology, sociology, psychology, engineering, and possibly other disciplines as well. Scholars have made considerable progress, each in his own field, in the study of communication, but little has as yet been achieved in integrating these researches or even in establishing a common strategy of research. As a result the specialist in one discipline is frequently working at cross-purposes with those in other fields. What might be called the first problem of communication, the exchange of information and achievement of understanding between diverse specialists working on problems of communication, is still in large part unsolved.

Stuart Chase attempts to bring together in one volume and in nontechnical language the major developments in communication, and to show as well the applications of this research to modern world problems. According to the publisher's blurb, Chase, in earlier books,

... succeeded in making economic subjects interesting and clear to the average intelligent reader. Since then he has turned to broader aspect of social science, reporting and trying to integrate diverse specialties, so that not only the general reader, but the experts may better understand what is going on. This volume is another skillful example of integration in the sciences dealing with human relations.

Chase himself is somewhat more modest in stating his objectives. He claims only to 'attempt to connect some of the diverse disciplines touching on communication' (xi). 'It is the purpose of this book to interpret significant findings about language and communication, to point out applications of these findings and the need for further scientific study' (10).

Power of words is accordingly divided into two parts, Findings and Applications. Part 1 has sixteen brief chapters, the first of which outlines the general field of communication. Chapters 2-16 report in nontechnical language the 'many new and exciting things' discovered about communication by engineers, brain physiologists, psychologists, linguists and 'freewheeling grammarians',

⁹ Murra, Hankin, and Holling 75.

anthropologists, physicists, mathematicians, semanticists (with especial emphasis on Alfred Korzybski), and 'the students of group dynamics and the techniques of listening' (10). Curiously, Chase omits some disciplines from his long list, notably sociology and social psychology.

Part 2, Applications, is described by the author as a series of case studies; it 'might be called applied communication theory' (10). The chapter headings indicate the scope of the applications: Tools for Writers, Economic Talk, Moscow Talk, Campaign Oratory, Guilt by Association, The Mass Media (newspapers, radio, motion pictures, etc.), Gobbledygook (i.e. the 'language' of bureaucrats), Medical Talk, and Schoolroom Talk. The final chapter, The Unused Potential, provides a brief summary of Chase's findings, a long list of 'suggestions for research badly needed now' (289), and a plea that men be taught to avoid well-known 'failures of communication' and so release the full power of their thinking. 'Perhaps the next great revolution following the industrial, will be the revolution in communication. This book is a small contribution to that end' (292).

It is perhaps pertinent to note that Chase claims no 'especial competence' in the disciplines relating to communication, though he is 'reasonably familiar with semantics [of the Korzybski variety] and economics' (xi). For the rest of the sciences on which he has elected to write, Chase is dependent upon 'the help of good men digging ever deeper into the subject'. The work of these good men (who include, among many others, Einstein, Norbert Wiener, Claude Shannon, George L. Trager, Benjamin L. Whorf, Alfred Korzybski, C. K. Ogden, and I. A. Richards) Chase sometimes 'cannot follow, except in a general way' (x). These failures of communication apparently do not trouble Chase; he is content with an understanding of the 'general approach' to a subject, even though this competence enables him, for example, to discuss Einstein's concept of relativity only 'on a pretty high level of generality' (115).

With this background and training, it is not surprising that Chase's work is reminiscent of the Sunday supplement approach to 'science reporting'. In his chapter on linguistics, for example, linguists are said to 'botanize happily' among 'the wild flowers of speech' (88-9), whether these be found among 'happy nature folk', who 'do not torture themselves with "spelling bees"' (95), or among modern Americans, whose 'blossoms of ... speech' can sometimes 'be pretty wild' (92). Every language is said to have rules for ordering and arranging its phonemes (called 'building blocks') into 'structures of words, sentences, and ultimate meanings, including cosmic ideas' (90). The word is difficult to define: it is 'not necessarily a single idea; it may contain several ideas' (93). Words may also, if we take the titles of Chase's books seriously, be tyrannical (*The tyranny of words*) and possess both positive and negative power. The 'word' troubles Chase a good deal, but troubles his readers even more. One can never be certain as to its referent; there is constant confusion of word and vocabulary with language, an excellent example of the failure of linguists to communicate to Chase.

Chase is impressed by the methods of linguistics. He finds them extremely precise, so exact that two linguists (even if only graduate students) working

independently on the same language, 'will return with almost identical records' (90). But these methods are also mysterious (the Sunday supplement touch) and involve the use of a 'code' or 'algebra' made up of 'strange symbols'. It is by means of this device that linguistic analysis is achieved. The steps, 'reasonably clear even to the layman', are these (92): 'First, the single sound unit called "phoneme"; next, the group of phonemes—which may be a word, or part of a word, called "morpheme"; then the arrangement of morphemes into "syntax"; and finally a discussion of cultural aspects and meanings of the patterned sounds, called "metalinguistics."' This account, said to be descriptive of Trager and Smith's methodology in *An outline of English structure*, is remarkable only for its brevity (it is not further explained anywhere in the book). It surely cannot communicate anything even to the most intelligent layman. But it does perhaps emphasize the 'mystery' of the science of language.

It should be noted, in fairness to Chase, that he has much to say, about linguistics and other topics, that is both sound and useful. Items of this kind are quite easily abstracted by the specialist from the tangled web of metaphor and journalistic writing in which Chase buries them. But it is probably not so simple a task for the layman to do this, and it is for him that Chase is writing. We may quite seriously doubt that Chase has really communicated to his readers anything like an adequate summary of recent scientific contributions to the study of communication. It is more likely that he has increased the confusions of his serious readers and, what is worse, encouraged among others the already widespread myth that science is a kind of magic, to be given, like the incantations of a primitive shaman, implicit belief but not understanding.

The central thesis of this book may be summed up in the following statements.

1. Language, or the use of words (in Chase's definition), is distinctive to man. It is a necessary part of every human culture and the bond that unites the members of every human society.

2. Words frequently have power, and this may be either positive or negative. Chase strongly emphasizes the negative power of words, and holds them responsible 'for untold and unnecessary conflict and misery' (ix). 'The misuse of man's chief characteristic [i.e. words] has made his life unduly insecure. ... Much, if not most, of the pain and misery in the world today can be laid to the misinterpretation of signals ...' (9).

3. The growth of modern technology and the mass media of communication has made words far more powerful and dangerous. Listeners must be taught to detect the misuse of words; communication must be improved, and men of diverse cultures must learn to understand each other. Failing this 'technological imperative to control technology' (11), warfare will destroy mankind.

Communication research has not yet solved this problem, according to Chase, and it may well be that the problem has no solution. But the assumption that 'civilization is too much for Homo sapiens ... has no force until man has used his full potential on the new environment which technology has built. That potential is now neutralized, due to avoidable failures of communication' (291). Chase concludes then that all is not lost, and that the revolution in communi-

cation, produced by the magic of the scientist, will ultimately and in time save man from himself.

Petite introduction à la psychomécanique du langage. By ROCH VALIN. (Cahiers de linguistique structurale, Faculté des Lettres de l'Université Laval, No. 3.) Pp. 91. Québec: Presses Universitaires Laval, 1954.

Reviewed by JOHN B. CARROLL, *Harvard University*

Custom decrees the right of an author, in his preface, to be the first to review his own work, and to defend himself from anticipated criticism. Valin's *avant-propos* (7-15) starts with a prediction that his views 'ne manqueront probablement pas de surprendre, voire de scandaliser, ceux de nos confrères pour qui la linguistique n'a qu'un visage' (7)—namely, that of traditional historical linguistics. These views are revolutionary, Valin admits, but he is able to allude to several instances in the history of science where ideas originally rejected by the scientific fraternity were later accepted without question (25 fn.). Furthermore, he says, they have been arrived at by a correct scientific method: 'Car c'est à la *spéculation* et non à l'observation qu'il est dévolu en toute science de rendre raison des choses' (8). A single event is more valuable for science (witness Newton's falling apple) than the supposedly exhaustive frequency counts which current linguistics goes in for (9). Not only are these views revolutionary; they represent a tremendous step forward, and have been all but unnoticed in the years since they were first brought to light by Gustave Guillaume, the author's teacher: 'Dix ans plus tard [than the appearance of Guillaume's dissertation], en 1929, paraissait un ouvrage qui marquera un jour prochain une date importante dans l'histoire de la linguistique: *Temps et verbe*. Ce livre, s'il n'est pas passé tout à fait inaperçu, est resté à coup sûr incompris de la linguistique chevronnée' (12). These views may be difficult to understand or to accept, but the reader who is tempted to be skeptical is reminded of the advice of the Scotch savant Chrystal, 'Read on, and faith will come to you' (37 fn., reiterated 47 fn. with additional encouragement). Unfortunately, there is only one way to attain a real understanding of these theories: 'C'est de fréquenter assidûment, pendant deux ou trois ans, les conférences de M. Guillaume' at the École pratique des Hautes-Études in Paris (14; 22 fn.).¹ The author defies anybody who does this to be able to escape the conclusion that these theories are correct, 'tant les preuves s'accumulent, magnifiques et brutales, avec l'irréfutabilité de l'évidence atteinte' (14). To be sure, the present work is only an introduction; it cannot possibly do justice to the subject (23 et passim), but 'Si les humbles pages qui suivent avaient l'heureuse fortune de

¹ It is a fact, however, that most of Guillaume's published writings are still reasonably accessible, and may be consulted for light on his theoretical position. Thus, one may look up articles of Guillaume's in the following places: *Journal de psychologie* 30.354-72 (1933), 34.161-78 (1937), 36.183-98 (1939); *BSL* 39:1.5-23 (1938), 40:3.14-24 (1939); *Acta linguistica* 1.167-78 (1939), 3.5-118 (1942/43); *Le français moderne* 9.9-30, 171-80 (1941), 11.89-107 (1944), 13.70-82, 207-29 (1945), 19.29-41, 115-33 (1951), 21.127-36 (1952). The present review does not presume to give any account or evaluation of these extensive writings; it is addressed solely to Valin's presentation.

vaincre ou seulement d'ébranler certaines résistances et de faire naître à l'endroit des théories linguistiques de M. Guillaume un courant tardif de curiosité qui en hâtât la publication, nous aurions la conviction d'avoir rendu à la Science un très grand service' (15). And although the psychomechanics of language is still imperfect, one hopes that someday it will attain the rigor which is exemplified in the work of the comparativists (19 fn.).

Valin, who is a professor in the Faculté des Lettres of the Université Laval, nowhere claims that he himself has made any original contribution save possibly that of clarifying and organizing Guillaume's theories for the student; on the contrary, he is continually at pains to express his admiration for the genius and originality of Guillaume, and to defend him from detractors. He appears to have collected all reviews of his teacher's work, favorable or unfavorable, and does not lack scorn for those who misunderstood or criticized him. Several highly favorable reviews from the pen of Meillet² are quoted as showing that at least one linguist was able to rise above the apathy which seems to have greeted most of Guillaume's writings.

The title of the present work speaks of 'la psychomécanique du langage'. The connection with what passes as psychology today is extremely remote; certainly it is not the psychology taught in American schools, and probably not even the psychology discussed by most psychologists in France. And the connection with mechanics is purely metaphorical. What, then, is 'la psychomécanique du langage'? Briefly, it seems to be an attempted explication of certain Saussurian notions concerning the relations between language (as a system or code) and the speech event, with the promise of elucidating thereby the mental processes which are manifested in speech. Valin complains (19) that for all its rigor in cataloguing the forms of language and tracing familial relationships, linguistics has 'aussi peu que rien' to say about the 'nature' of human language and about the connection of language and thought. This, then, is the problem of the psychomechanics of language.³

In a section entitled *La méthode et ses fondements* (23-31), Valin introduces what he regards as a basic postulate: that the mental operations involved in language necessarily require time. The periods may be infinitesimally short, but they exist. He quotes the observation of Guillaume that 'it takes time to think'. The task of the psychomechanics of language is thus twofold: first, to identify and carefully define, one by one, the operations of thought which are concealed under observable phenomena, and second, to refer these operations to the 'temps opératif'. This is to be done, apparently, by a type of imaginative intuition based on an analysis of language facts. For the present, the results are to be displayed by a species of graphic representation (the pamphlet contains no less than 29 of these schematic diagrams), but Valin hopes that some day such representations may be replaced by an 'algebra' of special symbols. For example, one can represent (28) the development of the French language from

² BSL 31:3.1-2 (1931), 34:3.20-6 (1933).

³ This translation is not strictly accurate, since it is difficult to render the distinction between *langue* and *langage*. By using the latter term with the definite article, Valin seems to mean 'the act of language', or more accurately 'the speech event'.

Vulgar Latin by a line of which the left-hand end represents the 1st century A.D. and the right-hand end points somewhere indefinitely into the future after the 20th. The extent of the line represents 'diachronie', but vertical cuts represent various 'synchronies', such as the Vulgar Latin of the 4th century. The psychomechanics of language (alternatively *la linguistique de position*, *psychosystématique*, or *mécanique intuitionnelle*—these terms all having been proposed by Guillaume) is, however, concerned chiefly with the very brief *temps opératif* involved in a single speech act.

At this point, the psychologically inclined reader expects that Valin is about to offer some empirical measurements of *temps opératif*, such as reaction times for free association, but he is disappointed. The next section, *La dichotomie langue-discours* (32-9) plunges him into an extended terminological explication. *La langue* is the language system, the code, 'langage en puissance'; *discours* is the actual speech event or product, 'ce qui est momentanément exprimé', 'langage effectif'; the *acte de langage* is 'l'activité transitionnelle qui accompagne obligatoirement le passage de la "langue" au "discours", 'a 'cinétisme de transition' (32 f.). So far, we are on fairly familiar ground. But Valin is entranced with this process whereby *la langue* (potential language, as it were) gets somehow converted into *discours* (language in being). *L'acte de langage* starts (and here one of Valin's diagrams is introduced) with a 'statisme initial' in which resides a 'potentialité permanente', and ends with a 'statisme final' with an 'effectivité momentanée'; during this process 'tension I' is reduced and 'tension II' increases. It appears from later discussion that 'tension I' represents a change in mental activity which proceeds from the broadest possible conception of something (e.g. *l'homme* as a generic term) down to a specific concept (e.g. *un homme* as applied to a particular man); 'tension II', on the other hand, operates wholly in the field of 'effet', i.e. while *discours* ('langage effectif') is emerging in the speech act. While Valin is not very explicit about 'tension II', I take it that this is said to increase because the stimulus value of speech increases as a sentence is uttered. Indeed, the unit of *discours*, we find later (46), is the sentence, and the effective value of a sentence can be said to increase as it unrolls.

The next section, *Signe, signifiant et signifié* (40-59), is again full of terminological distinctions; it would be the despair of a translator, but if pressed I should translate its title approximately as 'sign, meaning, and referent'. The section seems to be an extremely elaborate way of saying merely that the sign, Janus-faced, has a twofold function: it relates to a *signifié de puissance* ('potential referent') in *la langue*; and it relates to an actualized *signifié d'effet* ('actual referent') in *discours*. For example, the sign *apple* has a generic, 'potential' meaning in the language code, but refers to a particular apple in space-time when used in speech (general semanticists please copy). This idea generates at one point a rather quotable statement, reminiscent of certain remarks of Sapir and Whorf: 'La langue est donc une réalité éminemment abstraite dans laquelle on trouve, non pas des représentations toutes faites qu'on n'aurait qu'à utiliser telles quelles dans le discours et dont l'ensemble constituerait une *image* de l'univers, mais seulement les schèmes abstraits des opérations de pensée à effectuer

pour obtenir la représentation de telle ou telle ou telle fraction d'univers' (47 f.; italics in the original). The chapter shortly degenerates, however, into a particularly dangerous line of reasoning. Valin again becomes concerned with just how *langue* gets converted into *discours*: there must be a particular point in time, he implies, when this mysterious passage takes place. Let us locate this point near the transition P_n/P'_0 , he says, for this is the end of the *langue* part of the process (from P_0 to P_n) and at the same time the beginning of the *discours* part of the process (from P'_0 to P'_n). Let us focus our attention on the relation P_n/P'_0 —'ce qui nous oblige à définir les conditions dans lesquelles on passe de P_n en P'_0 et, en conséquence, à examiner si la transition d'une position à l'autre est immédiate ou si, au contraire, les deux portions de mouvement ... sont séparées par un intervalle de temps' (51). Here at all events, the reader might think, Valin will be forced to introduce experimental evidence, perhaps some measurements of the number of milliseconds required on the average to pass from P_n to P'_0 . But no. Valin proceeds to aver, presumably buttressing himself with whole continents of intuition, that in the Indo-European languages the transition from P_n to P'_0 's'effectue, depuis les temps les plus anciens où ces langues soient attestées, sans intervalle appréciable de temps' (51), while in the Semitic languages he finds a 'diastème' separating these two points. How can he argue thus? A satisfactory answer to this question, he observes, would require a digression not possible—'ni même souhaitable'—in a work of the present scope. All that counts here, he continues, is to know that such a 'diastème' exists. In Arabic, for example, the root *k-t-b* is a *signe de puissance*. 'Dans un esprit arabe, cet assemblage de consonnes évoque, sans en délimiter les contours, l'idée vague d'écrire, sans qu'il soit précisé s'il s'agit de l'action d'écrire, de la chose écrite ou celui qui écrit' (52). But at P'_0 , the first instant of *discours*, words like *kâtûb* or *kitâb* emerge full-blown: they have been invested with a 'traitement vocalique' during some extra interval, which must be this 'diastème'.

The dangers of such arguments hardly need reciting. Any inferences about how 'un esprit arabe' reacts to a bare triconsonantal root, as set forth in dictionaries and grammars, are surely gratuitous. The assumption that the 'traitement vocalique' which clothes the root with tense, person, or other grammatical meanings lies wholly in the province of *discours*, merely creates artificial difficulties; is not the discontinuous vocalic morpheme a property of *la langue*? Finally, any attempt (especially without empirical evidence) to find pieces of time to account for wholly supposititious mental states or processes smacks of a Herbartian psychology which was outmoded by the middle of the 19th century.

As if to add a final fillip to these speculations about the 'diastème', Valin claims that the Indo-European languages show some evidence of having been, in the course of their prehistory, root-languages of the Semitic type, the gradual disappearance of the root feature having been accompanied by a progressive reduction of the size of the *diastème*. The evidence to which Valin appeals is 'sous forme d'alternances vocaliques qui se sont parfois perpétuées, à titre d'archaïsmes, jusque dans des états de langue très évolués' (53 fn.). Since we are given no illustrations, we cannot infer whether Valin would claim English *man* ~ *men*, for example, as evidence of a prehistoric root *m-n*.

Not much meat will be found in the section *Du mot à la phrase* (60-2). The sentence, we are told, is composed of words—'une vérité banale en grammaire', but it is not a simple juxtaposition of words. 'La vérité est que, une fois en place le dernier signifié composant, il y a *sommation* des éléments constituants, opération qui a pour effet de faire perdre à chacun son individualité et de créer un signifié total résultant de la somme des parties. ... La phrase disparaît avec la circonstance qui l'a fait naître' (61).

In the section *De la langue au mot* (63-80), the author again takes up the theme of the *langue-discours* dichotomy elucidated earlier. The *acte de langage*, we learn, is an *acte de représentation* plus an *acte d'expression*. *La langue*, of course, is a system for representing everything which is thinkable for man in a given state of civilization; *discours* is the expression of what is thought at the moment. Valin then attempts to demonstrate the fertility of *la psychomécanique du langage* by applying it to the system of the article in French. 'L'article est, *en langue*, une forme sans contenu notionnel, la survenance de la matière sémantique ayant lieu tardivement, *en discours* (66). (In a footnote, he points out that the mere utterance of the word *le*, for example, evokes not even a vague conceptual representation of anything.) Further, 'l'article est le signe d'un double mouvement qui porte alternativement la pensée de l'*universel* au *singulier* (article UN) et du *singulier* à l'*universel* (article LE).' A diagram (67) for the 'mécanisme de l'article' shows, at the left in the field of UN, 'tension I' decreasing from U_1 (*universel*) to S_1 (*singulier*) and, at the right in the field of LE, 'tension II' increasing from S_2 to U_2 . The diagram, I take it, is supposed to represent 'mouvements' of thought. Ordinarily, the movement goes from UN to LE, so that if the movement is allowed 'se dérouler sans contrainte jusqu'à son terme', we will get 'LE vrai soldat ne craint pas la fatigue', for example, even when we intend to make a universal statement. But this movement can be somehow interrupted, so that we now get 'UN vrai soldat ...', UN having a 'valeur d'universel' (68).

At the risk of being accused by Valin of a 'profonde incompréhension de l'ouvrage analysé' (13 fn.), I cannot see how one can seriously entertain such propositions. The evidence for these 'mouvements' of thought is purely fictional; we are not told what might 'intercept' them. In any case, the linguistic rules governing the use of the definite and indefinite articles, either in French or in English, can be stated without appeal to 'movements of thought', as can also, I believe, the meaning of any particular use of a definite or an indefinite article. Valin has not shown a way to reveal 'des mécanismes secrets de pensée que l'on trouve à la substructure des langues' (71).

The *Conclusion* (80-7) briefly presents several obvious 'corollaires' of the system developed earlier. Perhaps general semanticists will again be interested in this one: 'les valeurs diverses d'expression qu'un seul et même mot est susceptible de prendre *momentanément* dans le discours ne sont que les *conséquences*, possibles et nécessaires, de *conditions* posées en langue par une certaine représentation, laquelle est précisément le signifié de puissance dont le signe linguistique est le support.' And psychologists who would like to study thought processes by introspection have long recognized the difficulty 'que *penser* est,

pour l'esprit humain, une chose et *saisir* le contenu de cette pensée, une autre chose' (80; all italics in the original). A final corollary, well stated but nothing startling, is this (87): 'Le système général de la langue et les systèmes particuliers qui en sont la teneur doivent être imaginés comme des *commodités de pensée*, empiriquement découvertes au long des millénaires et au hasard des vicissitudes—favorables pour certains groupements humains, défavorables pour d'autres—qui jalonnent l'histoire des civilisations.'

Valin seems to be addressing himself to what I have elsewhere described (*The study of language* 89, 92 f.) as *encoding behavior*, that is, the behavior of the speaker in converting what has been called *intentional behavior* (thoughts, intentions, etc.) into a linguistically coded *message*. Valin has written nearly 90 pages on the fact that such a conversion takes place—which seems fairly obvious. But to the question how it takes place, I cannot see that he has contributed any sound ideas.

Studien über sprachökonomische Entwicklungen im Deutschen. By W. H. A. KOENRAADS. Pp. xxviii, 191. Amsterdam: J. M. Meulenhoff, 1953.

Reviewed by W. F. TWADDELL, *Brown University*

Koenraads discusses in an introduction (xv-xxviii) the 'tendencies toward linguistic economy and linguistic efficiency' as operating in history. A first chapter (1-47) treats the development of modern linguistic research, particularly German, with respect to the account taken of these tendencies: among the more recent names, those of Jespersen and Horn are most important. A second chapter (48-118) is an examination of many linguistic changes between Old High German and modern German as exemplifications of the tendency toward efficiency. A final chapter (119-76) subjects written German of the first half of the twentieth century to close scrutiny.

'Linguistic economy' is a result of simplification, even at the expense of expressive capacity. 'Efficiency' exists when there is equal expressive capacity with less equipment, or more expressive capacity with equal equipment. 'Economy' processes are analogy and systematization, whereby the number of allomorphs is reduced; simplification, whereby the number of 'semantically closely related or allied' morphemes is reduced; conversion, whereby the versatility of morphemes is increased, in that the same morpheme is a member of several form-classes; and shortening, whereby the phonemic 'size' of morphemes or allomorphs is reduced. Put crudely, these processes of 'economy' result in a shorter complete listing of the allomorphs of a lexicon: fewer phonemes per allomorph, fewer allomorphs per morpheme, fewer morphemes per semantic category and form-class. 'Efficiency' factors are those of clarity, distinctiveness, and precision, as balanced against 'economy' processes. It is Koenraads' thesis that many linguistic changes result in an increase of either economy or efficiency without compensating loss in the other, or of both economy and efficiency.

Within the justified limits of his study, Koenraads presents many interesting examples and discussions. His second chapter is a group of sketches of lin-

guistic changes (nonphonemic) from Old High German to modern German, appraised as to their outcome in terms of economy and efficiency. Morphology, word-formation, and syntax are exemplified; the overall history of the paradigms of substantives, adjectives, verbs, numerals, and pronouns illustrates economy and efficiency, in that a smaller total list of allomorphs accommodates the existing compulsory grammatical categories.

The third chapter is full of interesting observations on differences between the written German prose of about 1900 and that of about 1950. Four pairs of passages, similar as to length and topic, are compared with respect to noun and adjective compounds, showing a rather considerable increase in compounding in the later passages. Other tendencies discerned by Koenraads during the half-century are:

- an increase in the inseparable status of *durch, über, um, wider* with verbs;
- strong and weak adjective endings in series of modifiers: *ein schönes Ganze, der Versand frischer holländischen Heringe* (cf. here Goethe's *mit angehaltne stillen Wüten*);
- retreat of the genitive -s (*des Schwarzwald, des März*) and of the connecting -s- of compounds;
- productivity of certain affixes: *bestandpunktet, entmotten, überspielen, umvolken, Vergrößerstädterung, Zertalung; erdhaft, psychologeln, Neusprachler, abermalig, selbstbekennerisch, überdurchschnittlich, Hörschaft, Westlertum*;
- the well-known continued retreat of the genitive case, and some indications of a similar retreat of the dative (replacement with prepositional phrases, often with accusative);
- spread of the preposition *ab*: *vorrätig ab 2 DM, ab fünften September*;
- dependent clauses without anticipatory *darüber, davor, dazu*, etc.;
- appearance of *würde(n)* in the if-clauses of conditions and in other dependent clauses;
- increased use of the uninflected present participle.

Koenraads systematically works with a language structure as a code, as a resource, as a repertory of the devices potentially available to some ideal speaker for purposes of communication throughout the whole community. His citations are thus examples of potential communication resources rather than samples of communicative behavior. His framework has no place for redundancy as a positive component of efficiency. To be sure, the 'efficiency' with which he operates is what he defines it to be; it would be unfortunate to misread Koenraads' 'efficiency' as implying some ideal of 'effectiveness in linguistic communication'.

Nevertheless, the kind of study that Koenraads has undertaken, precisely because of the thoroughness and acuity with which he has carried it out, must raise the question of some measure of 'effectiveness', of the operative suitability of an 'economical, efficient' code to the demands of communication. And that question involves consideration of redundancy as the necessary noise-resistant factor in communicative effectiveness. For instance, a lexicon constructed with full exploitation of a 50-phoneme repertory would be highly

economical as to morpheme length; a morphology constructed without alternant allomorphs and with no restrictions on form-class membership would be economical. These two economies would permit a very large increase in the number of morphemes in the interest of clarity, distinctiveness, and precision in the semantic domain. But such a highly economical and efficient language would have to be spoken under practically perfect acoustic conditions and in a community of extremely intelligent speakers—who would learn to talk some time in their twenties.

The kind of material presented and intuitively categorized by Koenraads will need to be quantified and measured against inclusive theorems drawing upon such studies as those of Shannon and Fano. Beside including redundancy as an ingredient, a measure of suitability or effectiveness must allow for the factors of functional burdening in practical communication. The relative frequency of messages bears upon the 'economy and efficiency' (in Koenraads' sense) of their form: the higher-frequency messages 'ought to be' phonemically shorter and more intensely differentiated. Semantically related elements 'ought to be' phonemically maximally differentiated: *micro-* and *macro-* are a bad pair of English prefixes; the adjectives *causal* and *casual* are graphically bad, as are *explosion* and *expulsion* (in a context of phonetic discussion). Another variable of functional burdening is the social context of common antecedent experiences of speaker and hearer: the technical terms which Koenraads regards as efficient elements are indeed so within a subcommunity of experts; but that context is itself a variable. Some of the items Koenraads finds in contemporary German literary criticism are not likely to be effective as communication outside a small group of practitioners: *Vereinsamung*, *Sprachansicht*, *urbildlich*, *Werthaftigkeit*.

Until we have a synthesis of information theory and the methods of linguistic analysis, we must welcome studies in either line, however partial they are in the light of the total integral phenomenon of communication through language. Koenraads' work is such a study: it presents familiar material in some suggestive new collocations; it is necessarily concerned with details which cannot yet be brought into meaningful relations within a major theoretical framework, but the examples are many and striking, and their treatment testifies to a gift for delicate grammatical analysis.

Die Architektonik des deutschen Wortschatzes. By PAUL MENZERATH. (Phonetische Studien, No. 3.) Pp. [viii], 132, with 14 illustrations, 24 tables, and index. Bonn: Ferd. Dümmlers Verlag, 1954.

Reviewed by W. F. TWADDELL, *Brown University*

This is an examination of a sample of the German vocabulary and its distribution in terms of two variables: the number of syllables and the number of sounds. The sample consists of 20,453 headwords in Viëtor-Meyer, *Deutsches Aussprachewörterbuch* (1921); inflected forms like (*den*) *Himmeln*, (*des*) *Tags*, (*du*) *impfst*, (*ihr*) *wartetet* are disregarded. This exclusion is in the interests of manageability; it is assumed that no serious distortion of relationships in terms

of the two variables is thereby introduced; any qualitative gaps (such as the five-consonant cluster in *impfst*) can be readily filled in with a knowledge of German suffixal phonology.

The examination of this sample reveals certain regularities in the distribution of its items. Taking either syllable number or sound number as the independent variable, the distribution in terms of the other variable is substantially 'normal': its graph is a bell-shaped curve. There is a 'favorite' length in number of sounds for a given syllable length, and vice versa.

Menzerath presents his material in detail, and his tabulations are convenient. A condensed presentation of his data on monosyllables appears in Table 1. (Arabic numerals in the first line and in the left-hand column represent the number of consonants, as counted by Menzerath, before or after the nucleus; V is a short vowel, V: a long vowel, Vv a diphthong.)

	0V	1V	2V	3V	Total	%
V0	0	0	0	0	0	0
V1	11	245	163	15	434	34.3
V2	44	439	195	11	689	54.4
V3	7	96	34	1	138	10.9
V4	2	2	1	0	5	0.4
	64	782	393	27	1266	
%	5.0	61.8	31.0	2.1		
<hr/>						
V:0	7	62	22	5	96	14.7
V:1	20	280	167	8	475	72.7
V:2	8	40	22	0	70	10.7
V:3	3	7	2	0	12	1.8
V:4	1	0	0	0	1	0.1
	39	389	213	13	654	
%	5.9	59.5	32.6	2.0		
<hr/>						
Vv0	6	36	21	4	67	21.9
Vv1	8	113	72	8	201	65.7
Vv2	4	21	9	0	34	11.1
Vv3	1	3	0	0	4	1.3
	19	173	102	12	306	
%	6.2	56.5	33.3	3.9		
<hr/>						
	122	1344	708	52	2226	
%	5.48	60.38	31.81	2.34		

TABLE 1

The table which Menzerath presents as more significant than the breakdown in Table 1 is the simpler one which ignores the distribution of pre-nucleus and post-nucleus differences and presents solely the number of items with *n* sounds and *z* syllables (p. 96). In Table 2, long vowels and short vowels are reckoned as one sound each, diphthongs as two sounds.

s	1	2	3	4	5	6	7	8	9	Total	n
22									3	3	22
21							2	1	1	4	21
20						2	3	3	0	8	20
19						2	3	0	1	6	19
18						5	2	2	1	10	18
17					3	10	11	1		25	17
16				4	14	20	8	4		50	16
15				6	44	29	8			87	15
14			1	43	81	50	4			179	14
13			4	99	154	52	1			310	13
12			28	268	200	31				527	12
11			157	572	201	10				940	11
10		25	566	896	167	3				1657	10
9		91	1125	883	43					2142	9
8		430	1893	643	12					2978	8
7	2	1394	1840	204	1					3441	7
6	69	1603	1105	22						2799	6
5	444	1492	253							2189	5
4	962	1256	7							2225	4
3	645	101								746	3
2	114	4								118	2
1	9									9	1
	2245	6396	6979	3640	920	214	42	11	6	20453	

TABLE 2

Individual chapters are devoted to such topics as the ratio of sounds-per-syllable to the number of syllables; the ratios of long to short vowels before various kinds of post-nuclear sounds or clusters; the overall distribution of individual vowels in the sample; comparisons of Table 2 above with similar tables for English, Italian, Rumanian, Catalan, Portuguese, and Serbo-Croatian.

I can compare one set of Menzerath's statistics, the relative frequency of vowels and diphthongs in monosyllables, with a somewhat similar set prepared as preliminary to the study reported in the 1941 J. A. Walz Festschrift. My figures differ from Menzerath's in that they cover monosyllables and dissyllables, and include inflectional forms; interesting differences follow, and even more interesting similarities. The figures given in Table 3 are percentages, rounded to the nearest whole percent. (The letter symbols are Menzerath's; the totals are figured from the raw data, and are not affected by the rounding off of the individual percentages.) Clearly, my inclusion of inflected dissyllabic forms has boosted the figures for the 'umlaut' vowels—a reflection of the kind of differences to be expected from a different basis of selection. But the totals

	i	æ	a	o	u	ü	ö		Totals
Menzerath	12	9	17	9	10	1	1		57
Twaddell	11	14	14	6	7	4	2		57
	i:	e:	ä:	a:	o:	u:	ü:	ö:	o:
Menzerath	5	4	1	7	5	4	1	1	0
Twaddell	6	4	3	6	4	3	2	2	0
	ei	au	eu						
Menzerath	7	5	1						13
Twaddell	7	4	3						13

TABLE 3

for the three categories short, long, and diphthongal are similar—a reflection of the ironing-out effect.

Quite apart from the interesting specific correlations which Menzerath reports, his study is one more solid piece of evidence that when a lexicon is subjected to serious statistical study, some major correlations of regularities appear, and also some interesting correlations of irregularities. There can be quarrels with the basis of selection (Menzerath is well aware that list-frequency and running-text-frequency are not the same); one may cavil at treating short and long vowels as one sound each and diphthongs as two sounds, despite distributional factors which would point toward grouping together long vowels and diphthongs. Similarly, some might prefer to treat *pf* and/or *ts* as one or two sounds each. Menzerath's consistent operation with the two variables of syllable number and sound number necessarily ignores stress phonemes: it is something of a shock to see *Aspirin*, *Auerhahn*, *daneben* treated (82) as members of 'the same class' 'siebenlautige Dreisilber'. But the important thing is that when SOME procedure of classification is consistently followed, SOME correlations emerge.

What we do not know (or—more modestly and accurately—what I do not know) is the nature of these correlations, as found by Menzerath and others who have done statistical work with lexicons. Are they a characteristic of language, or of human social behavior, or of biological behavior, or simply of the distribution of any large heterogeneous aggregate within its given limits? Some of the specific numerical correlations seem to be roughly constant for many languages; others seem to be characteristic of individual languages. Some of the correlations now known may prove to be secondary—necessary consequences of other correlations. For example, the bell-shaped graphs representing the ranks and rows of Table 1 may reflect not a sovereign distribution of sound-numbers in monosyllables, but a result of two more fundamental distributions: those of prenucleus consonantism and of nucleus-plus-following-consonantism. At any rate, it is interesting that the prenucleus consonantism of Table 1 shows strikingly similar percentage distributions in the three sets:

	0V	1V	2V	3V
before short vowel	5.0	61.8	31.0	2.1
before long vowel	5.9	59.5	32.6	2.0
before diphthong	6.2	56.5	33.3	3.9

Nevertheless, a larger body of data might well show a sovereign correlation between prenucleus and postnucleus clusters. A qualitative correlation emerged from a study of consonants before and after stressed vowels in German (*Acta linguistica* 1.189 ff.); perhaps Menzerath's procedures point toward a quantitative ('number of sounds') correlation.

Over and above the statistical and tabular material of Menzerath's study, the graceful essay on methodology (1-18) is a piece of pleasant and profitable reading.¹

Analyse et enregistrement de la voix parlée et chantée. By DR HENRI MALHIAC.
Pp. 71. Toulouse: Société Générale d'Impression, 1953.

Reviewed by PIERRE DELATTRE, *University of Colorado*

French students of laryngology will find in this book a valuable complement to the manual of J. Tarneaud, *Traité pratique de phonologie et de phoniatrie* (Paris, 1941). Malhiac is a disciple of Calvet, who, like Tarneaud, is an eminent specialist in speech and voice disorders, with emphasis on the latter.

Both Malhiac and Tarneaud describe in detail the use of kymograms (Rousselot, Grammont, and Menzerath) for the study of movement synchrony by the articulators, and the use of oscillograms (Gemelli, Parmenter) for the study of vowel color as well as a more dependable study of pitch and loudness. But Malhiac goes one big step further than Tarneaud. He describes the benefits that can be secured from 'Visible Speech' as developed by the Bell Telephone Laboratories during the second World War. This enables him to define four linguistic notions in terms of a deaf child's capacity for learning from the sight of his own speech patterns on a screen: the notions of loudness, of duration, of pitch, and especially of 'vowel and syllable'. It is worth noting here that Malhiac, speaking of ACOUSTIC patterns, does not use the word *consonant*: the deaf child learns to recognize not consonant patterns but syllable patterns. This implies that there may be no simple acoustic invariants for consonants in the visible patterns of speech; the deaf child's teacher can show him acoustic invariants in the vowel patterns only. The acoustic patterns of a consonant depend so much on the neighboring vowel that they do not show any unity comparable to that which is felt articulatorily. We know that recent experiments in synthesis even indicate that a single acoustic factor can be identified as two different consonants depending on the vowel that follows.

In addition to quite competently describing for the laryngologists the main voice recording techniques from the kymogram to the spectrogram, Malhiac makes a practical contribution of his own. The last part of his book is devoted to the description of a simple means for obtaining a *courbe vocale* that shows the natural range of one's voice as well as the passage notes from chest to head voice. A subject sings all the notes of his acquired range, first as strongly as he can, then as weakly as he can. The strong and the weak intensities for each note are recorded electronically, and a graph is drawn of loudness versus pitch. This graph takes the oval shape of a football that is pinched in the middle. The ends

¹ [Paul Menzerath died on 8 April 1954.—BB]

of the oval seem to furnish the pitch range of the subject's natural voice: the low and the high pitch limits are reached when the stronger tone has the same intensity as the weaker one. The pinch in the middle seems to point to the few notes that lie between the chest voice and the head voice: for those notes the intensity range is comparatively small, especially in an untrained voice.

Exchange of knowledge with the continent of Europe is slow. Malhiac's technical descriptions of 1953 correspond to what was known in the States around 1946, mainly from Dudley's all-important article *The carrier nature of speech*, and Potter's patterns in *Visible speech*. It appears that by 1953 French laryngologists were not yet acquainted with Martin Joos's monograph *Acoustic phonetics*, or with any of the Haskins Laboratories' research in the technique of speech synthesis.

NOTES

MORTIMER GRAVES, EXECUTIVE DIRECTOR OF THE AMERICAN COUNCIL OF LEARNED SOCIETIES, has prepared the following statement for the information of members of the Linguistic Society of America and of the Council's other constituent organizations.

After the first SELECT COMMITTEE OF THE HOUSE OF REPRESENTATIVES TO INVESTIGATE TAX EXEMPT FOUNDATIONS (the Cox Committee) had concluded its operations and reported the discovery of nothing reprehensible in the activities of those foundations with which American scholarship is accustomed to deal, a new SELECT COMMITTEE OF THE HOUSE OF REPRESENTATIVES TO INVESTIGATE TAX EXEMPT FOUNDATIONS was formed under the chairmanship of Representative B. Carroll Reece of Tennessee. The studies made by the investigative staff of this second committee were not confined to the foundations, but considered at much length the 'accessory organizations' supposed to distribute the foundations' money—the SOCIAL SCIENCE RESEARCH COUNCIL, the AMERICAN COUNCIL ON EDUCATION, the NATIONAL EDUCATION ASSOCIATION, the AMERICAN COUNCIL OF LEARNED SOCIETIES, and others.

These staff studies pictured the foundations and the accessory organizations as combined in a tight 'interlock' which not only dominated American scholarship, social science, and education, but by means of this domination had succeeded, in the fifty years since the organization of the first great foundations, in undermining American political stamina, in socializing our educational structure, and in leading the country far down the road to internationalism and collectivism. After receiving these reports and listening to half a dozen anti-foundation witnesses, the Committee suspended public hearings—not, however, before the studies and the testimony had received wide newspaper publicity. The Committee, thereupon, gave the foundations and accessory organizations opportunity to file rebuttal statements.

The materials before the Committee had made attack primarily upon the social sciences, especially upon their 'empiricism', and upon modern education. The humanities, as such, were not similarly vilified, but the AMERICAN COUNCIL OF LEARNED SOCIETIES was completely linked into the web of subversion along with the social scientists and the educators, indeed at one point was given a position of pre-eminence among its sister organizations to which it has never, even in its most enthusiastic moments, aspired.

The rebuttal statements of the SOCIAL SCIENCE RESEARCH COUNCIL and the AMERICAN COUNCIL ON EDUCATION amply demonstrated the fantasy of the charges made against these organizations and their fields of concern. It devolved upon the officers of the AMERICAN COUNCIL OF LEARNED SOCIETIES to speak on behalf of the humanities. Such a statement was accordingly drafted and verified by the Executive Staff and presented by the Chairman, President C. W. de Kiewiet of the University of Rochester.

This five-thousand word document is too long to be fully reproduced here; moreover, much of its content is mere fact well known to the readers of this journal. It went beyond any direct refutation of the charges, allegations, or innuendoes made before the Committee, in that it tried to give the Committee a factual picture of the real organization of scholarship and the social sciences in this country and of the history and activities of the ACLS, in place of the distortions with which the Committee had been confronted. It ended with the following paragraphs:

'The body of this statement has been directed, as was no more than proper, to the assumptions and presuppositions which were implicit in the reports of the Committee staff and some of the "friendly" testimony which the Committee heard. But the Council cannot let this opportunity pass without saying vigorously and directly that it does not share a number of those assumptions and preconceptions.

'It believes that, far from being committed to any particular body of doctrine,

America is a land of boundless experiment, of constant and relentless search for better ways of doing things, for richer experience, to make human life fuller and more attractive. Nothing could be less American than an assumption that Americans had reached the ultimate boundary of thought—political, economic, social, or cultural as well as physical—in 1903 or 1953, or are destined to reach it in 2003.

'A corollary of this interpretation of our tradition is the belief in the maintenance of a completely free market in ideas, no matter how unpalatable they may be to our preconceived notions. The moment we have to protect any mature American from any idea whatsoever, that moment we must stop boasting about American democracy.

'The American Council of Learned Societies is concerned with thought, with ideas, with mankind's concept of itself and its place in nature. It believes that the best interests of America require uncompromising exploration of any thinking that mankind has ever done or is doing. There is no subversion comparable with an interference in the traffic in ideas.

'Ideas are explosive materials. They must not be handled carelessly nor ignorantly. All the activities of the American Council of Learned Societies have been directed at creating and fostering in America the mechanisms through which ideas can be handled understandingly and without fear.

'To this end it has done whatever it could to develop Americans trained to participate fully in the pursuit and communication of all humanistic knowledge and to provide the tools of study, teaching, and research with which such trained Americans have to work.

'The Council is proud of its record in these activities. It holds, moreover, that in the harsh decades ahead, many of our most pressing problems will lie in the very fields of the humanities with which the Council is concerned. In its opinion no work is more important to the future security and welfare of the nation.'

In presenting the statement to the Reece Committee, President de Kiewiet said:

'I cannot forego the opportunity of commenting upon the unwisdom of the aspersions that have been cast upon the scholars and teachers for whom the American Council of Learned Societies is proud to speak. Education is a principal architect of American greatness in all fields, political as well as scientific, cultural as well as technological. We owe our solidarity in an age of crisis to the manner in which we have taught the history and politics of the nation. An attack upon education becomes in part an attack upon American history, an attack, indeed, upon the defensive system of this country. Faith is lessened, courage is diminished, and essential bonds are broken. To lay broad and loose charges against education can itself become a form of subversion against which it is the duty of intellectual leaders to speak forcibly and emphatically.

'I feel grateful to the Reece Committee for at least recognizing that humane studies are powerful forces in any society. Statements apparently made by staff members of the Committee misconstrue the nature of that power and assign to it a baneful influence. The American Council of Learned Societies welcomes the opportunity to reassert its faith in the beneficial power exercised by the thought and studies of the responsible men who make the study of man their lifework. It is an influence that cannot be suppressed. Only those societies try to do so that are fearful of freedom. What we know to be great in our society, our political thought, our humane laws, our sense of human dignity, our powers of self-discovery and self-realization, are all born of the humanist mind. To preserve and extend these is the real function of all those for whom the American Council of Learned Societies elects to speak. Wisdom cannot be bought, and accepts no dictation. Scholarship and learning are the foundation of the nation's wisdom and skill. A society in which scholars and teachers are held in honor is far more likely to produce the wisdom and skill without which it cannot survive in the modern world.'

The statement had a very good press; it was widely quoted, and always with gratifying praise either expressed or implied. While we cannot expect that this attack was the last gasp of a dying American anti-intellectualism, there seems little doubt that both the foundations and the accessory organizations have gained rather than lost stature in the eyes of the intelligent public through this episode. The intellectual must fight back.

The Executive Offices of the ACLS (1219 Sixteenth St., N.W., Washington 6, D. C.) will be happy to send a copy of the statement and the letter of presentation to any reader of this journal who requests it.

THE CANADIAN LINGUISTIC ASSOCIATION was organized in Winnipeg on 27 May 1954; its aim will be to promote the scientific study of language, with particular emphasis on the languages of Canada. Officers for the current year are Henry Alexander, President; Gaston Dulong, Vice-President; M. H. Scargill, Secretary-Treasurer and Editor; Jaroslav B. Rudnyčkyj and Walter S. Avis, Associate Editors. Membership is open to all, in the United States as well as in Canada; the dues for 1955 are \$2. Members will receive two copies of the *Journal of the Canadian Linguistic Association*. Interested persons should write to the Secretary-Treasurer (Room 202, Arts Building, University of Alberta, Edmonton, Alta.). Manuscripts for publication in the *Journal*, in French or English, may be sent to the same address; they should be limited to about 2000 words.

THE CHICAGO LINGUISTIC SOCIETY, an inter-university group of faculty and students at the University of Chicago, Northwestern University, and other institutions in the Chicago area, continues the tradition of a similar group which existed before the last war. The present organization was established in February 1951; since then, regular meetings have been held monthly through the school year, with one or two linguistic papers read and discussed at each meeting. The organization of the CLS is informal; there are no dues. The necessary business, including the distribution of notices, is carried on by two 'consuls' representing the two chief institutions. The current consuls are Katharine T. Loesch of Northwestern University and Erica Reiner of the University of Chicago.

LA SOCIÉTÉ INTERNATIONALE DE LINGUISTIQUE ROMANE was revived in August 1953, with the following officers: Président, M. Roques; Vice-Présidents, J. Orr and W. von Wartburg; Secrétaire-Administrateur, A. Terracher; Secrétaire-Administrateur adjoint, P. Gardette; Secrétaire-Trésorier, C. Bruneau; Secrétaire-Trésorier adjoint, G. Straka. The journal of the Society, *Revue de linguistique romane*, is to be published by Éditions IAC de Lyon; two issues are planned for 1954. Subscriptions, at 1500 francs, may be sent to M. G. Straka, Université de Strasbourg, France.

A NEW JOURNAL IN OUR FIELD, entitled *General linguistics*, has been established by the Department of Modern Foreign Languages at the University of Kentucky, Lexington, with John A. Rea as editor, and John L. Cutler and George P. Faust as associate editors. The winter number each year will be devoted to papers presented in the linguistic section of the preceding University of Kentucky Foreign Language Conference. Brief articles and notes, as well as

longer articles, are invited from all readers. Subscriptions, at \$2 a year, may be sent to the editor.

THE NORTHEAST CONFERENCE ON THE TEACHING OF FOREIGN LANGUAGES, dedicated to the improvement of teaching methods for modern and classical languages and to their successful maintenance in the curriculum at all levels, will hold its next meeting on 1 and 2 April 1955 at the Hotel New Yorker, New York. The host institution will be New York University; earlier hosts have been Yale University and Brown University. The growing importance of the Conference is attested by the fact that it is now supported in large part by sixty institutions in New England and the Middle Atlantic States, and that in the year 1954 it was attended by 800 foreign language teachers. Further information may be secured from the Conference Chairman for 1955, Professor Germaine Brée (Room 735, East Building, New York University, Washington Square, New York 3, N. Y.).

TWO MEMBERS OF THE LINGUISTIC SOCIETY have recently been honored by European organizations of scholarship. ALBERT MOREY STURTEVANT, Professor Emeritus of Germanic Languages and Literatures in the University of Kansas, has been elected an Honorary Member of the Icelandic Literary Society. ALBRECHT GOETZE, Laffan Professor of Assyriology and Babylonian Literature in Yale University, has been elected a Corresponding Member of the Académie des Inscriptions et Belles Lettres, Institut de France.

EINAR HAUGEN OF THE UNIVERSITY OF WISCONSIN asks that the following notice be published, to call the attention of linguists to the republication of an important work.

Scholars interested in Old Icelandic will be glad to hear that the most comprehensive dictionary of the language so far produced, Johan Fritzner's *Ordbog over det gamle norske sprog* (3 volumes, 1886-96), is again available. A complete photostatic copy, bound in buckram, is scheduled for publication in October 1954. A supplementary volume of corrections and additions will follow in mid-1955, edited by Didrik Arup Seip and Trygve Knudsen. The price to pre-publication subscribers is \$30 postpaid; the supplement alone can be bought for \$3.25. After publication the price will be \$4.24 for the supplement, \$43.50 for all four volumes. Orders should be sent to Tryggve Juul Møller, Forlag, Fr. Nansens Plass 8, Oslo, Norway.

ISIDORE DYEN OF YALE UNIVERSITY reports that in his recently published monograph, *The Proto-Malayo-Polynesian laryngeals*, footnote 1 (on page 50) contains a number of serious errors. The entire footnote should be deleted, and replaced by the following:

1. For a list of the PMP phonemes, see Dempw. 3.9 (with d' unintentionally omitted). To this list another proto-phoneme is to be added; see Dyen 4. I find it convenient to use a set of symbols which is in part different from Dempwolff's and I quote his reconstructions, when that is necessary, in my symbolism. In the following list of symbols that differ, my symbol precedes a colon and his equivalent follows: c : k', D : d, e : a, h : ', j : g', ñ : ñ, N : ŋ, q : h, r : l, R : r, s : t', T : t, w : v, y : j, z : d'.

PUBLICATIONS RECEIVED

This listing acknowledges the receipt of recent works that appear to bear on the scientific study of language. No book can be returned to the publisher, nor can the Editor promise that every book received will be reviewed in this journal. Reviews are published as circumstances permit, and copies are sent to the publishers of the works reviewed.

- Acta linguistica Academiae Scientiarum Hungaricae* 3.211-474 (1953).
Aegyptus 34.1-192 (1954).
Aesthetics and language: Essays. Ed. with an introduction by William Elton. Pp. vi, 186. New York: Philosophical Library, 1954.
Afrika und Übersee 38.49-140 (1954).
The alliteration of Old Saxon poetry. By W. P. Lehmann. (Norsk tidsskrift for sprogvidenskap, Supplement 3.) Pp. 231, with folding chart. Oslo: H. Aschehoug & Co. (W. Nygaard), 1953.
The alliterations of the Edda. By W. P. Lehmann and J. L. Dillard. Pp. 168 lithoprinted. Austin: Department of Germanic Languages, University of Texas, 1954.
Altfranzösisch go — neufranzösisch ga: Eine syntaktische Betrachtung. By Ernst Zellmer. Pp. 23, lithoprinted. Frankfurt a.M., 1954.
Altkirchenslavische Grammatik: Schrift-, Laut- und Formensystem. By Nikolaus S. Trubetzkoy, ed. by Rudolf Jagoditsch. (Österreichische Akademie der Wissenschaften, Philosophisch-historische Klasse: Sitzungsberichte, 228. Band, 4. Abhandlung, vorgelegt in der Sitzung am 7. Dezember 1949 und 30. Jänner 1952; gedruckt mit Unterstützung des Vereins der Freunde der Österreichischen Akademie der Wissenschaften.) Pp. 197. Wien: Rudolf M. Rohrer, 1954.
Alt- und mittelenglische Anthologie. Ed. by Rolf Kaiser. Pp. 474, with 8 plates. Berlin, 1954.
Annales Academiae Scientiarum Fennicae, Ser. B, Tom. 84: Emil Öhmann zu seinem 60. Geburtstag am 25. Januar 1954 von Freunden und Fachgenossen. Pp. 672. Helsinki: Suomalainen Tiedekatemia, 1954.
Applied linguistics in language learning. Ed. by Ernst Pulgram. (Institute of Languages and Linguistics, Monograph series on languages and linguistics, No. 6.) Pp. viii, 85. Washington: Georgetown University Press, 1954.
Die Architektonik des deutschen Wortschatzes. By Paul Menzerath. (Phonetische Studien, No. 3.) Pp. [viii], 132. Bonn: Ferd. Dümmlers Verlag, 1954.
Archiv orientální 21.1-480 (1953).
Archivum linguisticum 5.1-132 (1953).
Area and culture studies, Nos. 1-3 (1951-53). Tokyo University of Foreign Studies.
Aspects du vocabulaire latin. By Alfred Ernout. (Études et commentaires, Vol. 18; Ouvrage publié avec le concours du Centre National de la Recherche Scientifique.) Pp. 238. Paris: Librairie C. Klincksieck, 1954.
Aspects of language. By William J. Entwistle. Pp. xi, 370. London: Faber and Faber, 1953.
Audio-visual aids in language teaching, with critical bibliography. By Ruth Hirsch. (Institute of Languages and Linguistics, Monograph series on languages and linguistics, No. 5.) Pp. vi, 89. Washington: Georgetown University Press, 1954.
Bahasa dan budaja 1:6 (1953), 2:1-4 (1953-54).
Bibliography of Scottish linguistic studies. By J[ohn] S. Wooley. Pp. 37. Edinburgh: James Thin, for the University of Edinburgh Linguistic Survey of Scotland, 1954.
Biblos 29.1-563 (1953).
Boletim de filologia 13.185-383 (1952).
Boletín de filología 6.1-276 (1950) = Nos. 43-45. Montevideo, Uruguay.
Boletín de filología 7.1-444 (1952-53). Santiago de Chile.

- Boletín indigenista venezolano 1.1-166 (1953). Publicación trimestral: Órgano de la Comisión Indigenista, Ministerio de Justicia; Caracas, República de Venezuela.
- Bulletin analytique: Philosophie 7.711-998, i-vi, 1-32, 1-67 (1953).
- Bulletin of the Chinese Association for the Advancement of Science 1:5 (1953). Taipei, Taiwan.
- Bulletin of the School of Oriental and African Studies 16.1-424 (1954). University of London.
- Les caractères statistiques du vocabulaire: Essai de méthodologie. By Pierre Guiraud. Pp. [v], 116. Paris: Presses Universitaires de France, 1954.
- Christopher Cooper's English teacher (1687). Ed. by Bertil Sundby. (Lunds Universitets Årsskrift, NF Avd. 1, Bd. 50, Nr. 5; Lund studies in English, Vol. 22.) Pp. cxvi, 10*, [122]. Lund: C. W. K. Gleerup; Copenhagen: Ejnar Munksgaard, 1953.
- The classical weekly 47.81-208 (1954).
- Coisas e palavras: Alguns problemas etnográficos e lingüísticos relacionados com os primitivos sistemas de debulha na península ibérica. By José Goncalo C. Herculano de Carvalho. (Separata de 'Biblos', Vol. 29; diss. Coimbra.) Pp. xii, 413, with 13 folding maps. Coimbra, 1953.
- The concessive relation in Old English poetry. By Randolph Quirk. (Yale studies in English, Vol. 124.) Pp. xiv, 150. New Haven: Yale University Press, 1954.
- Cultura neolatina 13.1-120 (1953).
- De proprietate sermonum vel rerum: A study and critical edition of a set of verbal distinctions. By Myra L. Uhlfelder. (Papers and monographs of the American Academy in Rome, Vol. 15.) Pp. [vii], 116. Rome: American Academy in Rome, 1954.
- Descriptive studies in Spanish grammar. Ed. by Henry R. Kahane and Angelina Pietrangeli. (Illinois studies in language and literature, Vol. 38.) Pp. xiii, 241. Urbana: University of Illinois Press, 1954.
- A dictionary of linguistics. By Mario A. Pei and Frank Gaynor. (Mid-century reference library.) Pp. [viii], 238. New York: Philosophical Library, 1954.
- English topographic terms in Florida 1563-1874. By E. Wallace McMullen Jr. Pp. [ix], 227. Gainesville: University of Florida Press, 1953.
- Entzifferung verschollener Schriften und Sprachen. By Johannes Friedrich. (Verständliche Wissenschaft, Vol. 51.) Pp. vi, 147. Berlin, Göttingen, Heidelberg: Springer-Verlag, 1954.
- Estructura del sistema de aspectos y tiempos del verbo griego antiguo: Análisis funcional sincrónico. By Martín Sánchez Ruipérez. (Theses et studia philologica salmanticensia, Vol. 7.) Pp. xi, 179. Salamanca: Consejo Superior de Investigaciones Científicas, Colegio Trilingüe de la Universidad, 1954.
- Etudes germaniques 9.1-256 (1954).
- Eusko-Jakintza 6.1-215 (1952).
- An exposition of Qui habitat and Bonum est in English: Edited from the manuscripts with introduction, notes, and glossary. By Björn Wallner. (Lund studies in English, Vol. 23.) Pp. lxxii, 121, with 1 plate. Lund: C. W. K. Gleerup; Copenhagen: Ejnar Munksgaard, 1954.
- Felna, Nos. 15, 16 (1952) = Igl talaccabagn: Rätische gesprächsproben mit deutscher übersetzung und phonetischer umschreibung versehen, by Gangale; pp. 28. Copenhagen: Felna Publishers.
- Les formes surcomposées en français. By Maurice Cornu. (Romanica helvetica, Vol. 42.) Pp. xi, 268. Bern: A. Francke AG. Verlag, 1953.
- El Fuero de Teruel. Ed. by Max Gorosch. (Leges hispanicae Medii Aevii, Vol. 1.) Pp. 667. Stockholm (Uppsala: Almqvist & Wiksells Boktryckeri), 1950.
- General American pronunciation: A phonetic reader for Japanese students. By Robert H. Gerhard. Pp. [xiii], 226. Tokyo: Sanseido, 1954.
- Gengo kenkyu, No. 24 (1953).
- Die Gliederung des indogermanischen Sprachgebiets. By Walter Porzig. (Indogermanische Bibliothek, 3. Reihe: Untersuchungen.) Pp. 251. Heidelberg: Carl Winter, Universitätsverlag, 1954.

- A grammar of Manichean Sogdian. By Ilya Gershevitch. (Publications of the Philological Society, Vol. 16.) Pp. xiv, 308. Oxford: Basil Blackwell, 1954.
- Die griechischen Lehnwörter in den Prosaschriften Ciceros. By Päivö Oksala. (Annales Academiae Scientiarum Fennicae B.80.1.) Pp. 176, 3. Helsinki: [Suomalainen Tiedekatemia], 1953.
- Guagninus' Toponymy of 1611 [in Ukrainian]. By O. Kupranec'. (Onomastica, No. 8.) Pp. 32. Winnipeg: Ukrainian Free Academy of Sciences, 1954.
- Hanunóo-English vocabulary. By Harold C. Conklin. (University of California publications in linguistics, Vol. 9, pp. 1-290.) Pp. [iii], 290. Berkeley and Los Angeles: University of California Press, 1953.
- Harvard journal of Asiatic studies 16.293-497 (1953).
- Die Heimat der indogermanischen Gemeinsprache. By Paul Thieme. (Akademie der Wissenschaften und der Literatur in Mainz: Abhandlungen der geistes- und sozialwissenschaftlichen Klasse, Jahrgang 1953, Nr. 11.) Pp. 79. Wiesbaden: Franz Steiner Verlag, 1954.
- Hespérís 40.1-306 (1953).
- Hethitisches Wörterbuch: Kurzgefasste kritische Sammlung der Deutungen hethitischer Wörter. By Johannes Friedrich. (Indogermanische Bibliothek, 2. Reihe.) Fasc. 4, pp. 289-344. Heidelberg: Carl Winter, Universitätsverlag, 1954.
- Hiatus in English: Problems of catenation and juncture. By Aasta Stene. (Anglistica, Vol. 3.) Pp. 102, with folding chart. Copenhagen: Sodenkilde and Bagger, 1954.
- An historical dictionary of German figurative usage. By Keith Spalding, with the assistance of Kenneth Brooke. Fasc. 4, pp. 137-84. Oxford: Basil Blackwell, 1954.
- Historische Grammatik der italienischen Sprache und ihrer Mundarten. By Gerhard Rohlfs. (Bibliotheca romanica, Series prima: Manualia et commentationes, Vols. 5, 6, 7.) Vol. 1: Lautlehre, pp. 548; 2: Formenlehre und Syntax, pp. 586; 3: Syntax und Wortbildung mit dem Register zu den Bänden I, II und III, pp. 434. Bern: A. Francke AG. Verlag, 1949, 1949, 1954.
- Humanitas 1:2.1-558 (1953).
- Der indefinite Agens im Mittelenglischen (1050-1350): Die Wörter und Wendungen für 'man'. By Hans Heinrich Meier. (Schweizer anglistische Arbeiten, Vol. 34.) Pp. 256. Bern: A. Francke AG. Verlag, [1953].
- Indogermanisches etymologisches Wörterbuch. By Julius Pokorny. Fascicle 8, *lek- to neus-*, pp. 672-768. Bern: A. Francke AG. Verlag, 1954.
- Inkuti pukunot oo lmaasai. By John Tompo Ole Mpaayei, ed. by A. N. Tucker. (Annotated African texts, 3. Maasai.) Pp. xi, 74. London and Cape Town: Geoffrey Cumberledge, Oxford University Press (for the School of Oriental and African Studies, University of London), 1954.
- Internationale Zeitschriftenschau für Bibelwissenschaft und Grenzgebiete 1:2.1-228 (1951/52).
- International journal of American linguistics 20.83-257 (1954).
- Isländisches etymologisches Wörterbuch. By Alexander Jóhannesson. Fasc. 4-5, pp. 481-800, germ. *tand-* to *syep*. Bern: A. Francke AG. Verlag, 1954.
- Italica 31.1-136 (1954).
- Izvestija Akademii Nauk SSSR: Otdelenie literatury i jazyka 12.297-400 (1953).
- Jazykovedný časopis 7.1-255 (1953). Bratislava: Slovenská Akadémia Vied.
- Jornal de filologia 2.113-216 (1954).
- Journal de la Société des Américanistes NS 42.1-536 (1953).
- Journal of the Burma Research Society 36:2.1-91 (1953).
- Journal of the Oriental Institute 3.111-314 (1953); Baroda.
- The journal of the Polynesian Society 62.1-292, 73-89, Memoir 29.147-62 (1953).
- Kazania Gnieźnienskie: Podobizna, transliteracja, transkrypcja. By Stefan Vrtel-Wierczyński. (Zabytki języka i literatury polskiej, No. 2.) Pp. xxiv, 167, plates I-XLIX. Poznań: Nakładem Poznańskiego Towarzystwa Przyjaciół Nauk, 1953.
- Kurzgefasstes etymologisches Wörterbuch des Altindischen (A concise etymological

- Sanskrit dictionary). By Manfred Mayrhofer. (Indogermanische Bibliothek, 2. Reihe: Wörterbücher.) Fasc. 1, *a-* to *arālaḥ*, pp. 1-48, i-xxxv; fasc. 2, *arālaḥ* to *Emūṣāḥ*, pp. 49-128. Heidelberg: Carl Winter, Universitätsverlag, 1953, 1954.
- Lateinisches etymologisches Wörterbuch. By Alois Walde, 3rd ed. by J. B. Hofmann. (Indogermanische Bibliothek, 2. Reihe: Wörterbücher.) Vol. 2, Fascicle 21 [last], *vēnus* to *zōnātim*, pp. 753-851. Heidelberg: Carl Winter, Universitätsverlag, 1954.
- The Latin language. By L. R. Palmer. (The great languages.) Pp. ix, 372. London: Faber and Faber Ltd, 1954.
- Leuvense bijdragen 43.53-119, Bijblad 1-146 (1953).
- Levende talen, Nos. 173, 174, 175 (1954).
- Lexikografický sborník: Materiály z I. celoštátnej konferencie čs. lexikografov, konanej v dňoch 5.-7. júna 1952 v Bratislave. [Ed. by Štefan Peciar.] Pp. 215. Bratislava: Vydavateľstvo Slovenskej Akadémie vied, 1953.
- Lexis 3.169-328 (1953).
- Lingua nostra 15.1-32 (1954).
- A linguistic study of the *Shih ming*: Initials and consonant clusters. By Nicholas Cleaveland Bodman. (Harvard-Yenching Institute studies, Vol. 11.) Pp. xi, 146. Cambridge, Mass.: Harvard University Press, 1954.
- A Luganda grammar. By E. O. Ashton, E. M. K. Mulira, E. G. M. Ndawula, A. N. Tucker. Pp. ix, 516. London, New York, Toronto: Longmans, Green and Co., 1954.
- Lulelappsk ordbok (Lulelappisches Wörterbuch). By Harald Grundström. (Skrifter utgivna genom Landsmåls- och Folkminnesarkivet i Uppsala, Ser. C.1.) Fasc. 12.2: Grammatische Übersicht über die lulelappische Dialekte, 2. Konjugation, pp. 1845-1920. Uppsala: A.-B. Lundequistska Bokhandeln; København: Einar Munksgaard, [1954].
- Le maître phonétique III.32.1-16; [Supplement]: Miscellanea phonetica II, pp. 59 (1954).
- Man 53.145-96 (1953); 54.1-36 (1954).
- Mélanges de l'Université Saint Joseph 30.1-310 (1953).
- Method and perspective in anthropology: Papers in honor of Wilson D. Wallis. Ed. by Robert F. Spencer. Pp. xii, 323. Minneapolis: University of Minnesota Press, 1954.
- Middle English dictionary. Ed. by Hans Kurath and Sherman M. Kuhn. Parts E.2-3, F.1-2, pp. 121-628. Ann Arbor: University of Michigan Press, 1953-54.
- Die Mimesis in der Antike: Nachahmung, Darstellung, Ausdruck. By H. Koller. (Dissertationes Bernenses historiam orbis antiqui nascentisque medii aevi elucubrantes, Ser. 1, Fasc. 5.) Pp. 235. Bern: A. Francke, 1954.
- Norsk folkemål: Grunnskrifter og innlegg gjennom hundre år, til Halvdan Koht på åttiårsdagen 7. juli 1953. Ed. by Bjarte Birkeland and Reidar Djupedal. Pp. 300. Oslo: Det Norske Samlaget, [1953].
- Nueva revista de filología hispánica 7.337-708 (1953) = Homenaje a Amado Alonso, Vol. 2; 8.1-240 (1954).
- Onoma 3.1-196, Bibliography 1*-80* (1952, issued 1954). Bibliographical and information bulletin, International Committee of Onomastic Sciences; Centre International d'Onomastique, Louvain (Belgique).
- Orbis 3.1-381 (1954).
- Orientalia NS 23.1-336, 1*-80* plates I-XXXII (1954).
- Orientalia suecana 2.37-153 (1953); 3.1-39 (1954).
- Orientalistische Literaturzeitung 49.1-288 (1954).
- O vzájemném vlivu tvaru a významu slov [Influence réciproque de la forme et du sens des mots]. By Otto Ducháček. (Česká Akademie Věd a Umění.) Pp. 192. Praha: Státní Pedagogické Nakladatelství, 1953.
- The place names of Franklin County, Missouri. By Robert L. Ramsay. (The University of Missouri studies, Vol. 26, No. 3.) Pp. 55. Columbia, Mo.: University of Missouri Studies, 1954.
- Power of words. By Stuart Chase, in collaboration with Marian Tyler Chase. Pp. xii, 308. New York: Harcourt, Brace and Company, 1953, 1954.
- Pyrenäenwörter vorromanischen Ursprungs und das vorromanische Substrat der Alpen.

- By Johannes Hubschmid. (*Acta salmanticensia*, Serie de filosofía y letras, Vol. 7, No. 2.) Pp. 81. Universidad de Salamanca, 1954.
- The relationship of the Semitic and Egyptian verb systems. By T. W. Thacker. Pp. xxvi, 341. Oxford: Clarendon Press, 1954.
- Renaissance dictionaries: English-Latin and Latin-English. By DeWitt T. Starnes. Pp. xii, 428. Austin: University of Texas Press, 1954.
- Revista colombiana de antropología 2.1-303 (1954).
- Revue des langues romanes 71.247-397 (1953-54).
- Sborník prací filosofické fakulty brněnské university 2.1-356, with 20 plates (1953) = Zdeňku Nejedlému k 75. narozeninám.
- The Scottish national dictionary. [Ed. by David D. Murison.] Vol. 4, Part 2, pp. 113-240. Edinburgh: Scottish National Dictionary Association, 1954.
- Semítica 1.1-87 (1948); 2.1-75 (1949); 3.1-85 (1950); 4.1-95 (1951-52). Cahiers publiés par l'Institut d'Études sémitiques de l'Université de Paris; Librairie d'Amérique et d'Orient, Adrien-Maisonneuve.
- Die slavischen Farbenbenennungen: Eine semasiologisch-etymologische Untersuchung. By Gunnar Herne. (Publications de l'Institut slave d'Upsal, No. 9.) Pp. 147. Uppsala: Almqvist & Wiksells Boktryckeri AB, 1954.
- Slovo 3.1-124 (1953).
- Songs and stories of the Ch'uan Miao. By David Crockett Graham. (Smithsonian miscellaneous collections, Vol. 123, No. 1.) Pp. xii, 336, with 24 plates. Washington: Smithsonian Institution, 1954.
- The Sophists. By Mario Untersteiner, transl. from the Italian by Kathleen Freeman. Pp. xvi, 368. New York: Philosophical Library, 1954.
- The southern Bantu languages. By C. M. Doke. (Handbook of African languages.) Pp. 262, with folding map. London, New York, Capetown: Oxford University Press, for International African Institute, 1954.
- Speculum 29.1-182 (1954).
- Stand und Aufgabe der allgemeinen Sprachwissenschaft. By Ernst Otto. Pp. viii, 183. Berlin: Walter de Gruyter & Co., 1954.
- Structure drill in Cantonese: First fifty patterns. By K. P. K. Whitaker. (Structure drills through speech patterns, Vol. 4.) Pp. xxix, 101. London: Lund Humphries & Co. Ltd., 1954.
- Studien zum bestimmten Artikel in den germanischen Sprachen. By Heinrich Matthias Heinrichs. (Beiträge zur deutschen Philologie, Vol. 1.) Pp. 143. Giessen: Wilhelm Schmitz Verlag, 1954.
- Studies in English demonstrative pronouns: A contribution to the history of English morphology. By Trygve Heltveit. Pp. 138. Oslo: Akademisk Forlag, 1953.
- Subject and predicate: A contribution to the theory of syntax. By Manfred Sandmann. (Edinburgh University publications: Language and literature, No. 5.) Pp. xiv, 270. Edinburgh: University Press, 1954.
- Syntaxe grecque; 2nd edition, revised and enlarged. By Jean Humbert. (Collection de philologie classique, Vol. 2.) Pp. 464. Paris: Librairie C. Klincksieck, 1954.
- Das Tempussystem des Deutschen und des Französischen: Übersetzungs- und Strukturprobleme. By Hans Weber. (Romanica helvetica, Vol. 45.) Pp. 275. Bern: A. Francke AG. Verlag, 1954.
- The theory of proper names: A controversial essay; 2nd ed. By Sir Alan Gardiner. Pp. viii, 77. London, New York, Toronto: Geoffrey Cumberlege, Oxford University Press, 1954.
- Tijdschrift voor nederlandse taal- en letterkunde 72.1-80 (1954).
- Ukrajins'ka mova ta jiji stanovyšče sereď inšyx slov'jans'kyx mov. By P. Kovaliv. (Slavistica, No. 20.) Pp. 48. Winnipeg: M. Shashkevych Society, 1954.
- Védico y sánscrito clásico: Gramática, textos anotados y vocabulario etimológico. By Francisco Rodríguez Adrados. (Manual de lingüística indoeuropea, No. 2.) Pp. 209. Madrid: Consejo Superior de Investigaciones Científicas, Instituto Antonio de Nebrija, 1953.

Virittäjä 58.1-208 (1954).

Vocabolario dei dialetti della Svizzera italiana. By Silvio Sganzini. Fasc. 2, pp. 41-88. Lugano, 1954.

Vsevolod Hancov, Olena Kurilo [in Ukrainian]. By G. Y. Šerech. (Ukrainian Free Academy of Sciences, Series: Ukrainian scholars, No. 5.) Pp. 79. Winnipeg: Ukrainian People's Home in Toronto, 1954.

Werkwoordsvorme in Afrikaans in die verlede tyd. By Meyer de Villiers. (Annale van die Universiteit van Stellenbosch, Jaargang 26, Reeks B, No. 1.) Pp. 92. [Stellenbosch, South Africa], 1951.

The words we use. By J. A. Sheard. (The language library.) Pp. 344. New York: Frederick A. Praeger, 1954.

Zum schleswiger Niederdeutsch: Kritik und Forschung. By Peter Jørgensen. (Det Kongelige Danske Videnskabernes Selskab, Historisk-filologiske Meddelelser, Vol. 34, No. 2.) Pp. 151, with folding map. København: Ejnar Munksgaard, 1954.

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